The Impact of Local Labor Market Conditions on Opioid Transactions: Evidence from the COVID-19 Pandemic

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Executive Summary

The opioid crisis in the United States is an ongoing public health emergency. In 2019, 49,900 people died from opioid overdoses (Mattson et al. 2021). Preliminary estimates suggest elevated opioid overdose deaths in the months following the onset of the COVID-19 pandemic. The Centers for Disease Control and Prevention (CDC) reported 69,700 opioid overdose deaths in the 12-month period ending in December 2020, an increase of 40 percent from 2019 (Baumgartner and Radley 2021).

A better understanding of how changing labor market conditions associated with the pandemic affected opioid use can inform policies designed to alleviate the harms of future epidemics and pandemics that lead to widespread closures of businesses, schools, and government entities. However, despite the preliminary evidence of increased opioid use during the COVID-19 pandemic, it is not clear whether changes in labor market conditions indeed led to an increase in opioid use. Ongoing trends in opioid use and other factors potentially affected by the pandemic, such as access to physicians and elective medical procedures, might be masking any direct effects of labor market conditions. The shock of the pandemic, combined with geographic variation in the vulnerability to job loss due to the pandemic, provides an opportunity to directly estimate the effects of changes in local labor market conditions on opioid use.

This study uses county-level data on employment and opioid transactions from the first quarter of 2018 to the fourth quarter of 2020. To tease out the causal relationship between the changes in labor market conditions associated with the pandemic and opioid use, we implemented a quasi-experimental regression design that takes advantage of (1) the unexpected nature of the pandemic, and (2) cross-county variation in industry composition, which leads to county-level variation in vulnerability to the effects of the pandemic on the labor market. We first constructed a measure of county-level vulnerability to job loss because of the pandemic. We then tested whether counties with larger shares of jobs in the most vulnerable industries had a larger change in opioid transactions after the pandemic relative to counties with lower shares of jobs in those industries. We conducted an analysis of all counties and an analysis by region because data from the Quarterly Census of Employment and Wages (QCEW) suggest the pandemic affected employment in different parts of the country differently over time.

Opioid transactions

We use this term in the report to be consistent with how the data we analyzed were collected. Specifically, we analyzed data from the Drug Enforcement Administration's Automated Reports and Consolidated Ordering System (ARCOS). ARCOS collects reports on legal transactions of controlled substances, including opioids, from drug manufacturers and distributers to hospitals, retail pharmacies, and medical providers.

Our findings suggest that, overall, reductions in employment due to the pandemic directly led to an increase in legal opioid transactions. Although legal opioid transactions decreased during the COVID-19 pandemic

Main study findings

- The COVID-19 pandemic was a substantial shock to employment.
- The *observed* relationship between the employment rate and legal opioid transactions is positive.
- Counties varied widely in their vulnerability to job loss due to the pandemic.
- Worsening labor market conditions led to an *increase* in opioid transactions relative to what they would have been in the absence of the pandemic's shock, though the magnitude of this impact varied by geographic region

overall, they decreased at a slower rate than before the onset of the pandemic. Using our estimation strategy to isolate the relationship between employment changes and opioid transactions, we find that worsening labor market conditions associated with the pandemic led to an increase in opioid transactions relative to what they would have been in the absence of this shock. This increase is especially notable in the Northeast and West regions.

Our findings have implications for early stages of future epidemics and pandemics that lead to widespread closures of businesses, schools, and government entities. They indicate that locations more affected by a contraction in employment might experience a sharper increase in opioid transactions even if those areas are less affected by high caseloads of the disease, thus potentially increasing health emergencies related to opioid misuse and putting stress on emergency medical providers.

I. Introduction and Background

The opioid crisis in the United States is a public health emergency. In 2019, 49,900 people died from opioid overdoses (Mattson et al. 2021). Death rates involving synthetic opioids, which consist mostly of illicitly manufactured fentanyl—alone or combined with other drugs, including prescription opioids—exploded in recent years, from 1.0 death per 100,000 people in 2013 to 11.4 deaths per 100,000 people in 2019.[1] Despite a decline in opioid prescriptions from a peak of 81.3 dispensed per 100 persons in 2012, prescription rates remain high, with 46.7 prescriptions dispensed per 100 persons in the United States in 2019; the opioid prescription rate remains above 70 prescriptions dispensed per 100 persons in Alabama (85.8), Arkansas (80.9), Louisiana (74.6), Tennessee (74.6), and Kentucky (72.3) (CDC 2021).

Preliminary evidence suggests elevated drug overdose rates in the months following the onset of the COVID-19 pandemic in the United States (Haley and Saitz 2020). For example, the CDC reported 69,700 drug overdose deaths in the 12-month period ending in December 2020, a 40 percent increase from 2019 (Baumgartner and Radley 2021). Emergency medical services related to opioid overdoses also increased early in the pandemic (Slavova et al. 2020; Ochalek et al. 2020; Glober et al. 2020). Preliminary evidence showed an increase in the use of cocaine, fentanyl, heroin, and methamphetamine during this period (Wainwright et al. 2020). Individuals with opioid use disorders are also at a higher risk of increased substance use during the pandemic, primarily because of obstacles to treatment (Mallet et al. 2021).

We hypothesize that the COVID-19 pandemic may have impacted opioid use among workers in the United States through multiple channels, including employment-related ones. We also hypothesize that the expected overall effect of the pandemic on opioid use is uncertain because some factors associated with the pandemic may lead to increases in opioid use while others may lead to reductions. On one hand, the spike in unemployment due to the pandemic might have led to an increase in opioid use if factors such as job loss and reduced earnings impacted workers' physical and mental health, causing increased use among existing users and an increase in new users (Ahammer and Packham 2020; Carpenter et al. 2017; Krueger 2017). On the other hand, having less income available to purchase legal or illicit drugs, and a reduction in work injuries because of the economic contraction, might have decreased opioid use (Hollingsworth at al. 2017; Savych et al. 2019). Loss of health insurance through employer-sponsored plans might have also affected opioid use by decreasing access to prescription opioids (Hollingsworth et al. 2017), though take-up of Medicaid during the downturn might have mitigated this effect (Bundorf et al. 2021). Other factors not related directly to employment may have affected opioid use as well. For example, pandemic-related restrictions might have limited access to prescribing physicians and elective medical procedures that often lead to opioid prescriptions (Mehrotra et al. 2020; Ziedan et al. 2020).

A better understanding of how changing labor market conditions associated with the pandemic affected opioid transactions can inform policies designed to alleviate the harms of future epidemics or pandemics that lead to widespread closures of businesses, schools, and government entities. However, despite the preliminary evidence of increased opioid use during the COVID-19 pandemic, it is not clear whether changes in labor market conditions indeed led to an increase in opioid transactions. Ongoing trends in opioid use and other factors mentioned above such as reduced access to physicians and elective medical procedures might be masking any direct effects of labor market conditions on opioid use. The shock of the pandemic, combined with geographic variation in the vulnerability to job loss due to the pandemic,

[1] Mattson et al. (2021) calculated age-adjusted death rates by applying age-specific death rates to the 2000 U.S. Census standard population age distribution.

provides an opportunity to directly estimate the effects of changes in local labor market conditions on opioid use.

The U.S. Department of Labor's (DOL) Chief Evaluation Office (CEO) contracted with Mathematica and the University of Connecticut Health Center to generate new evidence on factors associated with opioid use among workers. This study uses county-level data on employment and legal opioid transactions from the first quarter (Q1) of 2018 to the fourth quarter (Q4) of 2020 to address the following research question: How do unexpected changes in local labor market conditions affect opioid use?

To tease out the causal relationship between the change in labor market conditions associated with the pandemic and opioid use, we implemented a quasi-experimental regression design that takes advantage of (1) the unexpected nature of the pandemic; and (2) cross-county variation in industry composition, which leads to county-level variation in vulnerability to the effects of the pandemic on the labor market. In brief, we find that worsening labor market conditions associated with the pandemic led to an increase in legal opioid transactions relative to what they would have been in its absence. This increase is especially notable in the Northeast and West, which were more vulnerable to job loss due to the pandemic compared to the North Central and South regions.

In the remainder of the report, we first describe the data and methods we used to conduct the analyses (Chapters II and III). Chapter IV presents findings from the analyses and Chapter V summarizes the implications of the findings and identifies research areas for future study. Appendices A and B provide additional detail on analysis methods and tables containing supplemental results, respectively. Appendix C includes results of sensitivity analyses.

II. Data

We conducted descriptive and regression analyses using multiple data sources. In combination, these data sources provide measures of COVID-19 incidence, employment, opioid transactions, and the stringency of state-imposed social distancing measures. We relied on data from three key sources: the Quarterly Census of Employment and Wages (QCEW), the Automated Reports and Consolidated Ordering System (ARCOS), and the COVID-19 Government Response Tracker.

Table 1 includes a description of each data source and the key measures we used for descriptive and regression analyses. We used the QCEW to measure employment by industry at the county level. The QCEW is derived from administrative data and is more complete than employment data from surveys.

We used ARCOS data to measure opioid transactions. The ARCOS data quantify legal opioid transactions rather than opioid use. Ideally, we would be able to measure opioid use directly. Although the ARCOS data do not provide a direct measure of opioid use, they should be capturing the amount of opioid pills that are circulating and therefore available for consumption by the public. This includes any portion of prescribed opioids that are ultimately used for nonmedical purposes, either by the patient for whom they were prescribed or by other individuals. However, because ARCOS only covers legal opioid transactions, we cannot capture the sale and illicit use of synthetic opioids and heroin, which have been responsible for a substantial and growing share of opioid deaths in recent years (Baumgartner and Radley 2021). Hence, the findings from our analyses may not have captured the full extent of the impact of changes in local labor market conditions due to the pandemic on opioid use.

Finally, we used the COVID-19 Government Response Tracker, developed and maintained by researchers at the University of Oxford, to measure the stringency of state-level COVID-19 public health and safety measures (Hale et al. 2021). The tracker is updated daily and includes composite measures that conveniently summarize the various policies enacted over time in different locations.

Table 1. Data sources and measures

Source	County- or state-level measure	Periodicity	Type	Analyses
QCEW. This data set contains the number of jobs in a county, quarter, and industry. It is based on state unemployment insurance records and covers about 97 percent of all civilian employment. We imputed suppressed values in QCEW; see Appendix A for a detailed description of the imputation procedure.	Total private sector employment-to-population ratio in county	Quarterly: 2019 Q1 to 2020 Q4	Continuous, time varying	Descriptive, quasi-experimental
	Pre-pandemic share of jobs in industries most vulnerable to COVID-19 in county	2019 (average)	Continuous, constant	Descriptive, quasi-experimental
ARCOS. This data set includes the universe of transactions of the main controlled substances in the United States to points of sale or distribution to consumers (e.g., hospitals, retail pharmacies, medical providers). It contains the total grams of each opioid type by three-digit ZIP code and quarter. We converted all transactions to MME at the county-quarter level.[a]	MME per capita in county	Quarterly: 2018 Q1 to 2020 Q4	Continuous, time varying	Descriptive, quasi-experimental
COVID-19 Government Response Tracker. This database includes daily measures of stringency of social distancing measures on a discrete ordinal scale that differs by category (Hale et al. 2021).	State-level average stringency of social distancing measures	Quarterly: 2020 Q1 to 2020 Q4	Continuous, both constant and time varying	Descriptive, sensitivity tests

Notes: Population rates are calculated using estimates from the U.S. Census Bureau.

[a] We excluded methadone and buprenorphine from the analyses because they are used to treat opioid addiction. We used conversion factors to standardize a gram of each opioid into MME. To map three-digit e codes to counties, we built a three-digit ZIP code-to-county crosswalk using the Census Bureau's crosswalk between counties and ZIP Code Tabulation Areas—areas smaller than three-digit ZIP codes (U.S. Census Bureau n.d.). We used the share of a three-digit ZIP code population living in a county as a weight to map ARCOS transactions to the county level.

ARCOS = Automated Reports and Consolidated Ordering System; MME = morphine milligram equivalents; QCEW = Quarterly Census of Employment and Wages

III. Methods

The study objective was to understand how the shock to labor market conditions associated with the COVID-19 pandemic impacted opioid transactions. Direct estimates of the observed relationship between changes in employment and opioid transactions might be misleading for at least three reasons: (1) overall, opioid transactions and employment both declined over the time spanning the onset of the pandemic; (2) opioid transactions and employment could both be impacted by factors that are not measured in the data, such as changes in access to medications and elective procedures; and (3) changes in opioid use could have affected employment, leading to bias due to reverse causality.

To overcome these biases, our analytic approach takes advantage of the unexpected nature of the COVID-19 pandemic and cross-county variation in industrial composition. The latter variation means counties across the nation differ in their vulnerability to the effects of the pandemic on the labor market; counties with higher shares of jobs in industries that are sensitive to the impacts will be more vulnerable than counties with lower shares of such jobs. Specifically, we implemented a variation of a difference-in-differences (DiD) design. The classic DiD model compares the change in outcomes between two time periods spanning a sudden policy change or event (in this case, the onset of the COVID-19 pandemic) among two groups: one more affected by the change and one less affected by the change (Cameron and Trivedi 2005). DiD models produce unbiased impact estimates when the two groups have similar trends in the outcome measure before the sudden change.

Instead of splitting all counties into just two groups, as in the classic DiD model, we used QCEW data to assign each county a measure of its vulnerability to job loss because of the pandemic. This measure is equal to the average share of jobs in the industries most vulnerable to COVID-19 in the county in 2019; this period avoids any interference of the pandemic in the calculation. Our model then tests whether counties with larger shares of jobs in the most vulnerable industries had a larger change in opioid transactions after the pandemic relative to counties with lower shares of jobs in those industries. If opioid use increases in counties that are more vulnerable according to this measure relative to those that are less vulnerable, we can attribute the increase to a decrease in county-level employment. We use county as our unit of analysis because it is the smallest geographic area available in the QCEW, and we observe considerable variation in industry composition across counties. Appendix A provides more details about our estimation strategy.

Two assumptions must hold for this causal analysis to be valid. First, the differences in the effect of the pandemic between counties with higher and lower industry vulnerability can only be due to the pandemic's impact on local job markets. That is, the county-level industry vulnerability measure cannot be associated with any other pandemic-related changes that might affect opioid transactions, such as government restrictions that might have reduced access to physicians who prescribe opioids. Second, trends in opioid transactions before the onset of the pandemic must be similar across counties with different levels of vulnerability (this is typically called the "parallel trends assumption"). In the next chapter, where we summarize the results of our analyses, we also describe threats to our model assumptions and provide evidence that suggests the assumptions have not been violated.

IV. Results

In the following chapter, we present a variety of results. In brief, we show that:

- The COVID-19 pandemic was a substantial shock to employment.
- The *observed* relationship between the employment rate and legal opioid transactions is positive.
- Counties varied widely in their vulnerability to job loss due to the pandemic.
- Worsening labor market conditions led to an *increase* in opioid transactions relative to what they would have been in the absence of the pandemic's shock, though the magnitude of this impact varied by geographic region.

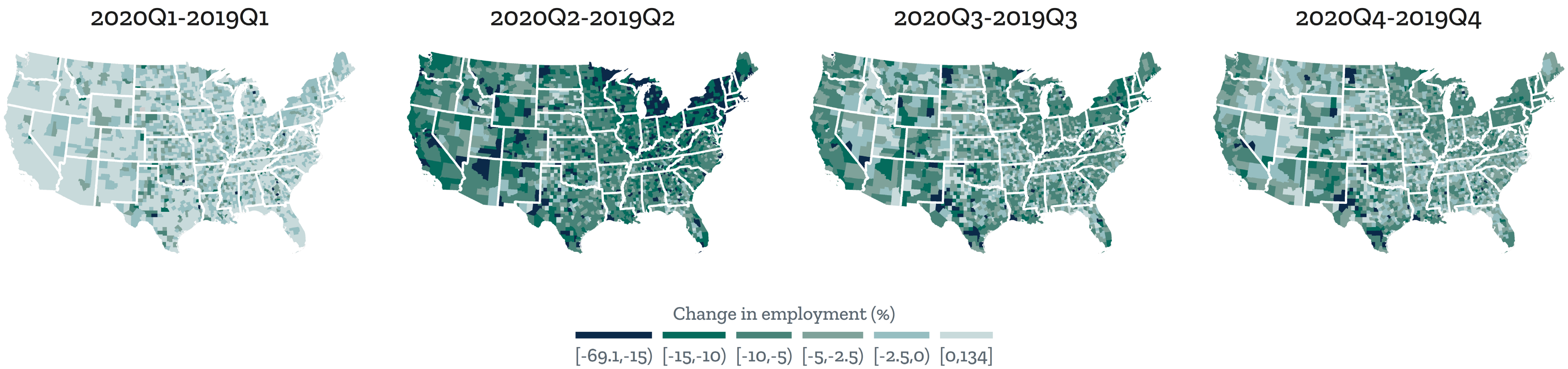
A. Descriptive statistics

The COVID-19 pandemic was associated with a substantial decline in employment beginning in Q2 of 2020 (Figure 1). Nationally, average county-level employment declined by 9.3 percent in Q2 of 2020 compared with the same quarter in 2019, though rates of decline varied widely by county. The sharp employment declines in Q2 of 2020 are reflected in the darker colors in that period compared to Q1 of 2020. The employment rates relative to 2019 improved in the Q3 and Q4 of 2020 but still represented much lower rates compared to the same quarters in 2019.

An examination of national and county-level trends in opioid transactions suggests a positive relationship with employment because both employment and opioid transactions declined in Q2 of 2020. Opioid transactions declined slightly in the period before the pandemic, but the rate of decline slowed in 2020 (Figure 2). The trend in MME per capita is different for methadone and buprenorphine, which are used primarily to treat opioid use disorders. For those opioids, MME per capita increased from 106.3 to 123.0 over our study period. Considering all other opioids in the ARCOS data except methadone and buprenorphine, quarterly MME per capita declined by 14.6 percent in 2019 and 8.2 percent in 2020, on average, compared with the previous year. MME per capita declined in 97.2 percent of counties in the Q2 of 2020 (Figure 3).[2]

[2] Appendix Table B.1 includes summary statistics before and after the onset of the COVID-19 pandemic for our industry vulnerability measure, positive COVID-19 cases per 100,000 individuals, private sector employment per capita, and opioid transactions per capita.

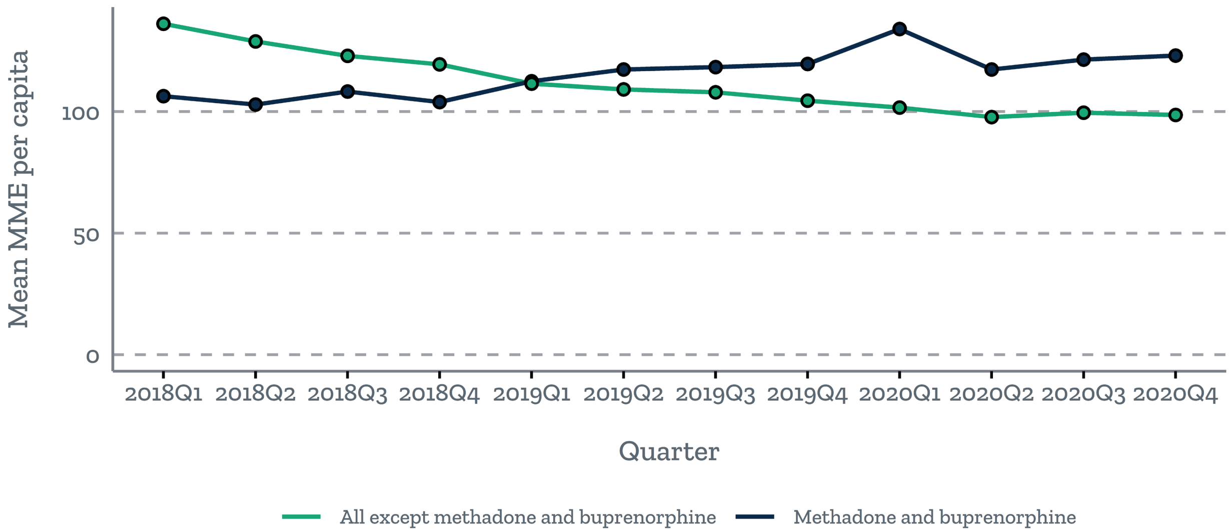
Figure 1. County-level, year-on-year changes in private sector employment, Q1 of 2020 to Q4 of 2020



Notes: This figure shows average changes in employment ratios in 2020 in relation to the same quarter in 2019. The employment ratio is defined as total private sector employment in a county divided by the county's population age 15 or older. The sharp declines in employment in Q2 of 2020 are reflected in the darker colors in that period compared to Q1 of 2020.

Source: Authors' analysis of employment data from the Quarterly Census of Employment and Wages and population estimates from the Census Bureau. Data used to generate this figure are available in a supplemental Excel file.

Figure 2. Trends in opioid transactions by drug type, Q1 of 2018 to Q4 of 2021

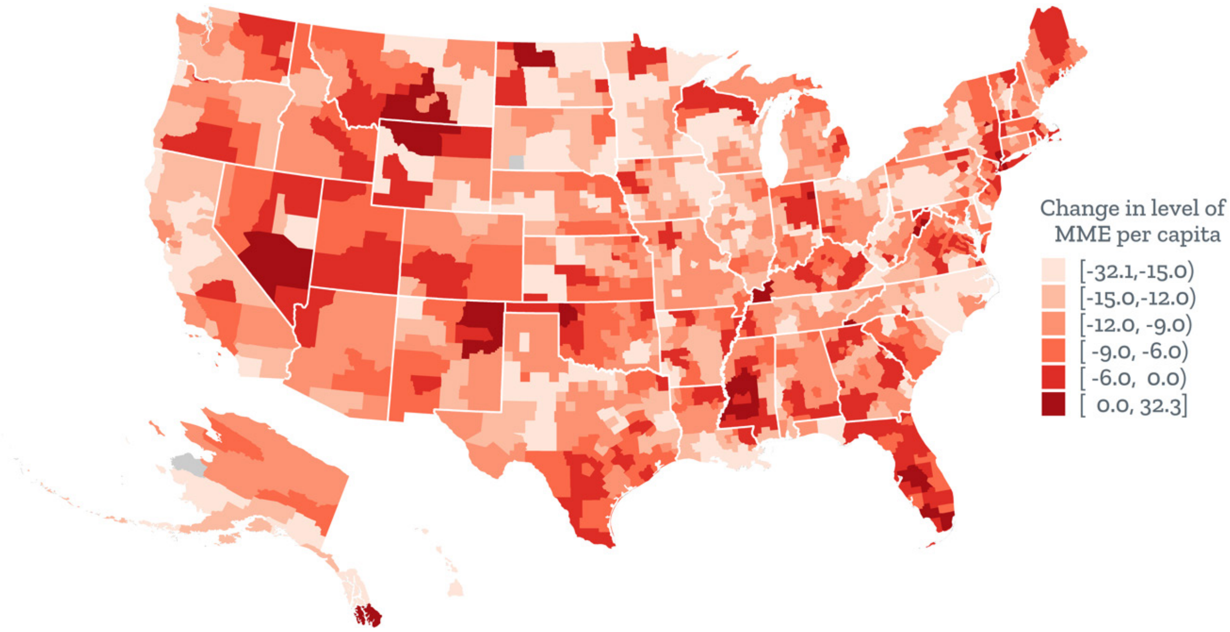


Notes: This figure shows trends in mean MME per capita by county and quarter based on opioid transactions in the Automated Reports and Consolidated Ordering System data. Total MME is calculated based on transactions in the Automated Reports and Consolidated Ordering System. MME per capita is defined as total MME in a county divided by the county's population age 15 or older. The opioids include codeine, fentanyl, hydrocodone, hydromorphone, meperidine, morphine, oxycodone, and oxymorphone, but not methadone or buprenorphine. We excluded those because they are used primarily to treat opioid use disorders.

MME = morphine milligram equivalents

Source: Authors' analysis of data on opioid transactions from the Automated Reports and Consolidated Ordering System.

Figure 3. County-level changes in opioid transactions from Q2 of 2019 to Q2 of 2020



Notes: The figure shows average changes in MME per capita in Q2 of 2020 in relation to the same quarter in 2019. In most counties, MME per capita declined between those quarters; the change ranged from a decline of 32.1 MME per capita to an increase of 32.3 MME per capita. The darkest color in the color scale indicates that a county had an increase. The rest of the color scale indicates levels of decline, where the lightest color indicates the largest decline and darker colors indicate smaller declines. MME per capita is defined as total MME in a county divided by the county's population age 15 or older. Total MME is calculated based on transactions in the Automated Reports and Consolidated Ordering System data of the following drugs: codeine, fentanyl, hydrocodone, hydromorphone, meperidine, morphine, oxycodone, and oxymorphone.

MME = morphine milligram equivalents

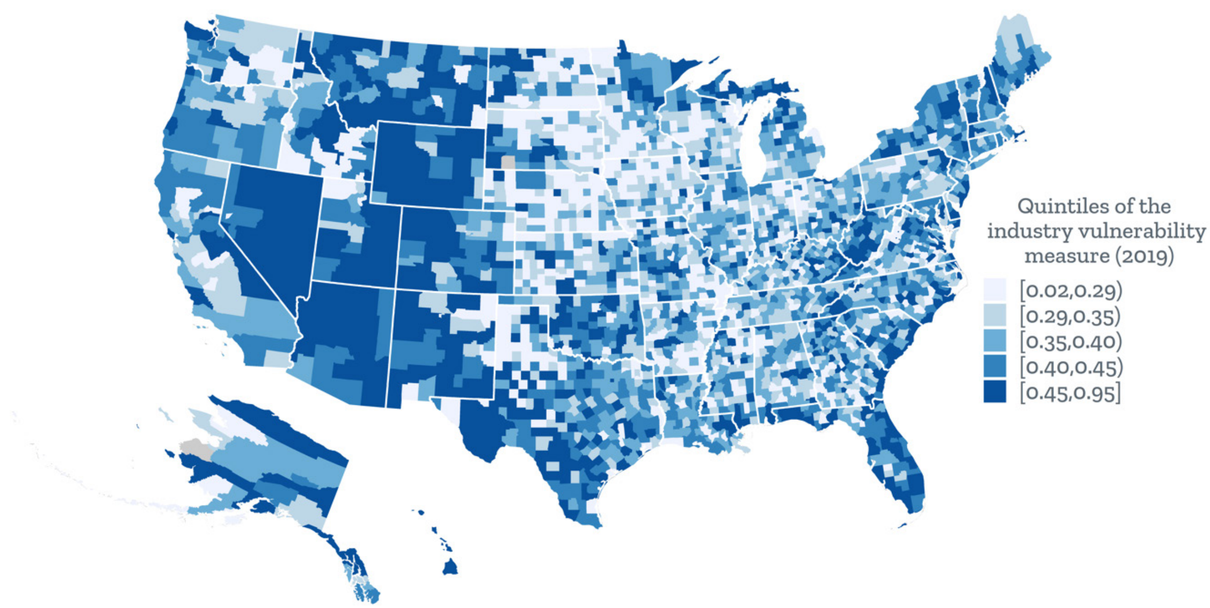
Source: Authors' analysis of data on opioid transactions from the Automated Reports and Consolidated Ordering System. Data used to generate this figure are available in a supplemental Excel file.

B. Industry vulnerability measure

As described in Chapter III, our modified DiD model relies on a pre-pandemic measure of county-level industry vulnerability to the COVID-19 pandemic. We defined vulnerable industries as those that had an overall employment drop larger than 10 percent in Q2 of 2020 relative to the same quarter in 2019, indicating they were the most affected by the pandemic. The vulnerable industries include arts, entertainment, and recreation; accommodation and food services; other services, excluding public administration; mining; administrative and waste services; retail trade; and educational services. The industry vulnerability measure for a county is equal to the proportion of jobs in these industries out of all private sector jobs in the county (see Appendix A for more details).

The average industry vulnerability in our sample is 0.38, which means that in 2019, on average, counties had 38 percent of private sector jobs in the industries most vulnerable to the COVID-19 pandemic. The industry vulnerability measure ranged from 0.02 to 0.95, with the median equal to 0.38 (Figure 4). According to this pre-pandemic measure, the West and Northeast were the regions most vulnerable to job loss because of the COVID-19 pandemic, with industry vulnerabilities of 0.44 and 0.40, respectively. Arizona, Hawaii, and Nevada were the most vulnerable states, along with the District of Columbia, and had more than half the jobs in the industries most vulnerable to the pandemic.

Figure 4. Pre-pandemic industry vulnerability, by county, 2019



Notes: The figure shows county-level industry vulnerability by quintiles, with darker colors indicating higher vulnerability. The first quintile includes the 20 percent of counties with the lowest industry vulnerability based on our measure, which ranges from 0.02 to 0.29 among counties in this quintile. The fifth quintile includes the 20 percent of counties with the highest industry vulnerability based on our measure, which ranges from 0.45 to 0.95 in this quintile.

Source: Authors' analysis of employment data from the Quarterly Census of Employment and Wages. Data used to generate this figure are available in a supplemental Excel file.

The industry vulnerability measure indeed captures only the employment changes that are attributed to the pandemic. As shown in Appendix Figure A.1, this measure is correlated with changes in employment after but not before the pandemic. However, a threat to our causal model is if other factors that are associated with industry vulnerability and are specific to the pandemic affected opioid use. For example, if pandemic-related restrictions were more stringent in higher vulnerability counties relative to lower vulnerability counties, reduced access to physicians may decrease opioid prescriptions, or increased isolation may result in increased stress leading to opioid use. These confounding relationships would cause our estimates to either under- or overstate the impact of employment changes on opioid use. Andersen et al. (2021) argue there is no evidence that pandemic-related restrictions had effects that differed by county based on the share of nonessential workers. Similarly, we would not expect these policies to have effects that differed by county based on our vulnerability measure. Nevertheless, we conduct an empirical test, described below, to check for correlation between the industry vulnerability measure and social distancing policies.

We used data from the COVID-19 Government Response Tracker to estimate the Pearson correlation coefficient between industry vulnerability and the average stringency of social distancing policies in the post-pandemic period in the second, third, and fourth quarters of 2020. We find small correlations, ranging from -0.34 to 0.33, between our measure and a variety of social distancing policies (Table 2). While the correlation coefficients in Table 2 do not indicate a clear relationship between industry vulnerability and social distancing measures, we also tested the robustness of our results to the addition of controls for stringency of select social distancing measures, and find estimates that are very similar to our main estimates (see Appendix C).

Table 2. Correlation between state average industry vulnerability and stringency of state-level social distancing measures during the COVID-19 pandemic

	Correlation with state average industry vulnerability (Pearson correlation coefficient)
School closing	**-0.34**
Workplace closing	0.04
Cancelled public events	0.06
Restrictions on gatherings	-0.10
Closed public transport	0.01
Stay-at-home requirements	**0.33**
Restrictions on internal movement	0.18
International travel controls	0.23

Notes: Stringency of social distancing measures is measured on a discrete ordinal scale that differs by category. For example, school closing measures are coded on a scale from 0 through 3, where 0 = no measures; 1 = recommend closing or all schools open with alterations resulting in significant differences compared to non-COVID-19 operations; 2 = require closing (only some levels or categories; e.g., just high school or just public schools); and 3 = require closing all levels. The COVID-19 Government Response Tracker provides a daily measure of stringency, and we take an average over all of the days in the second, third, and fourth quarters of 2020. State average industry vulnerability is a population-weighted mean of county industry vulnerability. Statistically significant estimates ($p<0.05$) are in bold.

Source: Authors’ analysis of data from the Quarterly Census of Employment and Wages and the COVID-19 Government Response Tracker.

Another threat is to the parallel trends assumption, which requires that trends in opioid transactions before the onset of the pandemic be similar across counties with different levels of vulnerability. This assumption would be violated if pre-pandemic trends in opioid transactions differed systematically between counties with high and lower vulnerability measures. In section C, below, we present the results of an event study analysis that further tests the parallel trends assumption.[3]

C. Quasi-experimental analysis

1. Main results and regional variation

Our main quasi-experimental model is a variation on the classic DiD model. Instead of splitting all counties into just two groups with high and low industry vulnerability to job loss because of the pandemic, we assign each county a precise measure of its industry vulnerability. Our model then tests whether counties with larger shares of jobs in the most vulnerable industries had a larger change in opioid transactions after the pandemic relative to counties with lower shares of jobs in those industries. Results from this model indicate that a 10-percentage point increase in industry vulnerability was associated with a 0.16 MME increase in opioid transactions (Table 3, Column 1). To put this estimate in perspective, a 10 -percentage point increase in industry vulnerability from 35 to 45 percent would move a county from the third quintile of the distribution of the industry vulnerability measure to the fourth quintile. However, this estimate for the effect of industry vulnerability on opioid transactions is not statistically significant.

The results from our main model cannot statistically validate the existence of a causal relationship between industry vulnerability and opioid transactions. However, our main estimate might be imprecise due to regional variation in the impact of the pandemic on employment, which varied by region over time. In particular, the pandemic had a major early effect on employment in the West and Northeast (see Figure 4). We might therefore expect to observe differences in impact estimates across regions. To test for differences by region, we implemented a variant of our model that allowed us to estimate impact estimates by region. The regional estimates (Table 3, Column 2) indicate that high-vulnerability counties in the Northeast and West indeed had larger increases in opioid transactions than high-vulnerability counties in the North Central and South regions. (We obtained similar results when we ran our main model separately by region.) Specifically, the estimates show that an increase of 10 percentage points in the share of jobs in vulnerable sectors in a county in the Northeast increased the MME per capita by 2.99 post-pandemic. The same increase in the vulnerability measure in a county in the West increased the MME per capita by 0.94 post-pandemic.

The regional estimates suggest modest increases in MME per capita in vulnerable counties in the Northeast and West regions. However, the impact of pandemic-related job loss on opioid transactions among opioid users might be substantial, especially in the Northeast. The CDC estimates that about 4.3 percent of the population fills an opioid prescription in each quarter, and the average prescription contains 873 MME (CDC 2018). Adjusting our measure of change in MME per capita to estimate MME per opioid user translates to an increase in MME of 69.4 (or 8 percent of a single prescription) among those who fill at least one opioid prescription in a single quarter.

[3] An event study analysis tests for differences in the outcomes between treated and untreated units, over multiple time periods before and after the initiation of the treatment. In the case of our model, the test is between counties with higher and lower levels of industry vulnerability to the COVID-19 pandemic, and the onset of the pandemic in March 2020 marks the beginning of the post-treatment period. The event study analysis offers evidence on whether trends in the outcome are different between treated and untreated units before and after the onset of the treatment.

Table 3. The effect of industry vulnerability on opioid transactions during the COVID-19 pandemic: estimates from main and regional models

	(1) Main results: MME per capita	(2) Regional results: MME per capita
COVID* Industry Vulnerability	0.16	
	(0.144)	
COVID* Industry Vulnerability: Northeast[a]		**2.99**
		(0.781)
COVID* Industry Vulnerability: South[a]		0.35
		(0.260)
COVID* Industry Vulnerability: West[a]		**0.94**
		(0.294)
COVID* Industry Vulnerability: North Central[a]		**-0.63**
		(0.238)
Mean	111.51	
Mean (Northeast)		107.33
Mean (South)		125.68
Mean (West)		111.17
Mean (North Central)		93.43
Observations	37,692	37,692

Notes: Column 1 shows the results of a pooled difference in differences regression estimating how industry vulnerability affects the impact of the COVID-19 pandemic on MME per capita and column 2 shows the results of a triple difference-in-differences regression estimating how industry vulnerability affects the impact of the COVID-19 pandemic on MME per capita across regions. The regression uses a continuous measure of industry vulnerability that ranges from zero to one and includes county and year-quarter fixed effects. Coefficients represent the post-pandemic change in MME per capita associated with a 10-percentage point increase in industry vulnerability (a similar change could move a county from the third into the fourth quintile of the distribution of the vulnerability measure). Standard errors are clustered at the county level. Statistically significant estimates ($p<0.05$) are in bold.

[a] These coefficients are interpreted as the DiD estimates within each region, which we obtained by combining the coefficients from interaction terms in the triple DiD regression.

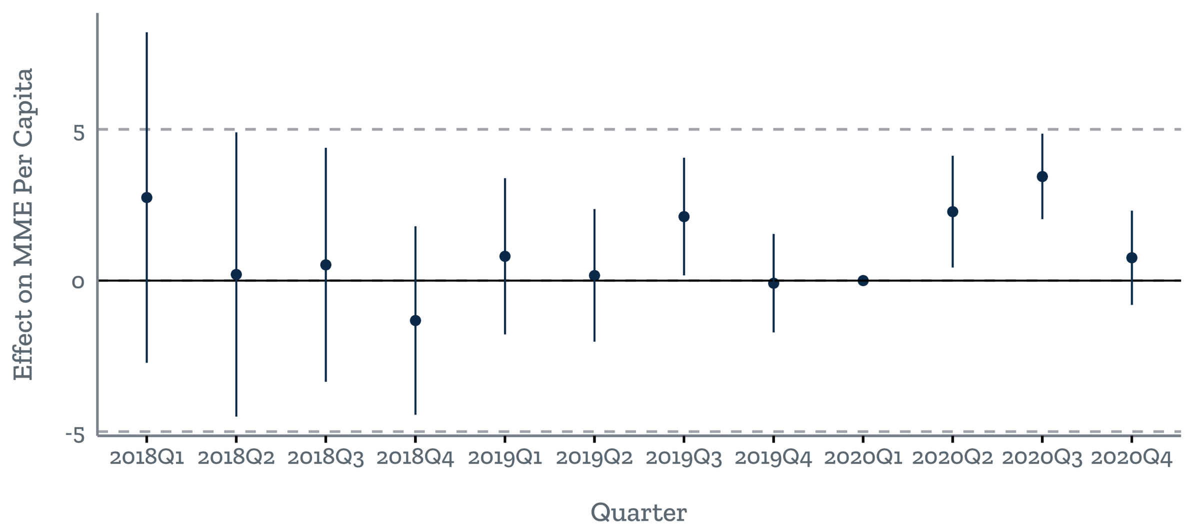
MME = morphine milligram equivalents

Source: Authors' analysis of data from the Automated Reports and Consolidated Ordering System and the Quarterly Census of Employment and Wages.

2. Event study analysis

We also present an event study analysis to test the parallel trends assumption. This analysis tests for differences in the outcome between treated and untreated units over multiple time periods before and after initiation of the treatment. In the case of our model, the test is between counties with higher and lower levels of industry vulnerability to the COVID-19 pandemic, and the onset in March 2020 marks the beginning of the post-treatment period. For our causal model to be valid, trends in opioid transactions before the onset of the pandemic must be similar across counties with different levels of vulnerability. Figure 5 plots estimates of the impact of industry vulnerability on opioid transactions for each quarter in our data (see Appendix A for details on how we implemented the event study analysis). In the pre-pandemic period (Q1 of 2018 through Q1 of 2020), statistically significant estimates or a consistent pattern of estimates would indicate that trends in opioid transactions differed systematically by industry vulnerability already before the pandemic; this would be a violation of the parallel trends assumption. Figure 5 plots estimated coefficients with 95 percent confidence intervals. In the pre-pandemic period (prior to Q2 of 2020), the confidence intervals of all coefficients except Q3 of 2019 include zero and are not statistically significant. The lack of statistically significant pre-pandemic coefficients, combined with the lack of a clear pattern in those coefficients, suggests the parallel trends assumption is not violated and our causal model is valid. In the post-pandemic period, we find statistically significant coefficients in Q2 and Q3 of 2020, but not in Q4.

Figure 5. Event study plot



Notes: This figure shows the estimated coefficients of an event study model estimating how different levels of industry vulnerability affect MME per capita before and during the COVID-19 pandemic. The regression includes county fixed effects, and the omitted category is Q1 of 2020. The bars show the 95 percent confidence interval of the point estimates. Standard errors are clustered at the county level.

MME = morphine milligram equivalents

Source: Authors' analysis of data from the Automated Reports and Consolidated Ordering System and the Quarterly Census of Employment and Wages.

3. Sensitivity tests

We conducted several sensitivity tests to determine the impact of our design choices on the estimates presented in this chapter. See Appendix C for descriptions of our sensitivity analyses and their results. In summary, we find our estimates are robust to a variety of alternative specifications, including:

- **Adding controls for state-level social distancing requirements to both our main and regional models.** For this test, we added controls for two state-level measures of the stringency of social distancing measures and observed how our estimates changed. We found similar estimates to our main results, presented in Table 3, which suggests our estimates were unlikely to be biased by correlation between the industry vulnerability measure and social distancing measures that might also affect opioid transactions (Appendix Table C.1).
- **Pooled (non-regional) analysis using two-stage least squares (2SLS) rather than our modified DiD model, with industry vulnerability as an instrument for employment change.** We used an instrumental variables (IV) method to directly include a measure of change in employment as the regressor of interest in a model of changes in opioid transactions. We find estimates suggesting an increase in opioid transactions in counties that experienced more job loss, supporting our assertion that we can attribute our estimates of the impact of changes in industry vulnerability to changes in employment (Appendix Table C.2).
- **Using two different binary measures of industry vulnerability rather than a continuous measure in a DiD model.** In both our main and regional analyses, we find that estimates derived from traditional DiD models using binary measures for high and low industry vulnerability are very similar to our main estimates, and confirm that the continuous measure of industry vulnerability performs similarly to a traditional DiD treatment measure (Appendix Table C.3).
- **Repeating our analysis using only non-tradeable industries in construction of the industry vulnerability measure.** This variation tests the robustness of our estimates to our choice of all industries to include in the vulnerability measure, and we find very similar estimates in both our main and regional analyses to our main estimates reported in Table 3 (Appendix Table C.4).

V. Conclusions

Overall, legal opioid transactions decreased during the COVID-19 pandemic, though more slowly than they had been decreasing before Q2 of 2020. However, we find that worsening labor market conditions associated with the pandemic led to an increase in legal opioid transactions relative to what they would have been in the absence of this shock. This increase is especially notable in the Northeast and West, which were more vulnerable to job loss due to the pandemic compared to the North Central (Midwest) and South regions.

Our findings have implications for early stages of future epidemics and pandemics that lead to widespread closure of businesses, schools, and government entities. COVID-19 has demonstrated that pandemics can create a variety of health-related emergencies that are not directly caused by the infectious disease itself. For example, the pandemic led to declines in use of preventive and elective medical care (Czeisler et al. 2020), likely declines in mental health and wellbeing (Pfefferbaum and North 2020), and an increase in opioid overdoses (Ochalek et al. 2020). Our findings indicate that locations more affected by a pandemic-caused employment contraction might present a sharper increase in opioid transactions, thus potentially increasing health emergencies and putting further stress on emergency medical providers.

A key limitation of our study is that we only measure the employment-related effects of the pandemic on *legal* opioid transactions. Although legal transactions are likely to be strongly correlated with both medical and nonmedical opioid use as well as overdoses (Modarai et al. 2013), our analysis might be underestimating the effect on overall opioid use because we cannot measure illicit use, which is responsible for a substantial and growing share of opioid deaths in recent years (Baumgartner and Radley 2021). Furthermore, while we find that changes in local labor markets affected opioid transactions, we cannot isolate the precise mechanisms that combine to generate this effect. As we outlined in Chapter I, declines in employment might increase opioid use via several channels, including the impact of job loss and reduced earnings on workers' physical and mental health, having less income available to purchase opioids, and having fewer work injuries. These possible mechanisms imply both positive and negative impacts on opioid use following declines in employment. Overall, however, our findings suggest that worsening labor market conditions will lead to increased opioid transactions.

There are several opportunities to expand on this work to gain a more comprehensive understanding of the relationship between job loss related to the COVID-19 pandemic and opioid use. First, we could examine the impact of the pandemic on opioid transactions over a longer time horizon as more ARCOS data become available from 2021 and down the road. Second, we could explore different geographic units of analysis, such as Metropolitan Statistical Area (MSA) or states, which might enable us to measure different opioid use-related outcomes like overdoses, deaths, emergency department visits, or calls to poison control centers. Finally, we could measure buprenorphine and methadone transactions as outcomes to observe the relationship between local job loss and the availability of medication-assisted treatment (MAT) for opioid use disorder.

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Appendix A: Methods

In the following sections, we describe the procedure we use to impute missing values in the Quarterly Census of Employment and Wages (QCEW) and our quasi-experimental estimation strategy.

Imputation procedure for missing values in QCEW

The QCEW suppresses total employment at the county-industry-quarter level when an employer could potentially be identified by the number of jobs in a county-industry, though state-industry level employment is always reported. To impute employment for missing county-industry cells, we imputed employment based on residual employment at the state level using the number of establishments in a county as weights and implementing the following steps:

1. Using the QCEW at the state-quarter level, compute total employment by state and industry ($StateIndEmp$).
2. Using the QCEW at the county-quarter level, compute the state total employment for disclosed cells and number of establishments in suppressed cells by summing them within each state and industry ($StateIndEmpDisclosed$ and $StateIndEstablishmSuppressed$).
3. Subtract the total employment from disclosed counties from the state total employment to obtain the number of jobs in each state and industry that is not disclosed at the county level:

$$StateIndEmpRemainder = StateIndEmp - StateIndEmpDisclosed$$

4. Divide the number of undisclosed jobs by the total number of suppressed establishments in the state and industry:

$$IndEmpByEstablishmSuppressed = \frac{StateIndEmpRemainder}{StateIndEstablishmSuppressed}$$

5. For each industry, assign undisclosed jobs to counties based on the number of establishments with suppressed values in each county by multiplying the number of suppressed jobs per suppressed establishment (from Step 3) and the number of establishments in the county (from Step 2):

$$CountyIndEmpImpute = IndEmpByEstablishmSuppressed * CountyNumberEstablishments$$

Quasi-experimental estimation strategy

We used a variation of a difference-in-differences (DiD) design. For our main model, rather than splitting our study sample into two groups, we constructed a continuous measure of county-level vulnerability to job loss because of the pandemic. This measure is equal to the average share of private sector jobs in the industries most vulnerable to COVID-19 in each county c in 2019. We use only private sector jobs because, due to confidentiality concerns, the number of public sector jobs is more likely to be censored at the county-industry level. Over our study period from 2018 through 2020, 54.2 percent of public sector jobs was missing in the QCEW. This period avoids any interference of the pandemic in the calculation.

1. Industry vulnerability measure

We defined the share of jobs in the industries most vulnerable to COVID-19 in a county c as:

$$IndustryV_c = \frac{\sum_{j \in vulnerable} employment_{jc2019}}{\sum_j employment_{jc2019}}$$

We defined vulnerable sectors as those that experienced an overall employment drop larger than 10 percent in Q2 of 2020 relative to the same quarter in 2019. The industries include arts, entertainment, and recreation (NAICS 71); accommodation and food services (72); and other services—except public administration (81), mining (21), administrative and waste services (56), retail trade (44-45), and educational services (61).

The industries we identified as vulnerable are listed in bold in Table A.1, and all show a decline in employment over 10 percent in the Q2 of 2020 relative to Q2 of 2019. The next highest decline, in manufacturing (NAICS 31-33), was two percentage points smaller than the 12 percent decline in employment in educational services. This group of industries is very similar to what is used in recent research about the COVID-19 pandemic. Forsythe et al. (2020) show that jobs in leisure and hospitality (71-72) and non-essential retail (NAICS 44-45 and New York state guidelines) fell sharply by the end of April 2020, while essential retail is the only sector that saw growth in job postings during this period. Cajner et al. (2020) show that paid employment from February to April 2020 fell by 50.7 percent in arts and entertainment (NAICS 71), 45.4 percent in hospitality (72), and 28.9 percent in retail trade (44-45). Kurmann, Lale, and Ta (2020) focus on low-wage employment and show similar results: By mid-April 2020, employment fell by 67 percent in leisure and hospitality, 56 percent in retail trade, and 51 percent in education.

To confirm that the industry vulnerability measure captures only the employment changes that are attributed to the pandemic, we plot changes in private sector employment per capita against the industry vulnerability measure both before and after the onset of the pandemic (Figure A.1). We find that the vulnerability measure is indeed correlated with changes in employment after but not before the onset.

2. Main model

We estimated the following model:

$$Opioid_{ct} = \beta_1 COVID_t \cdot IndustryV_c + [county\ FE] + [\text{year-quarter}\ FE] + \eta_{ct}$$

In this model: $Opioid_{ct}$ is opioid transactions in county *c* and year-quarter *t*, measured by morphine milligram equivalents (MME) per capita; $IndustryV_c$ is the measure of industry vulnerability to the pandemic, as defined above; and COVID is a binary variable indicating that an observation occurred after the beginning of the pandemic (i.e., in Q2 of 2020 or later). County fixed effects control for differences in patterns of opioid transactions across counties. Year-quarter fixed effects control flexibly for common trends in opioid transactions across counties. The interpretation of β_1, the coefficient of interest, is the change in the number of opioid transactions after the pandemic for each 1 percentage point increase in the share of jobs in the most vulnerable industries. A positive and statistically significant β_1 would imply that counties with a larger employment share in the most vulnerable industries had a larger increase in opioid transactions after the pandemic began compared with counties with a lower share of those industries. If so, employment changes were a mechanism through which the pandemic affected opioid transactions. To obtain subgroup estimates by region, we interact $COVID_t \cdot IndustryV_c$ with a categorical indicator variable for census region, using the North Central region as the base region.[4]

Table A.1: Percentage change in employment by industry and quarter in 2020

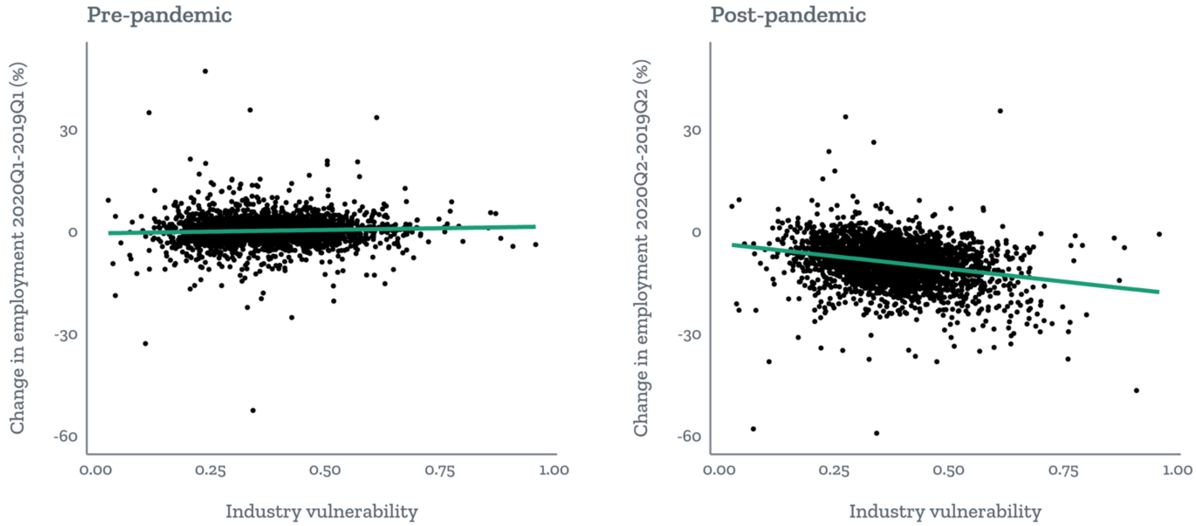
NAICS	Industry	2020 Q2	2020 Q3
71	**Arts, Entertainment, and Recreation**	**-48.19**	**-34.79**
72	**Accommodation and Food Services**	**-38.12**	**-24.11**
81	**Other Services (Except Public Administration)**	**-25.16**	**-15.40**
21	**Mining, Quarrying, and Oil and Gas Extraction**	**-21.85**	**-25.88**
56	**Administrative and Waste Services**	**-15.01**	**-10.57**
44-45	**Retail Trade**	**-12.90**	**-4.93**
61	**Educational Services**	**-11.94**	**-9.81**
31-33	Manufacturing	-9.89	-6.39
53	Real Estate and Rental and Leasing	-9.60	-8.44
51	Information	-8.09	-8.23
23	Construction	-7.65	-4.75
62	Health Care and Social Assistance	-7.32	-4.42
42	Wholesale Trade	-7.12	-6.03
55	Management of Companies and Enterprises	-5.86	-5.11
48-49	Transportation and Warehousing	-3.32	-0.51
11	Agriculture, Forestry, Fishing, and Hunting	-2.77	-3.64
54	Professional, Scientific, and Technical Services	-2.58	-2.32
22	Utilities	-1.12	-1.19
52	Finance and Insurance	-0.07	-0.27
99	Unclassified	6.22	23.38

Notes: The base period is the corresponding quarter in 2019. Bolded rows indicate the industries we identified as most vulnerable to job loss due to the COVID-19 pandemic.

Source: Authors' analysis of employment data from the Quarterly Census of Employment and Wages and population estimates from the Census Bureau.

[4] We use the Census Bureau definition of regions, which splits the states and District of Columbia into four regions: North Central (IL, IN, IA, KS, MI, MN, MO, NE, ND, OH, SD, and WI), Northeast (CT, ME, MA, NH, NJ, NY, PA, RI, and VT), South (AL, AR, DC, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, and WV), and West (AK, AZ, CA, CO, HI, ID, MT, NV, NM, OR, UT, WA, and WY).

Figure A.1 Correlation between changes in employment and industry vulnerability, pre- and post-pandemic



Notes: This figure plots county-level changes in the employment ratio against the industry vulnerability measure. The left-hand panel shows the change in employment in Q1 of 2020, which is the last quarter prior to the onset of the pandemic, relative to Q1 of 2019 plotted against the industry vulnerability measure. The right-hand panel shows the change in employment in Q2 of 2020, which is the first quarter after the onset of the pandemic, relative to Q2 of 2019 plotted against the industry vulnerability measure. The employment ratio is defined as total private sector employment in a county divided by the county's population age 15 or older.

Source: Employment data from the Quarterly Census of Employment and Wages and population estimates from the Census Bureau

3. Identifying assumption

Our identifying assumption, critical to drawing causal inference, is that *trends* in opioid transactions do not differ systematically by county-level industry vulnerability, and in the absence of the COVID-19 pandemic, those trends would continue. It is likely that *levels* of opioid transactions are correlated with industry vulnerability, so our measure is not external to the opioid transactions outcome. A correlation between industry vulnerability and levels of opioid transactions is not a threat to identification if the time trends in opioid transactions are moving in parallel before the beginning of the pandemic. To check for a violation of the parallel trends assumption, we conducted an event study analysis (see Figure 5).

We estimated the following event study model:

$$Opioid_{ct} = \sum_{\tau=-8,\tau\neq 0}^{3} \beta_\tau P_\tau \cdot IndustryV_c + [county\ FE] + [\text{year-quarter}\ FE] + \eta_{ct}$$

In the equation above, the coefficients of interest are represented by β_τ and measure the differential effect of industry vulnerability over time. The variables P_τ are event time indicator variables equal to one if, in quarter t, the pandemic hit τ quarters ago (e.g., in Q1 of 2018, eight quarters before the baseline, P_{-8} equals one; and in Q4 of 2020, three quarters after the pandemic, P_3 equals one). The event time indicator variable P_0 is normalized to zero—the baseline period is Q1 of 2020. Therefore, the estimated coefficients are interpreted as changes in opioid transactions relative to the period before the pandemic hit the hardest.

We tested the joint significance of the β_τ coefficients in the pre-pandemic period (quarters -8 through -1, or Q1 of 2018 to Q4 of 2019) using an F-test. The coefficients are jointly significant, with an F statistic of 4.99 and a *p*-value of 0. Joint statistical significance of the pre-period indicators could indicate that the trend in opioid transactions pre-pandemic was different in counties with low and high industry vulnerability. However, as shown in Figure 5, there is no systematic pattern in the coefficients in the pre-period. Although the coefficients are jointly significant, they vary around zero, with some negative and some positive coefficients, and only one that is statistically significant on its own. Considering this pattern of pre-period coefficients, we conclude there is no evidence against the identifying assumption of parallel pre-trends.

Appendix B: Additional Table

Table B.1. Summary statistics for industry vulnerability, employment, and opioid transactions: pre-pandemic (Q1 of 2018 to Q1 of 2020) vs. post-pandemic (Q2 of 2020 to Q4 of 2020)

	All counties			Low industry exposure counties			High industry exposure counties			
	Pre-pandemic	Post-pandemic	Change	Pre-pandemic	Post-pandemic	Change	Pre pandemic	Post-pandemic	Change	DiD
Industry vulnerability	0.38	--		0.30	--		0.46	--		
	(0.001)	--		(0.001)	--		(0.001)	--		
Private sector employment per capita	0.43	0.41	-0.03	0.43	0.41	-0.02	0.44	0.41	-0.03	-0.01
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)	(0.003)	(0.005)
Opioid per capita (MME)	115.80	98.66	-17.14	113.76	96.45	-17.31	117.84	100.87	-16.97	0.34
	(0.280)	(0.411)	(0.540)	(0.386)	(0.561)	(0.744)	(0.405)	(0.600)	(0.782)	(1.079)
Total private sector employment per quarter (million)	143.89	133.06		54.31	50.53		89.58	82.54		
Total MME per quarter (million)	28,722.42	24,419.47		9,955.80	8,372.06		18,766.63	16,047.41		
Total population 15 and older (million)	267.67			94.97			172.69			

Notes: Standard errors are in parentheses. The population denominator for all variables is the working-age population, defined as people 15 years or older. High industry vulnerability counties are those with industry vulnerability values at or above the median (0.377); low industry vulnerability counties are those below the median. The pre-pandemic period is defined as Q1 of 2018 to Q1 of 2020; the post-pandemic period is defined as Q2 of 2020 to Q4 of 2020.

DiD = difference in differences; MME = morphine milligram equivalents.

Source: Authors' analysis of data from the Quarterly Census of Employment and Wages and Automated Reports and Consolidated Ordering System data.

Appendix C: Sensitivity Analyses

We conducted the four sensitivity tests described below to determine the impact of our design choices on the estimates presented in Chapter IV.

Controls for state-level social distancing measures

We ran our main (pooled) and regional analyses, including controls for state-level social distancing measures. We included two state- and quarter-level measures of the stringency of social distancing measures based on data from the COVID-19 Government Response Tracker, which tracks state-level measures at the daily level. We used an index measure of stringency that combined all social distancing measures as well as a measure of the stringency of stay-at-home requirements.

Table C.1 presents the results of this sensitivity test, which are very close to the results from our main models. The social distancing measures are indeed correlated with our outcome measure. However, their inclusion in the model does not affect our main estimates that suggest the industry vulnerability measure affected opioid transactions only via its association with pandemic-related employment loss.

Table C.1. The effect of industry vulnerability on opioid transactions during the COVID-19 pandemic: estimates from main and regional models including stay-at-home or stringency index

	(1) Main results: MME per capita	(2) Main results: MME per capita	(3) Regional results: MME per capita	(4) Regional results: MME per capita
COVID* Industry Vulnerability	0.14	0.16		
	(0.145)	(0.147)		
COVID* Industry Vulnerability: Northeast[a]			**0.30**	**0.29**
			(0.079)	**(0.077)**
COVID* Industry Vulnerability: South[a]			0.03	0.03
			(0.026)	(0.026)
COVID* Industry Vulnerability: West[a]			**0.09**	**0.10**
			(0.029)	**(0.029)**
COVID* Industry Vulnerability: North Central[a]			**-0.07**	**-0.07**
			(0.024)	**(0.024)**
Stay-at-home index	**0.81**		**1.64**	
	(0.039)		**(0.402)**	
Stringency index		0.00		**0.05**
		(0.001)		**(0.015)**
Mean	111.51	111.51		
Mean (Northeast)			107.33	107.33
Mean (South)			125.68	125.68
Mean (West)			111.17	111.17
Mean (North Central)			93.43	93.43
Observations	37,692	37,692	37,692	37,692

Notes: Columns 1 and 2 show the results of a pooled difference-in-differences regression estimating how industry vulnerability affects the impact of the COVID-19 pandemic on MME per capita; columns 3 and 4 show the results of a triple difference-in-differences regression estimating how industry vulnerability affects the impact of the pandemic on MME per capita across regions. The regression uses a continuous measure of industry vulnerability that ranges from 0 to 1 and includes county and year-quarter fixed effects. Coefficients represent the post-pandemic change in MME per capita associated with a 10 -percentage point increase in industry vulnerability (a similar change could move a county from the third into the fourth quintile of the distribution of the vulnerability measure). Standard errors are clustered at the county level. Statistically significant estimates ($p<0.05$) are in bold.

[a] These coefficients are interpreted as the difference-in-differences regression estimates within each region, which we obtained by combining the coefficients from interaction terms in the triple difference-in-differences regression regression.

MME = morphine milligram equivalents

Two-stage least squares estimates

We asserted that the impact estimates from our main models can be interpreted as the causal impact of changes in employment on opioid transactions, though we indirectly measure employment using our industry vulnerability measure. As an alternative to a DiD design, we can use the industry vulnerability measure as an instrumental variable for actual changes in employment in a two-stage least squares (2SLS) instrumental variables (IV) estimation strategy. We estimated a 2SLS model, including controls for the stringency of state-level social distancing measures, and obtained estimates similar to those from our main model, supporting our assertion that we can attribute our main estimates to changes in employment. The 2SLS model (columns 3, 4) finds a negative causal relationship between changes in employment and changes in opioid transactions, indicating that an employment decline would cause an increase in opioid transactions. In contrast, a direct ordinary least squares (OLS) regression (columns 1, 2) estimates a positive relationship between changes in employment and changes in opioid transactions.

Table C.2. OLS and IV estimates of how changes in employment affected opioid transactions

	(1) OLS	(2) OLS	(3) IV	(4) IV
Change in employment ratio	**0.04**	**0.04**	**-0.29**	**-0.37**
	(0.015)	**(0.016)**	**(0.076)**	**(0.094)**
Stay-at-home index	**1.44**		**1.27**	
	(0.243)		**(0.273)**	
Stringency index		0.01		**-0.06**
		(0.008)		**(0.017)**
Mean of outcome	-6.87	-6.87	-6.87	-6.87
First stage F statistic	N.A.	N.A.	39.57	159.99
Observations	3,141	3,141	3,141	3,141

Notes: Columns 1 and 2 show the results of a pooled OLS regression; columns 3 and 4 show the results of an IV regression. In columns 3 and 4, the change in employment ratio is instrumented for using the industry vulnerability measure, and the coefficient reported is from the second stage of a two-stage least squares regression. The outcome is the change in MME per capita at the county level in Q2 through Q4 of 2020 relative to Q2 through Q4 of 2019 and Q1 of 2020, while the regressor of interest is the change in the employment ratio over the same timeframe. Standard errors are clustered at the county level. Statistically significant estimates ($p<0.05$) are in bold.

MME = morphine milligram equivalents; OLS = ordinary least squares; IV = instrumental variable.

Binary measures of industry vulnerability

We used two binary measures of industry vulnerability rather than a continuous measure in both our main (pooled) and regional analyses to confirm that the continuous measure of industry vulnerability performs similarly to a traditional DiD treatment measure. This analysis supports nonlinearities in the relationship between industry vulnerability measure and the effect on opioid transactions. We created two binary versions of the industry vulnerability measure: (1) an indicator that equals 1 if a county's vulnerability is at or above the median, and 0 otherwise; and (2) an indicator that equals 1 if a county's vulnerability measure is above 0.5 (that is if more than 50 percent of jobs are in vulnerable sectors), and 0 otherwise.

The results presented in Table C.3 show that the estimated effect of the COVID-19 pandemic on counties with an industry vulnerability score above the median is an increase of 0.34 morphine milligram equivalents (MME) per capita, while for counties with most jobs in vulnerable sectors it is an increase of 1.56 MME per capita relative to counties with most jobs in other sectors. These results suggest that the positive coefficient obtained in our main model is driven mostly by counties with a high share of jobs in the sectors that lost most jobs early in the pandemic. The regional results show similar patterns.

Table C.3. Adjusted difference-in-differences results with alternative binary measures of industry vulnerability

	(1) Main results: binary measure above and below median	(2) Main results: binary measure more than 50 percent	(3) Regional results: binary measure above and below median	(4) Regional results: binary measure more than 50 percent
COVID* Binary Industry Vulnerability	0.03	**1.56**		
	(0.032)	**(0.049)**		
COVID* Binary Industry Vulnerability: Northeast[a]			**0.32**	**0.44**
			(0.135)	**(0.151)**
COVID* Binary Industry Vulnerability: South[a]			**0.15**	0.13
			(0.055)	(0.080)
COVID* Binary Industry Vulnerability: West[a]			0.16	**0.30**
			(0.087)	**(0.080)**
COVID* Binary Industry Vulnerability: North Central[a]			**-0.11**	0.02
			(0.046)	(0.113)
Mean	111.5	111.5		
Mean (Northeast)			107.33	107.33
Mean (South)			125.68	125.68
Mean (West)			111.17	111.17
Mean (North Central)			93.43	93.43
Observations	37,692	37,692	37,692	37,692

Notes: Columns 1 and 2 show the results of a pooled difference in differences regression estimating how industry vulnerability affects the impact of the COVID-19 pandemic on MME per capita; columns 3 and 4 show the results of a triple difference-in-differences regression estimating how industry vulnerability affects the impact of the pandemic on MME per capita across regions. Columns 1 and 3 use a binary measure of industry vulnerability that equals 1 if a county's measure of industry vulnerability is at or above the medial value, and 0 otherwise. Columns 2 and 4 use a binary measure of industry vulnerability that equals 1 if a county's industry vulnerability is greater than 0.5, and 0 otherwise. All models include county and year-quarter fixed effects. Coefficients represent the post-pandemic change in MME per capita associated with a 10 -percentage point increase in industry vulnerability (a similar change could move a county from the third into the fourth quintile of the distribution of the vulnerability measure). Standard errors are clustered at the county level. Statistically significant estimates ($p<0.05$) are in bold.

[a] These coefficients are interpreted as the difference-in-differences estimates within each region, which we obtained by combining the coefficients from interaction terms in the triple difference-in-differences regression.

MME = morphine milligram equivalents

Non-tradeable industries

The fourth sensitivity analysis considered only non-tradeable industries in creating the industry vulnerability measure. We recalculated the measure by excluding the sectors of mining, quarrying, and oil and gas extraction (NAICS 21); and arts, entertainment, and recreation (71); and considered as vulnerable only retail trade (44-45), administrative and waste services (56), educational services (61), accommodation and food services (72), and other services (81). This analysis aims to test the relevance of the local economy to changes in opioid transactions post-pandemic since non-tradeable sectors are less affected by changes in national demand. Table C.4 shows that the estimated effect of how varying levels of industry vulnerability in non-tradable sectors affect MME per capita is similar to our main estimation results, though it is smaller.

Table C.4. Adjusted difference-in-differences results using only non-tradeable industries in the measure of industry vulnerability

	(1) Main results	(2) Regional results
COVID* Non-tradeable Industry Vulnerability	-0.01	
	(0.166)	
COVID* Non-tradeable Industry Vulnerability: Northeast[a]		**3.64**
		(0.886)
COVID* Non-tradeable Industry Vulnerability: South[a]		0.12
		(0.278)
COVID* Non-tradeable Industry Vulnerability: West[a]		**0.80**
		(0.371)
COVID* Non-tradeable Industry Vulnerability: North Central[a]		**-0.55**
		(0.249)
Mean	111.5	
Mean (Northeast)		107.33
Mean (South)		125.68
Mean (West)		111.17
Mean (North Central)		93.43
Observations	37,692	37,692

Notes: Column 1 shows the results of a pooled difference-in-differences regression estimating how industry vulnerability affects the impact of the COVID-19 pandemic on MME per capita; column 2 shows the results of a triple difference-in-differences regression estimating how industry vulnerability affects the impact of the pandemic on MME per capita across regions. The regression uses a continuous measure of industry vulnerability restricted to only non-tradeable industries that ranges from 0 to 1, and includes county and year-quarter fixed effects. Coefficients represent the post-pandemic change in MME per capita associated with a 10-percentage point increase in industry vulnerability (a similar change could move a county from the third into the fourth quintile of the distribution of the vulnerability measure). Standard errors are clustered at the county level. Statistically significant estimates ($p<0.05$) are in bold.

[a] These coefficients are interpreted as the difference-in-differences estimates within each region, which we obtained by combining the coefficients from interaction terms in the triple difference-in-differences regression.

MME = morphine milligram equivalents

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