University of Zurich
Department of Economics

COVID-19, Unemployment, and Drug Mortality in the US

Master's Thesis

Author
Ralf Blöchlinger
Scheffenegg 7,
9200 Gossau SG
ralf.bloechlinger@uzh.ch
Student ID: 19-612-548
Field of Study: Economics

Supervisor
Prof. Dr. Ana Costa-Ramón
Department of Economics and
Jacobs Center for Productive
Youth Development
University of Zurich

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Abstract

I study the effect of the 2020 COVID-19 unemployment shock on drug mortality in the US using a Bartik-style instrument based on pre-pandemic industry composition and national industry employment changes. I find no significant overall effect but a significant positive effect of the unemployment shock on drug mortality within rural counties. This effect accounts for approximately 28% of the increase in drug mortality in rural US counties from 2019 to 2022. Heterogeneity analyses and intermediate outcomes imply that economically vulnerable and credit-constrained rural counties saw larger increases in economic stress in response to the unemployment shock, culminating in drug abuse. Conversely, in urban counties, lower initial economic stress and fewer credit constraints potentially attenuated the relationship between unemployment shocks and drug mortality.
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1 Introduction

The US has experienced a continuous rise in drug mortality since the late 1990s (CDC, 2024).[1] Worryingly, this upward trend increased sharply in 2020 with the start of the COVID-19 pandemic. By 2022, annual drug mortality increased by 50% over pre-pandemic levels (Ahmad et al., 2024). Simultaneously, the COVID-19 pandemic caused a large and sudden increase in unemployment in the US, with unemployment rising from 3.5% in February 2020 to 14.7% in April 2020 (U.S. Bureau of Labor Statistics, 2020a).

Previous literature in economics has suggested a link between economic shocks and drug mortality.[2] Providing evidence on the extent to which unemployment caused this recent rise in drug mortality is essential due to the economic and social costs of drug mortality. Drug mortality is the primary cause of an increase in working age mortality – a phenomenon almost unique to the US among developed countries (Becker et al., 2021).

Isolating this channel is difficult because the pandemic simultaneously affected many other determinants of drug use. Social isolation (Chacon et al., 2021), disruptions to addiction treatment and healthcare more broadly (Russell et al., 2021; Jones et al., 2021), and a shift in the illicit drug supply towards more lethal synthetic opioids (DiGennaro et al., 2021) all changed the risk environment in ways that could confound any unemployment effect. Moreover, pre-pandemic risk factors were non-uniformly distributed across the US. Rural counties entered 2020 with higher economic vulnerability and weaker healthcare access (Andreyeva and Wang, 2023). These structural differences suggest that the same labor market disruption could translate into very different mortality outcomes in rural and urban areas (Monnat, 2019; Walters et al., 2022). As a result of these challenges, the literature has not yet conclusively established what caused the recent increase in drug mortality.

This paper aims to address this research gap by establishing whether the initial COVID-19 unemployment shock in the Spring of 2020 had a causal effect on subsequent drug mortality in the US over the following years. In particular, I aim to answer the following research questions: (i) did the initial COVID-19 unemployment shock in 2020 cause an increase in drug abuse mortality in the US? (ii) was this effect significantly different in urban and rural counties? (iii) what mechanisms explain the effect of the unemployment shock on drug mortality and observed differences between urban and rural counties?

To answer my research questions, I initially develop a toy model of endogenous drug

[1] Drug mortality is defined in this paper and by the CDC based on ICD-10 mortality codes X40-X44, X60-X64, X85, and Y10-Y14 (cf. CDC, 2024). Notably, this includes many types of drugs and medications, such as prescription and illicit opioids, but does not include alcohol.

[2] In addition to the empirical literature discussed below, the issue of drug consumption has also been studied theoretically, see, e.g., Becker and Murphy (1988).

supply and drug demand with a medically motivated mechanism whereby economic stress increases the propensity for drug abuse. Then, using county-level data from 2018 to 2023, I estimate reduced-form and instrumental variable regressions of drug mortality on the unemployment shock, employing a "Bartik-style" (Bartik, 1991) shift-share instrument. My instrument predicts the unemployment shock exposure of a county based on pre-pandemic county-level industry employment and national changes in industry employment in Spring 2020. The Bartik-style approach leverages the finding that the national distribution of shocks to industries is plausibly exogenous, conditional on controls, and averages out unobserved confounders of county-level unemployment shocks, which directly affect drug mortality. I initially highlight that various industries across different regions with different underlying characteristics were exposed to unemployment shocks through heterogeneous mechanisms. Then, I provide suggestive evidence about the instrument's validity and potential bias in reduced-form regressions. Following the estimation of the main results, I separately estimate the effect in rural counties and explore various heterogeneity dimensions of my estimates to provide plausible evidence for the mechanism by which unemployment may lead to drug abuse risk and why the mechanism may differ between urban and rural counties.

Reduced-form regressions and my instrument do not identify any significant relationship between the unemployment shock and drug mortality in population-weighted regression. This implies that the average treatment effect of the unemployment shock for the typical US citizen is plausibly zero. These findings importantly depend on including a rich set of controls that attenuate the estimated coefficients to zero. On the other hand, I find a significant effect in reduced-form and instrumental variable regressions when restricting my analysis to rural counties. The estimated effect accounts for approximately 28% of the increase in drug mortality in rural counties from 2019 to 2022. In an event study specification, I additionally investigate effect timing and find that the instrument passes placebo checks prior to the pandemic, with a first significant positive effect in 2021, in the first full year after the shock. The effect remains significant in 2022 before attenuating in 2023, suggesting a delayed effect of the unemployment shock on drug mortality which is in line with expected effect times of drug abuse turning to mortality (e.g., Powell et al., 2023).

My findings suggest that the effect is heterogeneous, and the impact and mechanism may differ fundamentally between urban and rural counties. Exploring heterogeneous effects further, the analysis reveals that the effect is concentrated in rural counties characterised by higher pre-pandemic economic vulnerability (poverty, mental distress, weaker credit access) and weaker social support systems, supporting the hypothesis that the shock primarily impacted drug mortality through increased economic stress. This channel is further evidenced by the shock's association with increased food insecurity, social vulnerability, and housing cost burden during the pandemic and limited take-up of debt or unemployment benefits in rural counties relative to urban counties. Qualitative studies of drug abuse during the COVID-19 pandemic (Sweeney et al., 2024) and medical research on drug use (Sinha, 2001) support this mechanism.

The attenuated effects in urban counties suggest an interrupted mechanism. I find similar, although weaker, effects of the unemployment shock on economic hardship indicators in metropolitan counties. However, these effects do not translate into drug mortality. Based on my stylized model and descriptive statistics, I provide suggestive evidence that easier access to unemployment benefits and credit markets, in combination with lower initial economic stress in urban counties, may have limited the effect of the unemployment shock on drug mortality.

The main risk to identification comes from violations of the exclusion restriction. In particular, the Bartik-style instrument requires that counties with relatively larger (or smaller) excess drug mortality are not correlated with industries experiencing relatively larger (or smaller) unemployment shocks, when weighted by industry employment shares. I show that my instrument passes placebo checks before the pandemic. A violation of the exclusion restriction thus requires county-level dynamic confounders correlated with industry shares in 2019 or national industry employment changes, which affect drug mortality after 2020 through channels other than exposure to the national unemployment shock. My robustness checks suggest that the estimated effect strongly depends on the hospitality and food services industry. However, controlling for baseline employment shares in these industries yields unchanged results, thereby reducing the risk of bias.

This paper makes multiple contributions to the literature on the economics of the US drug epidemic and the relationship between labor markets and drug mortality.

First, my paper adds to the general literature on the relationship between economic shocks and drug-related mortality. The literature suggests a relation between short- and long-run economic decline and drug mortality but finds mixed results. Both correlational approaches (e.g., Carpenter et al., 2017; Hollingsworth et al., 2017; Seltzer, 2020) and quasi-experimental variation (e.g., Venkataramani et al., 2020; Pierce and Schott, 2020; O'Brien et al., 2022; Lowenstein, 2024) have been used to study the relation between economic shocks and drug mortality. My study is most similar in design to Venkataramani et al. (2020), who study the effect of automotive plant closures as an exogenous unemployment shock, as it examines the cumulative impact of an initial unemployment shock rather than a continuous relation between unemployment and drug mortality. Regarding empirical strategies, it is also related to Lowenstein (2024), who studies the effect of business cycle fluctuations on drug mortality using a shift-share instrument and finds no significant effect. My main contribution in terms of results is expanding the analysis of unemployment shocks to the context of the recent increase in drug overdoses during the COVID-19 pandemic and adapting a plausibly causal lens using the shift-share instrument. My estimates point to an insignificant effect on the population level but a significant effect in rural counties. The nonexistent national effect of the unemployment shock on drug mortality found in this paper has to be interpreted cautiously. Large federal and state-level extensions of unemployment benefits may have offset the effects, leading to a lower bound of estimated effects. This is in line with the previous literature, which has studied the mitigating effect of unemployment insurance on drug abuse (Wu and Evangelist, 2022; Guo and Peng, 2024; Martins et al., 2024; Wolf et al., 2024). I also find suggestive evidence for this channel, noting that rural US counties experienced barriers to taking up unemployment benefits. The estimated positive effect in rural counties, on the other hand, may constitute an upper bound for the expected effect of the unemployment shock on drug mortality outside of the COVID-19 pandemic due to simultaneous economic stressors and the unique nature of the COVID-19 shock. Even if the generalizability of these findings to non-pandemic contexts is limited, similar patterns of unemployment, limited healthcare access, uncertainty, and isolation can be observed in other contexts, such as previous pandemics, wars, and natural disasters.[3] Due to increasing risks of similar shocks (e.g., Marani et al., 2021), my findings provide a valuable blueprint for future policy responses.

More specifically, I add to the expanding literature on the causes of the US opioid and drug epidemic by providing the first plausible causal estimate of the impact of the COVID-19 unemployment shock on drug mortality. Maclean et al. (2022) review the literature up to 2021. The extent to which economic hardship was a cause of the initial drug epidemic is contested with various papers qualifying and quantifying potential causes of the epidemic (e.g., Case and Deaton, 2015; Ruhm, 2019; Chacon et al., 2021; Currie and Schwandt, 2021; Alpert et al., 2022; Langabeer et al., 2022). The recent increase in drug mortality since 2020 is largely unexplored. My findings strongly suggest that the employment disruption caused by the COVID-19 pandemic was not a first-order cause of the recent increase in drug mortality

[3] For instance, the 1918 Influenza Pandemic led to similar lockdown responses, creating social isolation and significant short-term economic declines (Barro et al., 2020). In the Syrian Civil War, economic and healthcare collapse likely significantly contributed to overall mortality (Marzouk et al., 2025). River erosions in Bangladesh are associated with loss of employment and limited healthcare access due to infrastructure deterioration (Hossain et al., 2024).

in the US on the national level. However, I find a significant effect in rural counties, which is particularly pronounced in socially vulnerable communities. With this finding, I provide quantitative evidence of urban-rural heterogeneity in response to economic shocks. Monnat (2019) has previously suggested that urban and rural counties are exposed to different drug abuse risk factors, with rural counties more sensitive to economic fluctuations. I add to these descriptive findings by investigating mechanisms focusing on pre-existing economic vulnerabilities and barriers towards accessing financial aid. In particular, my results support the hypothesis that economic stress may increase drug abuse in underserved communities, as suggested by, e.g., Monnat (2018), Chacon et al. (2021), Deas (2024), and Amaro et al. (2021).

Finally, my paper contributes to policy discussions on approaches to address the US drug epidemic. I find pre-pandemic economic vulnerabilities and barriers towards accessing unemployment benefits and credit in rural counties to be important factors affecting the translation of unemployment shocks to drug mortality during the COVID-19 pandemic. To the extent that these findings are generalizable to non-pandemic contexts, easing access to benefits and credit may help mitigate the risk of drug mortality in vulnerable communities. Additionally, any policies aimed at increasing community resilience and decreasing poverty may directly reduce the risk of drug mortality. Understanding the generalizability of my results to other contexts is a crucial step in gaining a deeper understanding of forward-looking policy to mitigate the US drug epidemic. Further, a better understanding of the supply side of the drug market is needed. The increasing share of lethal synthetic opioids in drug mortality is potentially a driver of urban drug mortality (Ciccarone, 2021) and suggests that policies aimed at reducing the supply of synthetic opioids may be important in tackling the drug epidemic.

The rest of the paper is structured as follows: Section 2 outlines the context around COVID-19 and drug mortality in the US, Section 3 motivates the framework of the paper in a simple model of drug demand and supply, Section 4 explains the empirical approach and introduces the data, Section 5 presents the main results for the estimated effect of the unemployment shock on drug mortality, Section 6 investigates mechanisms, while Section 7 concludes.

2 Background

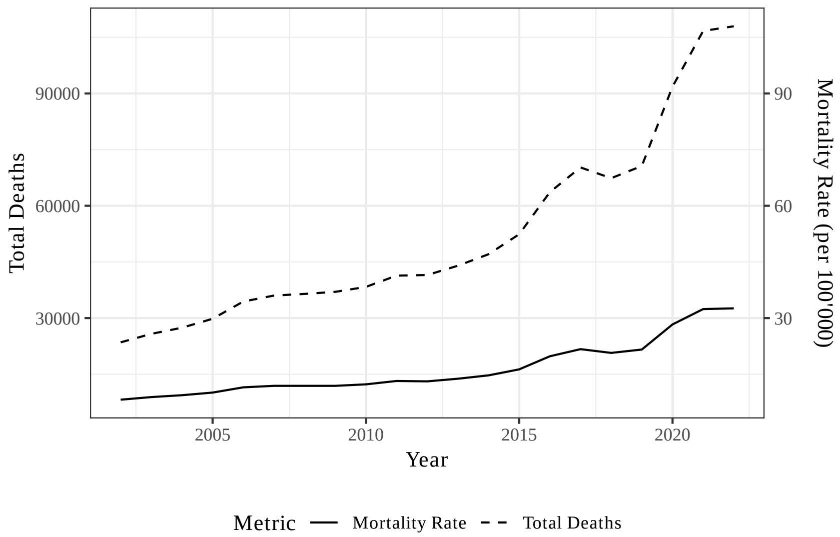
This section provides a brief overview of the development of the US drug epidemic over the last 20 years and US labor markets during the COVID-19 pandemic.

2.1 Drug Mortality in the US

I begin with a brief overview of the drug and opioid epidemic in the US. This section provides context and helps establish certain patterns for the stylized model in Section 3.

The US experienced a significant increase in drug mortality since the early 2000s, mainly driven by the so-called opioid epidemic (CDC, 2024). The overall development of drug deaths and drug mortality is illustrated in Figure 1. Appendix Figure A.1 additionally illustrates mortality from opioids, with specific considerations for heroin and synthetic opioids like fentanyl.

Figure 1: Total Drug Overdose Deaths and Mortality per 100,000 From 2002 to 2022



Notes: This figure shows total US annual drug overdose death counts and mortality per 100,000 from 2002 to 2022. Data source: Spencer et al. (2024)

From 2000 to 2010, the annual death rate by prescription opioids increased by more than a factor of 4. Starting from 2010 to the mid-2010s, the continuous rise in drug mortality was mainly driven by heroin. The rate of heroin deaths increased from approximately 0.9 per 100,000 population to 4.9 per 100,000 population. In 2018, heroin deaths started to decline, and synthetic opioids became the main driver of US drug mortality (Pergolizzi et al., 2021). Up to today, synthetic opioids, such as fentanyl, continue to be the leading drug-related cause of death. Other than opioids, stimulant drugs like methamphetamine and cocaine also saw increasing mortality rates. Fentanyl also affects these alternative drugs, with growing shares of cocaine and heroin containing fentanyl in testing (Lim et al., 2024). Finally, within the last wave, we see a change in trend with the beginning of the COVID-19 pandemic. Drug mortality has increased by approximately 50% from 2019 to 2022, reaching 108,000 deaths in 2022. For comparison, in the same year, approximately 42,000 motor vehicle accident deaths occurred in the US (NTAD, 2024). While this trend reversed in 2024, mortality remains elevated above pre-pandemic levels.

Various studies in economics and public health investigated the causes of the drug epidemic with a particular focus on the opioid epidemic. The opioid epidemic was initially fueled by the invention of OxyContin, a prescription opioid used as pain relief and aggressively marketed across the US (Maclean et al., 2022).[4]

The opioid epidemic evolved into a heroin epidemic in the early 2010s. The substitution from prescription opioids to heroin is hypothesized to have been caused by a reformulation of OxyContin, which made injecting the drug more difficult, and a regulatory crackdown on opioid prescriptions, such as increased prescription monitoring programs (e.g., Evans et al., 2019; Mallatt, 2022). Pill mill regulations, which heightened regulations for pain management clinics, have also contributed to a decrease in opioid dispensation and outcomes like opioid mortality. The evidence so far has not found an adverse spillover effect on heroin use (e.g., Meinhofer, 2016; Mallatt, 2022).

As illustrated in Appendix Figure A.1, the third wave saw an increasing share of synthetic opioids and a reduction of heroin as the main driver in drug mortality. Clear-cut empirical evidence about the causes of this change has become scarcer in recent years due to the more difficult approach towards finding exogenous variation and limited data availability. This recent change in the relative composition may be driven by supply-side factors (Ciccarone, 2021). Fentanyl is a very cheap and potent drug that can be used to stretch more expensive opioids (Mulligan, 2020). The heightened lethality compared to previous opioids may also explain the rise in overall death rates (Zoorob, 2019). However, it is not yet credibly established that this increase in mortality does not have significant other causes which coincide with the rise in fentanyl.

The causes of the final rise in drug mortality during the COVID-19 pandemic have not been studied in detail in the existing literature. As of today, there is little causal evidence quantifying the impacts of various potential causes. The increase in drug mortality per 100,000 population between 2019 and 2022, on a state level, is illustrated in Appendix Figure A.7. The recent increase has, to a varying degree, affected all US states.

In Appendix Figure A.5, I further illustrate average mortality on a county level when

[4] The effect of OxyContin's marketing on opioid exposure is, for example, shown in Arteaga and Barone (2025) who use court documents outlining OxyContin marketing, finding that marketing was concentrated in counties with higher cancer prevalence and showing subsequent strong links between marketing exposure and later year opioid outcomes. On the prescription side, Eichmeyer and Zhang (2022) compare assignments to physicians with a higher or lower opioid prescription tendency. They show that being assigned to a more prescription-friendly physician has a significant effect on subsequent opioid dependence. States with triplicate prescription policies made prescribing opioids more difficult and were less targeted by OxyContin's marketing (Alpert et al., 2022). These states continued to have slower growth in opioid deaths over the period from 2000 to the 2010s.

differentiating between rural and urban counties. Both urban and rural counties have seen an increase in drug mortality of similar magnitudes. Urban counties had higher pre-pandemic drug mortality rates, which persisted throughout the sample period. Additionally, urban counties had a higher share of synthetic opioid deaths prior to the pandemic. Both rural and urban counties saw a large increase in the share of synthetic opioid deaths during the pandemic, with urban counties' share remaining at elevated levels, as Appendix Figure A.6 illustrates.

2.2 COVID-19 and Labor Market Outcomes in the US

I next provide a brief, descriptive overview of the changes in US labor markets during the COVID-19 pandemic. The exposition provides context around the initial COVID-19 unemployment shock, focusing on differential mechanisms and effects across industries and characteristics affecting the generalizability of these shocks to other contexts. These descriptive findings motivate the empirical approach in Section 4.

The initial months of the COVID-19 pandemic gave rise to unprecedented labor market disruptions. In February 2020, just before the COVID-19 shock, unemployment was at 3.5%. Two months later, in April 2020, at the height of the initial unemployment shock, unemployment rose to 14.7%, the highest rate since the series was first recorded in 1948 (U.S. Bureau of Labor Statistics, 2020a). This unemployment shock was mainly driven by public health lockdown orders and subsequent firm-level decisions due to economic uncertainty. After the initial shock, employment slowly recovered, with unemployment levels of 6.7% at the end of 2020 (U.S. Bureau of Labor Statistics, 2020b). Industries focused on consumer interaction saw a weaker recovery due to a resurgence of COVID-19 cases in December 2020. 2021 saw robust job growth with a slight re-tightening of the labor market at the end of the year (U.S. Bureau of Labor Statistics, 2025). By March 2022, unemployment normalized at almost the pre-pandemic level, with a nationwide unemployment rate of 3.6%. By late 2022, total employment surpassed the pre-pandemic level, indicating a full recovery (Essien et al., 2023).

Appendix Figure A.2 shows the nationwide relative declines in annual employment from 2019 to 2020 on the 2-digit NAICS level. The initial labor market disruption affected nearly all industries, albeit to varying degrees.

Leisure and hospitality sectors were most strongly hit, with overall employment reducing by 7.7 million jobs at the peak of pandemic-induced job losses, an approximate 47% decrease (Bureau of Labor Statistics, U.S. Department of Labor, 2020). Most of these jobs were concentrated in the food/beverages industries as restaurants were forced to close due to social distancing mandates (Handwerker et al., 2020). Similarly, travel-related industries furloughed staff due to forced closures. Many of the workers were on temporary layoffs. Notably, the travel and leisure industry saw very persistent job losses. By late 2021, 4.9 out of 6 million jobs lost between February and April 2020 were recovered (Ramos, 2022). Workers in retail were similarly affected, and through a similar mechanism, although to a lesser degree. Specific retail segments experienced countervailing trends with increasing employment, while overall employment declined by 13.5% (Bureau of Labor Statistics, U.S. Department of Labor, 2020).

Healthcare saw among the sharpest and shortest unemployment shocks. Despite the additional strain on the healthcare system, many workers were initially furloughed due to the cancellation of elective procedures (Handwerker et al., 2020). Of 1.4 million jobs lost by April 2020, 60% were recovered five months later (Bureau of Labor Statistics, U.S. Department of Labor, 2020; Epstein and Sarna, 2021). Following the availability of COVID-19 vaccines, demand for elective procedures rebounded.

The oil & gas and manufacturing industries were indirectly affected by the pandemic. The global economic shock to demand and travel led to a sharp drop in oil demand, with oil prices briefly going negative. Lower oil prices caused oil companies to abort drilling and projects, leading to an approximate year-over-year employment loss of 15%. However, the unemployment shock was concentrated in June rather than April (U.S. Bureau of Labor Statistics,, 2025). Compared to other industries, oil had a relatively slow recovery. Despite rebounds in oil production and oil prices, 2022 employment remained below the pre-pandemic level (Ansell, 2023). Manufacturing was similarly confronted with delayed demand and supply chain shutdowns (Bureau of Labor Statistics, U.S. Department of Labor, 2022). 1.1 out of 1.4 million lost jobs were recovered by late 2021, yet supply-chain bottlenecks and the semiconductor shortage hindered a full recovery (Ramos, 2022). Construction and real estate saw similar initial declines, primarily due to uncertainty about the economic recovery leading to shutdowns of many construction projects. However, as construction was an essential industry, it recovered more quickly and saw a boom in late 2020 due to low interest rates fueled by pandemic monetary policy (Ramos, 2022).

More typically 'white collar' jobs, such as business and financial services, saw declines in employment in 2020, driven by economic uncertainty. These industries tended to recover quickly, with a full recovery by mid to late 2021 (Ansell, 2023). On the other hand, some industries, such as the technology sector, saw almost no job losses or quick recoveries, fueled by the accelerating transition to work-from-home and increasing demand for technology services due to the pandemic.

Due to the varying economic structures across different regions, the COVID-19 shock affected states differently. Nearly all regions were significantly affected by the initial unemployment shock. Selected states such as Minnesota and Nebraska saw far weaker unemployment shocks, with unemployment rising to approximately 8%. States focused on travel, notably Nevada and Hawaii, were among the hardest hit states and showed slightly elevated unemployment rates compared to the national average over the following two years but recovered by mid-2022. Within states, unemployment shocks had a more pronounced effect on cities than on rural areas (Cho et al., 2021). Urban counties were more heavily affected, with a larger share of urban populations reporting being unable to find work (Brooks et al., 2021).[5] The initial shock was also more persistent in urban counties, with average unemployment rates remaining elevated for longer over the initial years of the pandemic. On the other hand, rural counties faced a slower recovery at the end of the pandemic, with total rural employment returning to pre-pandemic levels almost a year after urban counties (Cho et al., 2021; Sanders, 2024). Appendix Figure A.3 and descriptive statistics in Appendix Table A.1 show that urban counties experienced a larger and initially more persistent unemployment shock, a higher share of workers in the most affected industries, such as leisure and hospitality, a lower share of workers in less affected industries, such as agriculture, and less concentrated labor markets in terms of industry employment.

The unemployment shock may be described as exogenous to the economy, as it was primarily policy-induced by public health measures as opposed to general business cycle fluctuations or a typical recession induced by economic shocks. Both in terms of the spread of affected industries and the sharp timing of the shock together with a rapid recovery and the large use of temporary layoffs, the COVID-19 shock is unique compared to typical, cyclical unemployment or structural unemployment shocks, which were often studied in previous papers (e.g., Hollingsworth et al., 2017; Venkataramani et al., 2020; Lowenstein, 2024).

All in all, various industries with different underlying characteristics and distribution across regions in the US were affected by the COVID-19 unemployment shock. The recovery was uneven across industries, with some recovering within a few months while others continued to see elevated unemployment rates two to three years after the initial shock. Pandemic-induced job losses disproportionally affected less educated, younger, and lower income groups – both in terms of affected industries and within industries (Handwerker et al., 2020; Seitelman et al., 2020).

[5] However, urban economies had more options for telework abilities which cushioned the economic shock to some degree.

3 A Model of Drug Supply, Demand, and Mortality

In this section, I set up an illustrative 'toy model' of drug supply and drug demand, which models drug mortality as an outcome. Before outlining the model, I will quickly illustrate potential mechanisms linking stress to drug abuse. I will then sketch my model focusing on two distinct markets, urban U and rural R. I assume that drug demand depends on an initial propensity to consume, prices, and an (economic) stress variable. On the supply side, I model a domestic supplier (in each market) and a foreign supplier who decides whether to enter each market. The foreign and domestic suppliers' drugs are indistinguishable from the consumer's viewpoint. Importantly, however, the drugs differ in their lethality. I model mortality in each market as a random variable depending on the quantity of each drug type consumed.

3.1 Mechanisms Linking Stress to Drug Abuse

The link between stress and drug abuse is well-established in the medical literature. Stress is associated with increased drug use and abuse (e.g., Sinha, 2001). Stress activates the corticotropin-releasing factor (CRF), which produces negative emotional states such as anxiety (Sinha, 2008; Koob and Le Moal, 2008). It is suggested that stress may increase susceptibility to drug consumption through the activation of the hypothalamic-pituitary-adrenal (HPA) axis, which regulates the body's response to stress. Changes in the HPA axis affect the brain's reward system and can augment positive effects associated with drug consumption, such as feelings of pleasure or stress relief (Nikbakhtzadeh et al., 2023). As a result, drug consumption may serve as a coping mechanism to alleviate stress.

Economic stress is a specific form of stress that is encompassed within the broader literature discussed above. Additionally, there has been a growing body of literature in recent years specifically examining economic distress. The literature indicates that economic stress can be a useful predictor for drug abuse and that unemployment impairs mental health (e.g., Paul and Moser, 2009; Glei and Weinstein, 2019; Amaro et al., 2021).[6]

Additionally, unemployment may lead to changes in social support networks, which can further exacerbate drug use and abuse. There is ample evidence in the medical literature that social support is a crucial factor influencing drug abuse outcomes (e.g., Bond et al., 2003; Polcin and Korcha, 2017) and social interaction can be a substitute for drug use, at least in experimental evidence involving laboratory rats (Venniro et al., 2018).

[6]See also the other papers discussed in the introduction of this paper.

3.2 Model Setup

Demand. I consider two separated markets, $i \in \{U, R\}$ where U is the *urban* and R the *rural* market. My simplified model assumes that aggregate drug demand in each market depends on an intrinsic propensity to consume drugs, a stress variable, and the market price, assuming a downward-sloping demand function. The rationale for this modelling choice comes from the above-discussed link in the medical literature between stress and drug abuse.

Bypassing the aggregation of individual utilities into market demand,[7] I model aggregate demand for each market i as

$$Q_i = \alpha_i + \beta_i S_i^2 - \gamma p_i, \tag{3.1}$$

where $\alpha_i > 0$ measures intrinsic propensity to consume at zero stress and zero prices, S_i is the stress stock variable, $\beta_i > 0$ measures the demand response to the stress stock, and p_i is the drug price. The stress term is included quadratically to model a nonlinear increase in drug consumption in response to the stress. A small increase in economic stress may not lead to drug abuse on its own but may do so when stress levels are already high (e.g., due to food insecurity).

I further implicitly model a link between unemployment shocks, moderating factors, and the stress index as follows:

$$S_i = \bar{S}_i + f\left(\Delta E_i, UB_i, CC_i, Pov_i\right) \tag{3.2}$$

The aggregate stress index S_i depends on an initial level $\bar{S}_i$ and a function which relates changes in employment ΔE_i, access to unemployment benefits UB_i, credit constraints CC_i, initial poverty, Pov_i, and social support networks $Social_i$ to changes in the stress index. This general functional form allows for differential effects of unemployment shocks on aggregate stress. In general, I posit that gaining employment reduces stress and losing employment increases stress, $\partial S_i / \partial \Delta E_i < 0$. The other factors can moderate this effect. In particular, during the COVID-19 pandemic, large unemployment benefit expansions may have been moderating factors, reducing the stress effect of unemployment. Similar results may hold for access to credit markets. At the same time, larger initial poverty increases stress associated with short-term income disruptions, and I also posit that the translation of stress into drug abuse depends on social support networks; that is, β_i is a function of a measure of social support with

[7] Note that this step eases the modelling but imposes an implicit assumption that changes in the stress variable do not affect the consumer's budget constraint, which is unlikely to hold in the case of unemployment shocks. However, the observed empirical patterns suggest that drug consumption instead increases in response to economic shocks, potentially indicating a very low or near-zero demand elasticity with a stress effect dominating price effects.

$\partial\beta_i/\partial Social_i < 0$. The public health literature generally supports these mitigating factors and, in particular, a stronger link between unemployment shocks and stress in financially constrained communities (Mathieu et al., 2022).

Supply. The supply side consists of a local supplier of drugs for each market, d_i, and one foreign supplier, f, who decides whether to enter each market. While the products of both suppliers are indistinguishable from a consumer's viewpoint, they differ in their respective lethality, δ_d and δ_f, as discussed below. This framework models the empirically observed presence of a local opioid supply and imports of fentanyl in a simplified way.

The profit of each local supplier is given by

$$\pi_i^d = (p_i - c_i^d)q_i^d \tag{3.3}$$

where q_i^d is the quantity supplied by the domestic supplier, p_i is the market price in market i, and $c_i^d \geq 0$ is the marginal cost for the local supplier in market i.

For the foreign supplier, the profit in market i is zero if the firm decides not to enter the market. Otherwise, it is given by

$$\pi_i^f = (p_i - c_i^f)q_i^f - F_i \tag{3.4}$$

where F_i is the fixed cost of entering market i, and the other terms are analogous to the above. Note that $Q_i = q_i^f + q_i^d$ for each market, and there are no strategic interactions between the urban and rural markets so that I can treat the markets separately. If entry occurs, the suppliers compete in quantities (Cournot competition).

Mortality. Finally, mortality is modelled as a probabilistic outcome. Domestic drug mortality is assumed to depend deterministically on the lethality factor of the domestic drug, δ_d, and the quantity of the domestic drug consumed in a given market. The mortality of the foreign, lethal drug also depends on its lethality factor, δ_f, and quantity consumed but is additionally exposed to multiplicative shocks captured in the random variable η with an expected value of 1, $\mathbb{E}\left[\eta\right] = 1$, and non-zero variance, $\text{Var}(\eta) = \sigma^2 > 0$. This randomness captures the fact that highly lethal drugs can distort the usual mortality patterns of drug abuse and are thus unpredictable. For instance, a first-time use of fentanyl may already be lethal, and due to the potency and density of the drug, there is a higher risk of unintentional drug consumption. These factors are captured in the multiplicative noise term. Overall mortality is thus modeled as

$$M_i = \delta_d q_i^d + \delta_f q_i^f \eta. \tag{3.5}$$

3.3 Equilibrium

In equilibrium, each supplier chooses quantities to maximize its profit, taking the other supplier's quantity and market demand as given. Prices adjust to clear markets.

For each market, multiple potential equilibria can occur: no entry, entry deterrence through discriminatory pricing, or entry and competition. Given our observation that both fentanyl and non-fentanyl drugs are prevalent in both markets, we focus on the entry equilibrium. In this equilibrium, the equilibrium quantities are then given by

$$q_i^d = \frac{\alpha_i + \beta_i S_i^2 - 2\gamma c_i^d + \gamma c_i^f}{3}, \quad q_i^f = \frac{\alpha_i + \beta_i S_i^2 - 2\gamma c_i^f + \gamma c_i^d}{3}. \tag{3.6}$$

Proposition 1 in Appendix C formally states and proves the conditions under which the foreign firm enters the market and the conditions under which both firms produce positive quantities. The proof also derives the above equilibrium quantities.

In the context of the COVID-19 drug supply shock, the increase in fentanyl can be rationalized by increasing the marginal cost of the domestic supplier. Domestic suppliers faced supply chain disruptions due to border closings and other disruptions to the usual provision of drugs. Fentanyl was less affected by these changes due to its high potency. This makes smuggling relatively easier than the transportation of input goods in the production of other drugs (Kolodny, 2021). From equation (3.6), we can see that, ceteris paribus, a decrease in c_i^f relative to c_i^d increases the market share of the more lethal foreign drug. At the same time, the larger increase in fentanyl exposure in urban counties fits our model by assuming that marginal costs for the foreign supplier are higher in rural relative to urban markets, i.e., $c_U^f < c_R^f$. Higher marginal costs of drug provision in rural counties seem plausible due to larger transportation costs associated with lower population density markets.

3.4 Effect of an Economic Stress Shock on Drug Mortality

I now consider comparative statics. The focus is on the effect of the unemployment shock on drug mortality. From (3.6), a marginal increase in stress increases equilibrium quantities by

$$\frac{dq_i^d}{dS_i} = \frac{2\beta_i S_i}{3}, \quad \frac{dq_i^f}{dS_i} = \frac{2\beta_i S_i}{3}. \tag{3.7}$$

As previously poorer, rural counties likely faced higher pre-existing economic hardship, rural baseline stress $\bar{S}_R$ may be higher than in urban counties ($\bar{S}_U$). Consequently, even if the increase in stress were the same across counties, the resulting increase in drug demand and subsequent mortality would be larger in these rural counties compared to urban ones, as can be seen from the partial derivatives (3.7).

Additionally, my model posits that the unemployment shock translates to a larger increase in stress in poorer and credit-constrained counties, suggesting that the increase in stress itself was also larger in rural counties. That is,

$$\left|\frac{dS_R}{d\Delta E}\right| > \left|\frac{dS_U}{d\Delta E}\right| \tag{3.8}$$

Therefore, an equivalently sized unemployment shock leads to a larger increase in drug consumption in rural relative to urban counties, a prediction which is in line with empirical findings (e.g., Monnat, 2019).

Changes in drug consumption have a corresponding effect on expected drug mortality:

$$\mathbb{E}\left[\frac{dM_i}{dS_i}\right] = \delta_d \frac{dq_i^d}{dS_i} + \delta_f \frac{dq_i^f}{dS_i} = \frac{2\beta_i S_i}{3}\left[\delta_d + \delta_f\right]. \tag{3.9}$$

It is a priori unclear whether the effect on drug mortality is larger in urban or rural markets. While there is a larger increase in consumption in rural areas, under our assumption of $c_U < c_R$, urban markets have a higher share of more lethal foreign drugs. The expected effect on drug mortality may be more or less pronounced depending on the exact specifications of the relation between unemployment shocks and stress variables, as well as other demand parameters and the relative drug supply.

Notably, while equation (3.9) shows that we expect to see a positive effect of the stress shock on drug mortality, the strength of the effect and the ability to detect such an effect depends on η, i.e., on the relative signal-to-noise ratio. The signal-to-noise ratio diminishes as the quantity supplied by the foreign firm increases. Under our assumption of lower marginal costs for the foreign supplier in urban markets, the noise is higher in urban than rural markets. This increased noise makes it harder to detect any pattern empirically.

To summarize, in my model of endogenous drug supply, under a plausible assumption of lower marginal costs of drug supply in urban markets, we obtain the observed pattern of a larger increase in imported, highly lethal drugs in urban relative to rural markets. Comparative statics imply that an unemployment shock increases economic stress and drug mortality in urban and rural counties. There are three important considerations: (i) both higher initial stress in rural counties and constraints which strengthen the effect of the unemployment shock on the stress index lead to a larger effect on drug consumption in rural counties compared to urban counties, (ii) the effects of the stress shock on mortality depend on the drug mix and explicit parameter constellations and may be more pronounced in either urban or rural markets, (iii) a larger presence of highly lethal foreign drugs in the urban markets decreases the signal-to-noise ratio, making the effect more challenging to detect in urban markets.

Importantly, in my model, the effect of unemployment on economic stress is lower when access to unemployment benefits is easier, private credit can be more easily obtained, and initial economic hardship is smaller. At the same time, the translation from stress into drug abuse is moderated by social support structures. My model thus creates implicit predictions about heterogeneity dimensions for my subsequent analysis, which will be explored in Section 6.

4 Empirical Strategy

This section describes the empirical strategy employed to estimate a plausibly causal effect of the COVID-19 unemployment shock on drug mortality in the US. I begin by outlining my data sources and then describe potential sources of endogeneity in reduced-form regressions. Following this, I outline the construction of my instrument and my main empirical specification. I also discuss the exclusion restriction of my instrument and provide suggestive evidence for its validity and the strength of the instrument.

4.1 Data and Descriptive Statistics

I introduce my various data sources and provide initial descriptive statistics.

My key outcome variable of interest is drug mortality. I obtain a panel dataset of annual county-level drug deaths from CDC Wonder from 2018 to 2023. Drug deaths are defined based on ICD-10 underlying cause-of-death codes using X40-44, X60-64, X85, and Y10-Y14.[8] For my main specifications, I use a county-level data set. This is in line with many recent studies of opioid mortality (e.g., Yang et al., 2023; Martins et al., 2024) and has certain advantages over the consideration of commuting zones.[9],[10]

[8]This is the definition also used by the CDC itself for its reports on drug mortality (CDC, 2024).

[9]Commuting zones are groups of counties with strong labor mobility links between them and thus often serve as a better measurement of a labor market than individual counties (Tolbert and Sizer, 1996; Com, nd). As COVID-19 policy was driven mainly on the national and state level and many commuting zones overlap multiple states, I cannot include state-year fixed effects, which would absorb variation driven by state policy. Further, commuting behaviour has been strongly disrupted during the pandemic, which is likely to induce noise, and recent papers suggest that drug mortality hotspots are better defined at the county level (e.g., Srinivasan et al., 2025).

[10]I exclude the states of Connecticut and Alaska from my analysis as both states underwent redefinitions of

For unemployment information, I obtain quarterly and annual county and nationwide industry-level employment data for 2019 and 2020 from the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW). I define an industry based on the 3-digit NAICS classification.[11] I further obtain monthly, county-level unemployment information from the BLS' Local Area Unemployment Statistics.

I enrich my data with various demographic, economic, and healthcare structure controls. I include longer-term annual trends in unemployment and labor force participation on the county level, using data from the Economic Research Service of the US Department of Agriculture. From the US Census Bureau, I obtain county-level data on age, population-age distributions, ethnicities, urban-rural population shares, and further demographics. I add data on median income and poverty estimates from the Small Area Income and Poverty Estimates Program (SAIPE). I further obtain education data, as well as other economic indicators such as housing cost burden, from the five-year American Community Survey and data on healthcare access (e.g., physician density) from the area health resource files by the Health Resources & Services Administration and the Center for Medicare & Medicaid Services information on national providers. As qualitative measures of healthcare access, I additionally consider various indicators from the CDC PLACES program, which estimates county-level incidence of mental distress and other health measures based on the Behavioral Risk Factor Surveillance System (BRFSS). To measure social cohesion, I add data on the number of social associations based on County Business Patterns. I add measures of food insecurity based on Feeding America's Map the Meal Gap data. Finally, I include data on the number of drug prescriptions per capita from the CDC's Opioid Prescribing Rate Maps.

A list including all variables used as controls and their sources is available in Appendix B.I. Additional variables used for heterogeneity analyses and other outcomes are listed in Appendix B.II. I use the 2019 data as my main pre-pandemic data. For some variables, I use the most recent available data prior to the pandemic or the 2020 data if data is infrequently updated.

Much of my paper will focus on the differences between urban and rural areas. Therefore, I provide descriptive statistics of key socioeconomic variables in urban and rural counties in Table 1. Urban and rural counties differ significantly in their socioeconomic structure. Urban counties are larger, have a higher population density, and are more diverse. They also have a lower share of individuals with less than a high school degree, significantly higher median

counties in 2020 and 2019, respectively. This redefinition prohibits me from tracking all the relevant outcomes and control variables over the sample period.

[11] For expositional purposes, some figures and statistics also use a 2-digit NAICS classification.

incomes, and a lower share of impoverished individuals. The healthcare structure is also different, with urban counties having more physicians per capita and a higher density of mental health services providers.

Table 1: Descriptive Statistics of Socioeconomic Variables in Urban and Rural Counties

	Rural	Urban	Difference
Panel A: Demographics			
Median Age	42.69	40.07	-2.61***
Pct. With Less Than High-School Degree	12.51	10.19	-2.32***
Population Density	42.51	677.06	634.54***
Panel B: Economic Characteristics			
Median Household Income (k USD)	50.81	64.05	13.24***
Poverty Pct.	15.65	12.40	-3.25***
Unemployment rate	4.20	3.93	-0.27***
Social Vulnerability Index	0.53	0.46	-0.07***
Panel C: Healthcare Access			
Physician Density	0.09	0.18	0.09***
Mental Health Provider Density	152.19	194.71	42.52***
N	1974	1165	
Total Population (million)	46.04	282.26	

Notes: Rural counties are defined as nonmetro counties based on Rural-Urban continuum codes. Values are averages for 2019 or the most recent pre-pandemic value, wherever available. Data sources in Appendix B.I. The last column shows the difference between average values of rural and urban counties with stars indicating the p-value of a t-test testing the null hypothesis that the means of rural and urban counties are equal. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

4.2 Issues in Reduced-Form Regressions

The goal of this paper is to estimate the causal effect of the COVID-19 unemployment shock on drug mortality. For this, I will start by estimating regressions of the following form:

$$\text{Overdose}_{ct} = \alpha + \beta \Delta \text{Unemp}_c + \theta^T \mathbf{X}_c + \gamma_{s,t} + \epsilon_{ct} \tag{4.1}$$

where c indexes counties, s indexes states, and t indexes years. Overdose_{ct} is the drug overdose death rate per 100,000, ΔUnemp_c is the unemployment shock,[12] $\mathbf{X}_c$ is a set of county-level controls and $\gamma_{s,t}$ are state-year fixed effects.

[12]For most analyses, I define the unemployment shock as the change in unemployment rates between February 2020 and the average unemployment rate from April to June 2020.

Suppose $\widehat{\beta}$ is the estimated coefficient on the unemployment shock when taking (4.1) to the data. $\widehat{\beta}$ is a valid estimate of the causal effect of the unemployment shock on drug mortality if the following condition holds:

$$\mathbb{E}\left[\epsilon_{c,t} \mid \Delta\text{Unemp}_c, \mathbf{X}_c\right] = 0.$$

This condition is likely violated. In particular, previous literature has studied the effects of drug abuse on many labor market outcomes, such as labor force participation, which implies that labor markets and drug abuse are bi-directionally linked (e.g., Harris et al., 2020; Kopecky et al., 2022). While the COVID-19 shock, primarily caused by federal mandates, is plausibly exogenous, there are remaining concerns that reverse causality could influence the result. If drug abuse risk lowers productivity, more at-risk counties may experience larger layoffs. At the same time, higher unemployment shocks may also be correlated with more stringent COVID-19 policies, which may reduce or increase drug abuse, e.g., due to changes to the drug supply, highlighting potential simultaneity concerns.

As shown in Appendix Figures A.9 and A.10, depending on the extent of controls included, the actual COVID-19 unemployment shock is a significant predictor of pre-pandemic drug mortality rates. The alternative instrument I propose in the next section passes the placebo check. This pattern implies that localities suffering greater excess job losses relative to the national industry-based predictions already had elevated drug mortality risks—likely reflecting underlying labor force vulnerabilities which are potentially tied to drug use, as documented in prior research. Relying on the realized unemployment shock in a reduced-form estimation risks confounding the effect of COVID-driven job losses with unobserved, pre-existing labor market characteristics. These characteristics and local vulnerabilities are hard to control for.

4.3 Instrumental Variable

Given the above-mentioned concerns when estimating a reduced-form regression, I instead propose the use of a Bartik-style shift-share instrument. The fundamental rationale behind this approach is that pre-pandemic industry composition is a strong predictor of the initial COVID-19 unemployment shock, and national changes in employment in industries should average out unobservable risk factors of drug abuse which may be correlated with county-specific unemployment shocks.

I construct my shift-share instrument Z_c for each county c as follows:

$$Z_c := -\sum_i \left(\frac{E_{ic,2019}}{E_{c,2019}} \right) \times \frac{\Delta E_i^{(N)}}{E_{i,2019}^{(N)}} \tag{4.2}$$

Here, $E_{ic,2019}$ is employment in industry i in county c in 2019, $E_{i,2019}^{(N)}$ is national employment in industry i in the second quarter 2019, and $\Delta E_i^{(N)} = E_{i,2020}^{(N)} - E_{i,2019}^{(N)}$ is the change in national employment for industry i from the second quarter of 2019 to the second quarter of 2020.

To estimate the causal effect of the unemployment shock on subsequent drug overdose death rates, I employ a two-stage least squares setup with first-stage regression

$$\Delta \text{Unemp}_c = \alpha + \beta Z_c + \theta^T \mathbf{X}_c + \gamma_s + \zeta_c \tag{4.3}$$

whereby ΔUnemp_c is the unemployment shock in county c, measured as the difference in the unemployment rate between the average from April to June 2020 and February 2020, $\mathbf{X}_c$ is a vector of county-level controls and γ_s is a state-level fixed effect. The second stage regression is given by

$$\text{Overdose}_{ct} = \alpha + \beta \Delta \widehat{\text{Unemp}}_c + \theta^T \mathbf{X}_c + \gamma_{s,t} + \epsilon_{ct} \tag{4.4}$$

where Overdose_{ct} is the drug overdose death rate per 100,000 in county c in year t, $\gamma_{s,t}$ is a state-year fixed-effect, and $\Delta \widehat{\text{Unemp}}_c$ is the predicted unemployment shock from the first stage regression. I weight observations by population size to account for differences in population size across counties, thereby measuring the effect on drug mortality per capita.

In most specifications, I use a stacked regression approach where each county has multiple observations, one for each year.[13] This allows for estimating a single shared coefficient $\hat{\beta}_i$ across all years for each county, increasing statistical power under the assumption that effects are consistent across the periods. I additionally test for separately estimating coefficients in each year in an event-study specification and for dynamic controls. I cluster standard errors at the state level to forego issues of downward bias in small clusters when clustering at the county level c and to allow for more conservative spillovers and correlation patterns in residuals.

Furthermore, I will use this two-stage setup in a heterogeneity analysis. To estimate heterogeneous effects, I include interaction terms between the instrument Z_c and the heterogeneity variable H_c in the first stage, and between the unemployment shock and the heterogeneity variable in the second stage. That is, I instrument for $\Delta \text{Unemp}_c + \Delta \text{Unemp}_c \times H_c$ using

[13] Both the instrument as well as most controls, except for dynamic COVID-19 controls and state-year fixed effects, are fixed at baseline, while the target variable changes from observation to observation.

$Z_c + Z_c \times H_c$.

4.4 Identification

The usual identification assumption in an IV regression is given by

$$\mathbb{E}\left[\epsilon_{c,t} Z_c\right] = 0$$

As Borusyak et al. (2025) and Goldsmith-Pinkham et al. (2020) highlight, identification in the context of a Bartik-style instrument can be viewed as either coming from random shocks to different industries or random assignment of industry shares to different counties. I take the *random shocks* view in this paper. In this case, the identifying assumption can be relaxed to the shifts being uncorrelated with an average of the share-weighted unobservable errors, $\epsilon_{c,t}$. Formally, for all industries i,

$$\mathbb{E}\left[g_i \sum_c s_{ic} \epsilon_c\right] = 0, \tag{4.5}$$

where g_i is the national employment shift for industry i, s_{ic} is the employment share of industry i in county c, while ϵ_c is the unobservable error term in county c.

The randomized shares interpretation is unlikely to be fulfilled. County-level industry concentration measures many simultaneous changes in drug and COVID policy and drug mortality risk. For instance, influential papers on the opioid pandemic highlight the "deaths of despair" phenomenon of higher opioid mortality in former manufacturing locations (e.g., Case and Deaton, 2015). While this does not immediately rule out the randomized shares interpretation, as this affects levels rather than changes, it makes the argument for randomized assignment of industry shares to counties seem implausible.

To motivate the identification assumption of the random shock view, I first illustrate the ideal identification scenario: assume that unemployment shocks are randomly assigned to different industries in a national lottery. In this case, even if the industry shares are correlated with drug abuse mortality (or risk of stronger reactions to economic shocks with respect to drug abuse), randomisation of unemployment shocks across industries will yield a valid causal estimate.

The COVID-19 unemployment shock provides a scenario in which the assignment of unemployment shocks to industries is plausibly random, as there are various ways in which industries of very different characteristics may have seen a large increase in unemployment. As previously discussed and illustrated in Appendix Figure A.2, which shows the relative decrease in unemployment for different NAICS industry codes, various industries were exposed to the COVID-19 unemployment shocks. In Section 2.2, I also highlight different mechanisms by which industries were exposed to the unemployment shock: various services industries, such as hospitality, saw some of the largest shocks due to forced closings in response to social distancing measures. Arts and recreational services saw rising unemployment due to social distancing measures which led to a halt for various activities and overall shrinking demand due to uncertainty. However, other industries, like real estate, also saw relatively large unemployment spikes due to financial and economic uncertainty. This diversity in mechanisms lends credence to some plausible exogeneity of the unemployment shocks across industries.

Nevertheless, some industry characteristics may directly influence drug abuse mortality, leading to endogeneity problems. In particular, unemployment shocks more strongly affected lower-education workers. Due to these potential endogeneity concerns, I must also address non-randomness due to the assignment of industries to counties. My identification assumption in equation (4.5) requires that industries with relatively high (or low) unemployment shocks are not concentrated in counties with relatively high (or low) unobservable errors. As an example of a potential violation, consider the case of a county with relatively low average educational attainment and a large concentration of workers in an unemployment-exposed industry. Assuming lower education is a direct risk for opioid mortality, this leads to endogeneity.[14] However, I can address many such potential confounders via the inclusion of adequate controls on the shift- and unit-level, such as controlling for educational attainment. I find further support for my assumption in the literature. Srinivasan et al. (2025) find different opioid mortality in different census tracts depending on industry employment but fail to find a time- and place-invariant relationship between the industry employment and drug mortality.[15]

4.5 First-Stage Evidence

I provide evidence of the instrument's strength and suggestive graphical evidence that rules out the importance of static confounders.

Table 2 summarizes the estimated first-stage regression. As is usually the case for shift-share instruments, my first-stage results are very strong with F-statistics far in excess of the rule of thumb level of 10, both when including and excluding a set of controls and state

[14] In general, lower educational attainment is associated with higher drug abuse risk (e.g., Gauffin et al., 2013). It is less clear whether education has a direct or indirect effect on drug abuse, e.g., through social isolation or poverty.

[15] Srinivasan et al. (2025) compare opioid overdose rates in different census tracts in Kentucky and Massachusetts. They find a higher mortality rate in manufacturing in Massachusetts but the opposite effect in Kentucky. Similarly, construction showed a significantly higher effect pre-2020, but the effect vanished after the pandemic. Arts has a significantly higher risk in Massachusetts but no effect in Kentucky.

fixed effects. This is largely unsurprising, given the large nationwide changes in industry employment, which often have a unified mechanism across states, as previously outlined.[16]

Table 2: First-Stage: Regressing Unemployment Shock on Instrument

	(1)	(2)
Predicted Unemployment	0.333***	0.161***
	(0.063)	(0.029)
N	9308	9305
R^2	0.63	0.63
Within R^2	0.14	0.3
Controls	No	Yes
State FE	Yes	Yes
F-stat	1127.94	923.43

Notes: In all regressions, the dependent variable is the 2020 unemployment shock. Column (1) includes state-year fixed effects while column (2) includes state-year fixed effects and the full set of controls as specified in Appendix B.I. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Further, Appendix Figure A.9 shows that regressing the instrument on various demographic characteristics and pre-pandemic drug mortality yields relatively small R^2 values and no significant coefficient on 2019 drug mortality. On the other hand, repeating this exercise with the annual county-level unemployment change from 2019 to 2020 yields a significant coefficient on pre-pandemic drug mortality and a far higher R^2 value, indicating that county characteristics and potential risk factors for drug mortality can explain a relatively large fraction of the realised unemployment shock. The instrument manages to average out these effects. This provides suggestive evidence that static pre-pandemic factors do not confound the instrument. There are some differences in observable characteristics which will be controlled for in my preferred specification. The validity of the instrument in the face of potential static pre-pandemic confounders is supported by a falsification exercise prior to 2020 in Figure A.8. Importantly, including state-year fixed effects absorbs any variation from time-varying shocks at the state level. This provides evidence against static confounders and rules out state-level dynamic confounders. Thus, violations of the exclusion restriction must arise from dynamic variation between counties of a given state correlated with the initial instrument. I discuss the possibility of dynamic confounders in more detail in the following sections.

[16] Note that as I run a stacked regression, the F-statistics may be somewhat overstated since each county has a unique value for the instrument across all three observations. However, running separate regressions for each year still yields strong F-statistics exceeding 300.

5 Effect of COVID-19 Unemployment on Drug Mortality

This section presents and discusses the empirical findings of the effect of the COVID-19 unemployment shock on drug mortality in the US. After presenting the main results, I estimate my model separately in rural counties. I also show the results of various robustness checks.

5.1 Effect of Unemployment Shock on Drug Mortality, Urban and Rural Counties

I present the main results from estimating equation (4.4) in Panel A of Table 3.

Table 3: Impact of Unemployment Shock on Drug Mortality

	OLS	OLS	Reduced Form	Reduced Form	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Complete Sample						
Unemployment Shock	0.457**	0.102	-	-	-0.088	-0.144
	(0.183)	(0.159)	-	-	(0.268)	(0.230)
Predicted Unemployment	-	-	-0.029	-0.053	-	-
	-	-	(0.089)	(0.085)	-	-
N	9308	9305	9308	9305	9308	9305
R^2	0.69	0.76	0.68	0.76	0.68	0.76
Within R^2	0.56	0.66	0.56	0.66	0.55	0.66
First Stage F-stat	–	–	–	–	1127.94	1954.16
Panel B: Rural Sample						
Unemployment Shock	0.416*	0.400***	-	-	0.448	0.902***
	(0.219)	(0.147)	-	-	(0.320)	(0.264)
Predicted Unemployment	-	-	0.136	0.228***	-	-
	-	-	(0.091)	(0.066)	-	-
N	5843	5840	5843	5840	5843	5840
R^2	0.51	0.63	0.51	0.62	0.51	0.62
Within R^2	0.24	0.42	0.24	0.41	0.24	0.41
First Stage F-stat	–	–	–	–	1460.77	834.33
Controls	No	Yes	No	Yes	No	Yes
Time frame	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects. The full set of controls is specified in Appendix B.I. Columns (1) and (2) show estimated coefficients when regressing the dependent variable on the unemployment shock. Columns (3) and (4) represent reduced-form results of regressing the dependent variable on the instrument. In columns (5) to (6), the unemployment shock is instrumented by the shift-share instrument. Observations are weighted by population size. Panel A is estimated in the complete sample, while in Panel B the sample is restricted to rural counties, defined as non-metro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

I find no effect of unemployment shock on drug mortality when weighting observations by population size, both in a direct regression (Column 2) and in the instrumental variable regression (Column 6), when including my full set of controls. This result implies that for the average, complier US citizen, the unemployment shock has not increased drug mortality risk. Notably, the result strongly depends on including an adequate set of controls. Column (1) repeats the simple regression without including various demographic, economic, and drug consumption controls and does find a significant effect.

These average results may mask stronger effects within subsamples of counties. I next turn to estimating the model when restricting my data to rural counties. The results are in Panel B of Table 3. The rationale for this approach is that rural and urban counties may be exposed to unemployment shocks and drug mortality in different ways (e.g., Monnat and Rigg, 2018). In particular, larger pre-existing economic distress and longer-term economic declines affecting rural areas may exacerbate the risk of unemployment shocks leading to drug consumption. Due to 80% of the US population living in metropolitan areas, these different effects and mechanisms may be diluted.

I find a significant effect of the unemployment shock on drug mortality when restricting my data to rural counties. My preferred specification in column 6 includes a full set of controls and state-year fixed effects while estimating a single coefficient for the annual effect over the three calendar years 2021, 2022, and 2023. This time frame uses all available calendar years post-shock and is in line with expected effect times.[17] Similar results hold for the reduced-form regression in Column (4) and the direct OLS regression in Column (2). The reduced form and IV regressions without my set of controls in Columns (3) and (5) yield an attenuated and insignificant estimated effect, indicating negative confounding. Pre-existing factors, as well as the unemployment shock itself, are likely correlated with drug mortality risk. The observed pattern indicates that counties with a larger unemployment exposure were simultaneously counties which were less likely to experience a large increase in drug mortality in the absence of the unemployment shock, leading to the suppressed effect before controlling for these differential characteristics.[18]

In Figure 2, I employ an event-study setup of my reduced-form regression, where I esti-

[17]E.g., Powell et al. (2023) find that an effect of some initial shock on subsequent drug overdoses tends to occur 2-3 years post-shock.

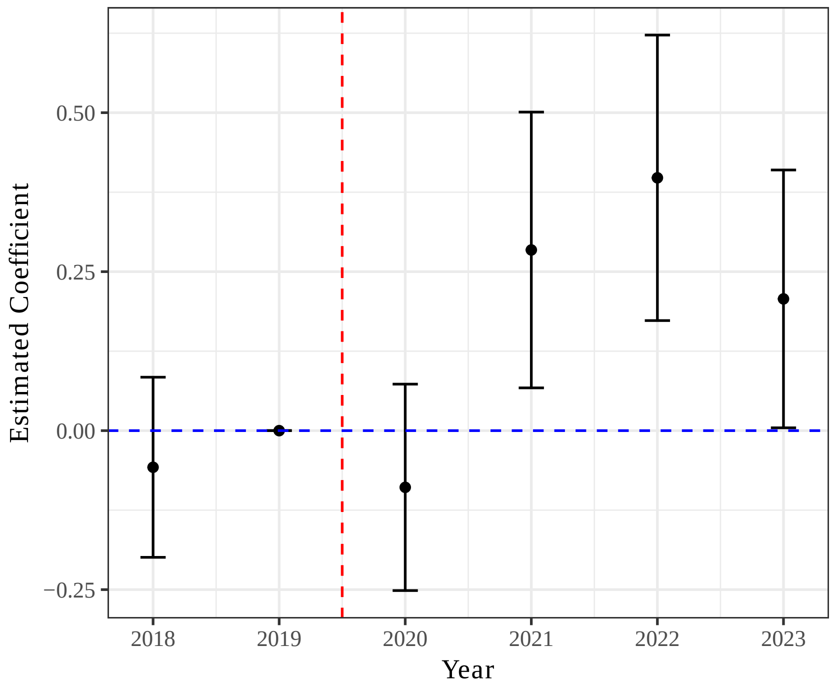
[18]As a concrete example, Figure A.9 shows that counties with larger black populations, smaller male populations, and higher median ages saw larger unemployment shocks. These are demographics which tend to be at lower risk of drug mortality during the US opioid epidemic (Jalal et al., 2018; Monnat, 2019). Thus, in the absence of the unemployment shock, these counties would have likely seen a lower increase in drug mortality than other counties, leading to the negative confounding.

mate the following equation restricted to rural counties:

$$\text{Overdose}_{ct} = \alpha_c + \delta_t + \sum_{j=-2}^{3} \beta_j Z_c + \epsilon_{ct} \tag{5.1}$$

Here, Overdose_{ct} is again the drug mortality rate per 100,000 in county c in year t, α_c is a county fixed effect, δ_t is a year fixed effect, and Z_c is the shift-share instrument. I restrict the sample to rural counties and plot the coefficients on the instrument for each year relative to 2019. The figure shows that the coefficient on the reduced-form instrument is not significant in 2018 and 2020, while it is significant in all years from 2021 to 2023.

Figure 2: Event Study Specification of the Effect of the COVID-19 Unemployment Shock on Drug Mortality (Rural Counties)



Notes: This figure shows the estimated coefficients $\hat{\beta}_j$ when estimating equation (5.1). 95% confidence intervals are based on standard errors clustered at the county level. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Observations are weighted by population size. The dependent variable is annual drug mortality per 100,000 and the estimation includes year and county fixed effects. The coefficients are normalized to 0 in 2019.

Similarly, Appendix Figure A.8 illustrates the coefficients on my instrument in a specification when estimating the two-stage regression separately for each year, restricting my sample to rural counties, with similar results. The coefficient remains insignificant from 2018 to 2020 but becomes relatively large and significant in 2021 and 2022 before attenuating slightly and losing significance in 2023. The estimated time frames are, therefore, in line with the expected effect times in the literature. In particular, the lack of an estimated effect in 2020 and the null findings for placebo checks in 2019 and 2018 are reassuring.

This divergence between the overall and rural-county-restricted results is noteworthy. The results imply that the average treatment effect for a US citizen was null. However, the average (complier) rural citizen experiences a significant increase in drug mortality risk in response to the unemployment shock. There is substantial heterogeneity in response to the unemployment shock between urban and rural counties. I explore mechanisms and reasons for this difference further in Section 6.

The IV estimate in my preferred specifications yields a higher estimate than the corresponding reduced-form and OLS regressions. When comparing the results to the OLS estimates, this may be caused by endogeneity.[19] The effect size may, however, also be different since the IV estimate only identifies the local average treatment effect (LATE), conditional on the exclusion restriction holding and technical assumptions on the shift-share instrument being a convex sum of weights on individual industry shares (Goldsmith-Pinkham et al., 2020). That is, the effect estimated in Column (6) of Panel B of Table 3, is not the nation-wide average effect but constitutes the average effect of a one-unit increase in the unemployment shock on drug mortality for compliers, i.e., for the counties for which the local changes in unemployment in the beginning of 2020 are most significantly influenced by changes in national industry employment. As I will show in Section 6, effects are heterogeneous across counties, and therefore, I cannot make an effect size inference based on the identified LATE. To provide an intuition for the complier counties, Appendix Table A.4 illustrates descriptive statistics of the top 20 percent of rural counties for whom the absolute difference between the instrument-predicted and actual unemployment shock is smallest and the remaining counties. There are few significant differences between the two groups, indicating that the compliers are not systematically different from the remaining counties. Compliers have slightly fewer mental health providers and a slightly larger employment share in manufacturing.

On the other hand, the reduced form results in Column (4) can be more easily interpreted. The estimated coefficient implies that a one percentage point increase in a county's industry-weighted exposure to the national unemployment shock leads to an increase of 0.228 annual drug deaths per 100,000, holding all else constant. Given an approximate 11 percentage point national unemployment shock, this simple calculation implies an increase in average rural county drug mortality of 2.5 per 100,000, which corresponds to approximately 28% of the

[19] This downward bias in the OLS estimate is consistent with the negative confounding discussed above and illustrated in Figure A.9. In particular, while larger 2019 drug mortality is a positive predictor of the unemployment shock, counties which saw larger drug mortality increases from 2018 to 2019 were less likely to experience a larger unemployment shock and demographic characteristics correlated with a larger unemployment shock tend to be associated with lower opioid risk (see Footnote 18). This leads to a downward bias in the OLS estimate.

increase in the average drug mortality rate in rural counties from 2019 to 2022.[20]

5.2 Robustness

I turn to various robustness checks of my estimated effects. The tables and figures presented and discussed in this section are restricted to rural counties. I replicate all results using the full sample in Appendix D.

As previously mentioned, Figure A.8 illustrates that my instrument passes placebo checks in 2018 and 2019. On the other hand, Appendix Figure A.9 shows that the 2020 unemployment shock is a significant predictor for 2019 drug mortality, while the instrument is not, providing evidence for the endogeneity concern of directly regressing drug mortality on the unemployment shock.

These results suggest that static confounders, which affect both the shift-share instrument and drug mortality, are not a major concern in my specification. However, dynamic processes correlated with initial industry composition may confound the estimated effects. As an illustrative example, consider that counties with certain industry compositions saw larger emigration in response to vanishing economic opportunities, leaving behind populations which are more vulnerable to drug abuse. I employ two approaches to mitigate such concerns. Firstly, I allow for time-variant controls. This allows me to capture dynamics in population characteristics, which may work as proxies for dynamic, unobservable effects. The results for this estimation are in Appendix Table A.5 and Appendix Table A.6. The results are practically unchanged compared to using fixed-at-baseline controls.

Second, to provide suggestive evidence about which industries drive the effect, I reconstruct my instrument when excluding one major 2-digit NAICS industry at a time and then re-estimate my model with the restricted instrument while controlling for 2019 employment shares in the excluded industry. The results are in Appendix Table A.3. I find estimates which are in line with my overall estimate, with the exception of excluding the hospitality and food services industry (NAICS 72). The exclusion of the hospitality industry leads to a smaller and insignificant coefficient estimate, suggesting that the hospitality industry is extraordinarily important for the estimated effect. This is plausible given the large overall employment of the hospitality industry and stronger, long-term exposure of hospitality to repeated economic shocks during the COVID-19 pandemic.

The hospitality and food services industry is broadly distributed across counties, as illustrated in Appendix Figure A.4, and encompasses a wide range of sub-industries, which

[20] Average drug mortality in rural counties increased from 17% in 2019 to 26% in 2022. The estimated effect thus accounts for approximately 28% of the nine percentage point increase in observed drug mortality.

reduces the likelihood of systematic bias. Nevertheless, the strong reliance on the hospitality industry highlights the importance of interpreting the results cautiously. As alternative robustness checks, I re-estimate the model when controlling for the baseline employment share in the hospitality industry and allowing for a custom year-trend based on initial services employment while using the original instrument construction based on all industries.[21] The results in Appendix Table A.9 are unchanged compared to my preferred specification, indicating that counties with a higher baseline employment share in the services industry are not on generally differential mortality trends.[22] Additionally, in Appendix Table A.10, I construct the instrument when excluding one subsector of the hospitality industry at a 3-digit NAICS level at a time. In this case, the coefficients remain attenuated but regain significance.

Appendix Table A.11 additionally estimates the preferred specification in subsamples, whereby one division is excluded at a time.[23] The results remain significant and quantitatively similar to my preferred specification in all cases, suggesting that no single division drives the estimated effect.

Additionally, as Goldsmith-Pinkham et al. (2020) suggest, I provide the results of regressing industry shares on various 2019 observables in Appendix Table A.2. Note that I control for any of those covariates in my regressions. The goal of these regressions is to show how much of the variation in industry shares of the major industries driving the results can be explained by other risk factors, which might also indirectly cause an increase in drug mortality.[24] While I find various statistically significant covariates, the overall R^2 values are low throughout. Thus, reassuringly, these results show that only a small fraction of variation in industry shares is explainable by covariates that may otherwise directly affect the post-COVID changes in drug mortality. This finding is supported by my regression of the instrument on various characteristics in Appendix Figure A.9.

Furthermore, I consider alternative instruments and unemployment shock definitions in Appendix Tables A.7 and A.8. The results remain qualitatively unchanged when constructing the instrument and unemployment shock over different time frames, e.g., defining the

[21] That is, I include the following controls in my regression for each county and year:

$$\text{H-Share}_c \times \mathbf{1}_{t=2020} + \text{H-Share}_c \times \mathbf{1}_{t=2021} + \text{H-Share}_c \times \mathbf{1}_{t=2022} + \text{H-Share}_c \times \mathbf{1}_{t=2023}$$

where $\mathbf{1}_t$ is an indicator function for the year t and H-Share_c is the share of employment in the hospitality industry in county c in 2019.

[22] This suggests that the alternative instrument construction may fail to find a significant effect due to the lack of power or because the excluded industry introduces a bias in the estimated unemployment shock, which is pronounced for certain counties which are not representative of the average complier.

[23] A division is an aggregation level of states defined by the Census Bureau. There are nine divisions, the definition of which can be found here: `https://www2.census.gov/geo/docs/maps-data/maps/reg_div.txt`

[24] Goldsmith-Pinkham et al. (2020) suggest providing regression results for the industries with the largest Rotemberg weights. For simplicity, I use the industries that saw the largest national unemployment changes.

unemployment shock as the year-over-year unemployment changes from 2019 to 2020 rather than restricting the unemployment shock to Spring 2020 or defining the instrument based on annual changes in industry employment.

6 Heterogeneity Analysis and Mechanism

I next explore heterogeneity dimensions of my estimates. I discuss plausible mechanisms by which unemployment shocks can lead to drug overdose deaths to motivate my heterogeneity analyses and discuss the implications of my results for these potential mechanisms. I support my heterogeneity analyses through additional regressions where I investigate the association of the instrumented unemployment shock with intermediate measures of economic stress. I then outline possible reasons for the different drug mortality responses of urban and rural counties to the unemployment shock. Based on the main results, I again restrict my analysis to rural counties and replicate selected results with the full sample in Appendix A.IV.

My key hypotheses follow from my stylized model of drug consumption and drug supply in Section 3. Based on strong evidence from the medical and public health literature briefly discussed in Section 3.1, I expect the mechanism of unemployment to drug abuse to work through a channel of increased stress (e.g., Sinha, 2001; Paul and Moser, 2009; Amaro et al., 2021). Unemployment may impair mental health, particularly through the effect of financial hardship, leading to drug use as a coping mechanism. The increased stress effect of the unemployment shock is likely larger in counties which previously faced economic hardship, e.g., due to high poverty or food insecurity rates.

I estimate heterogeneous effects by re-estimating (4.3) and (4.4) but allowing for an interaction term between the unemployment shock (and Bartik-style instrument, respectively) and the heterogeneity dimensions. I create z-scores of my heterogeneity dimensions to ease interpretation.

Based on the above-outlined mechanisms of increased economic stress, I primarily focus on measures of economic and social vulnerability. After considering various pre-pandemic characteristics, I additionally consider intermediate outcomes which measure changes in economic stress.

Additionally, unemployment may lead to drug abuse through channels that are different yet related. For instance, unemployment may lead to social isolation as the workplace may act as an important social outlet – and social isolation may, in turn, lead to drug abuse (Christie, 2021). This effect could be exacerbated during the pandemic-induced lockdowns. Also, unemployment can lead to insurance loss as the majority of the US population receives employer-sponsored health insurance (Bureau, 2024). Loss of insurance, in combination with a pre-existing pain problem, may lead to drug abuse as a way of self-medication. I briefly consider these heterogeneity dimensions and intermediate outcomes in a supplementary part of my analysis.

6.1 Economic and Social Vulnerability

My primary focus is on heterogeneity based on previous economic and social vulnerabilities. An unemployment shock greatly increases stress among economically and socially vulnerable groups, thus increasing the risk of drug abuse. As an example, consider populations living closer to poverty. Under such harsher financial constraints, unemployment shocks may more strongly translate to stress and despair, as unemployment more quickly leads to food and housing insecurity. For my heterogeneity dimensions, I consider a measure of frequent mental distress based on the CDC's Behavioral Risk Factor Surveillance System, a social vulnerability index, and the percentage of the population living in poverty prior to the pandemic.

Additionally, under the assumption that social isolation increases the risk of economic stress translating to drug abuse, areas with stronger social networks should see attenuated effects of the unemployment shock on drug mortality. I employ a proxy measure based on the number of non-business social associations registered in a county before the pandemic. Table 4 shows the results for these heterogeneity dimensions.

Table 4: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Social Vulnerability

	Social Associations	Poverty Pct	Mental Distress Index	Social Vulnerability Index
	(1)	(2)	(3)	(4)
Unemployment Shock	0.866***	0.997***	1.009***	0.914***
	(0.283)	(0.330)	(0.367)	(0.334)
Unemp. shock × Heterogeneity	-0.580**	0.867**	0.675***	0.706***
	(0.246)	(0.376)	(0.223)	(0.242)
N	5840	5840	5840	5840
R^2	0.63	0.63	0.63	0.63
Within R^2	0.42	0.41	0.41	0.41
First Stage F-stat	433.42	507	419.83	439.12

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Column (1): heterogeneity by normalized number of social associations in 2019, Column (2): heterogeneity by normalized share of population living in poverty in 2019, Column (3): heterogeneity by normalized share of population with frequent mental distress in 2019, Column (4): heterogeneity by normalized social vulnerability index in 2019. Observations are weighted by population size. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

I can confirm all of my hypotheses. In particular, a one standard deviation increase in the number of social associations in a rural county decreases the estimated effect size of unemployment on drug mortality by approximately two-thirds, suggesting an important mitigating factor of social support networks on the relation between unemployment and drug mortality – at least during the extraordinary circumstances of the COVID-19 pandemic. I also find highly significant effects of poverty, mental distress, and social vulnerability on the effects of the unemployment shock on drug mortality. A one standard deviation increase in pre-pandemic poverty over the mean almost doubles the estimated effect of the unemployment shock on drug mortality. These findings provide suggestive evidence for the mechanism by which stress increases in response to an unemployment shock are concentrated in previously vulnerable rural communities with heightened pre-pandemic stress levels, and these communities are more likely to turn to drug consumption as a coping mechanism.

Appendix Table A.12 repeats this heterogeneity analysis for the full sample. While I find no overall effect, I do find a positive but imprecisely estimated interaction effect for the poverty dimension and a significant positive effect for the mental distress dimension.[25]

[25] The other dimensions are muted and the sign on social associations switches. This is an artifact due to the

I next turn to estimating the association between the COVID-19 unemployment shock and various measures of economic hardship. To do so, I re-estimate equation (4.4) and replace drug mortality as an outcome with various post-shock indicators of economic stress.

Table 5: Impact of Unemployment Shock on Intermediate Economic Stress Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Food Insecurity		Housing Cost Burden		Social Vulnerability	
	All	Rural	All	Rural	All	Rural
Unemployment Shock	0.095***	0.107***	0.433***	0.419***	0.001	0.011***
	(0.017)	(0.023)	(0.069)	(0.084)	(0.002)	(0.003)
Mean	11.45	12.1	11.11	10.53	0.5	0.51
N	3102	1947	3097	1942	3102	1947
R^2	0.98	0.95	0.87	0.53	0.95	0.9
Within R^2	0.96	0.88	0.75	0.31	0.93	0.78
First Stage F-stat	654.83	270.73	644.97	270.03	646.01	270.88

Notes: In all regressions, the dependent variable is the respective column header. In columns (1) and (2), the dependent variable is the share of households with food insecurity in 2021. In columns (3) and (4), the dependent variable is the share of households with an excessive housing cost burden in 2021. In columns (5) and (6), the dependent variable is the 2022 Social Vulnerability Index. In each regression, the unemployment shock is instrumented by the shift-share instrument. All regressions include state fixed effects and the full set of controls as specified in Appendix B.I. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table 5 presents the results of regressing food insecurity measures, the share of households with an excessive housing cost burden, and the 2022 Social Vulnerability Index on the instrumented unemployment shock while controlling for pre-pandemic levels of these covariates. I find significant positive coefficients of the unemployment shock for both food insecurity and excessive housing cost burden in rural counties and the complete sample, providing suggestive evidence that the unemployment shock negatively impacted economic security in rural and urban areas, with a slightly larger coefficient on food insecurity in rural counties.[26] I additionally only find a significant effect of the unemployment shock on the social vulnerability index in 2022 in rural counties, indicating that potential second-order measures of economic distress were more negatively affected in rural counties.

Given the large federal unemployment benefits and renter protections during the pandemic, some explanations are required as to why these economic stress indicators measurably

measure of social associations being larger in rural than in urban counties, thus serving as a proxy for rurality in the complete sample.

[26]Given the relatively greater food insecurity in rural areas before the pandemic, these estimates may understate the increased economic stress, as previously 'food insecure' populations may experience even larger food insecurity. However, food insecurity is measured as a percentage of the population and thus does not measure the intensive margin.

increased and why the effect was plausibly larger in rural counties. I provide suggestive evidence that both access to unemployment benefits and credit were constrained in rural counties, which likely increased economic strain.

Appendix Figure E.1 plots the unemployment rate and the percentage of the population receiving unemployment benefits for rural and urban counties in California. I use this measurement as a noisy proxy for the relative take-up of unemployment benefits. Notably, the share receiving unemployment benefits is substantially lower in rural counties compared to urban counties, as also highlighted by Bell et al. (2023). These findings suggest that rural counties faced barriers in accessing unemployment benefits to which they were entitled, thereby increasing financial strain.

In Table 6, I provide additional heterogeneity analysis by measures of credit constraints and a state-level indicator for the extent of economic support during the second half of the initial pandemic year. I find a significantly larger effect of the unemployment shock on drug mortality in counties with higher credit constraints and a smaller effect in states with larger economic support. Additionally, in Appendix Table E.1, I regress 2021 debt-to-income ratios in urban and rural counties on pre-pandemic debt-to-income ratios and the instrumented unemployment shock. I find a positive effect in urban counties and no significant effect in rural counties, indicating that urban populations, on average, increased debt to smooth consumption. In contrast, rural populations, potentially due to credit constraints, could not access such credit markets, ultimately leading to greater economic stress.

Table 6: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Credit Constraints and Economic Support

	Credit Constrained Pct.	Credit Insecurity Index	State Economic Support
	(1)	(2)	(3)
Unemployment Shock	0.988***	0.831***	0.876***
	(0.345)	(0.278)	(0.290)
Unemp. shock × Heterogeneity	0.682**	0.447*	-0.311**
	(0.311)	(0.239)	(0.128)
N	5679	5679	5840
R^2	0.63	0.63	0.63
Within R^2	0.41	0.42	0.42
First Stage F-stat	458.88	450.18	442.11

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Column (1): heterogeneity by normalized share of population with credit constraints in 2019 (Scott et al., 2025), Column (2): heterogeneity by normalized credit insecurity index in 2019 (Scott et al., 2025), Column (3): heterogeneity by normalized index for state-level economic support during the initial pandemic year (Hale et al., 2021). Observations are weighted by population size. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

6.2 Alternative Mechanisms

I next investigate alternative mechanisms which may explain the effect of unemployment on drug mortality. Additional comments, results, and the relevant regression tables of these alternative mechanisms are in Appendix E.

Social Isolation. Unemployment shocks are likely correlated with social isolation, for example, if they are both caused by COVID-19-related lockdowns. It could, therefore, be that social isolation is one of the main drivers rather than unemployment itself.[27] Notably, lockdowns were generally decided on the federal and state-level and are thus absorbed by state-year fixed effects. To additionally investigate the social isolation channel, I consider heterogeneity dimensions of pre-pandemic risk factors of social isolation, such as the percentage of single-person households in Appendix Table E.3. Additionally, I regress intermediate outcomes which measure social isolation, such as Google mobility data and survey-based responses on the instrumented unemployment shock in Appendix Table E.4 and Appendix Table E.5. I

[27] Social isolation may increase or decrease drug abuse. Otachi et al. (2025) find that larger perceived social isolation decreases drug use as drug users may find social support networks which promote drug consumption. On the other hand, Jeffers et al. (2022) find that social isolation during the pandemic was associated with worse mental health outcomes and higher drug use.

do not find a strong link between unemployment shocks in rural counties and social isolation. Thus, social isolation on its own is unlikely to be the primary driver of the observed mechanism.

Labor Market Flexibility and Unemployment Persistence. In theory, prolonged periods of unemployment may lead to an increasing accumulation of economic stress. One could plausibly expect a larger effect of the unemployment shock on drug mortality in counties with weaker economic recovery or less flexible labor markets. In Appendix Table E.2, I employ various heterogeneity dimensions to measure labor market shock persistence. Heterogeneity analysis by pre-pandemic measurements of labor market flexibility, such as a Herfindahl-Hirschman index of industry employment and post-pandemic unemployment recovery, fails to find a significant effect. This null finding may be driven by data inaccuracies, e.g., due to labor market dropouts or since individual unemployment duration for vulnerable populations may differ considerably from county-level trends.

Health Insurance and Self-Medication. In the US insurance system, most of the population receives employer-sponsored health insurance. An unemployment shock can thus impede access to necessary health treatments, leading to self-medication through illicit opioids. As a secondary channel, I consider the capabilities of accessing mental health services during the pandemic, hypothesizing that access to mental health counselling may reduce stress associated with the unemployment shock, reducing the risk of drug abuse.

The results and more details on variable construction are in Appendix Table E.6. I do not identify a significantly stronger effect in counties with a higher share of employer-sponsored health insurance pre-pandemic. I find a significant and relatively large coefficient on the pre-pandemic injury death rate.[28] I find a marginally significant mediating effect of broadband access and no significant effect of mental health provider density. Additionally, investigating intermediate outcomes, I find no effect of the unemployment shock on healthcare utilisation but do find that the unemployment shock increased the rate of uninsured adults (Appendix Table E.7). The insurance loss effect is more pronounced in rural counties.

Initial COVID-19 Case Count and Prior Drug Mortality. Lastly, I investigate whether the unemployment shock affects counties differently depending on the initial intensity of the pandemic, measured by the number of COVID-19 cases reported during the initial outbreak in the Spring of 2020 and whether the unemployment shock affected counties differently based

[28] This positive coefficient may indicate that the risk of self-medication is present and leads to drug abuse. However, the channel of injury deaths is difficult to evaluate: higher chronic pain risk could also affect drug mortality in other ways, e.g., as counties with higher pre-pandemic addiction risk may respond differently to unemployment shocks.

on their pre-pandemic drug mortality rates.

Appendix Table E.8 summarizes the results. I do not find differential effects based on the initial intensity of COVID-19 infections during the March to April 2020 wave or pre-pandemic drug mortality. While the coefficient on pre-pandemic drug mortality is relatively large, it is imprecisely estimated. These results suggest that the increase in drug mortality in response to the unemployment shock cannot be solely attributed to counties with higher initial COVID-19 intensity or pre-pandemic drug mortality. A strongly heterogeneous effect by these dimensions could indicate dynamic confounders related to pre-pandemic characteristics, potentially confounding the estimated effect of the unemployment shock on drug mortality. I can rule out that the unemployment shock acts solely as a proxy for initial COVID-19 case intensity or only increased drug mortality in counties with higher pre-pandemic drug abuse risks.

I estimate a double-heterogeneity setup to investigate whether the presumed economic vulnerability effect persists when accounting for these alternative mechanisms. In Appendix Table A.13, I find that the main effect and the coefficient on the pre-pandemic poverty interaction term remain significant in all specifications, suggesting that the economic vulnerability channel remains strong when accounting for heterogeneity by healthcare access or social isolation risks.

To summarize, the results of the heterogeneity analysis imply that particularly socially vulnerable counties and counties exposed to economic stress through unemployment were most likely to experience fatal drug abuse. Heterogeneity by pre-pandemic measurements of social and economic hardship and intermediate outcomes support this reasoning. This is in line with the evidence obtained by Sweeney et al. (2024) in a qualitative study on drug use during the COVID-19 pandemic. These results point to a mechanism whereby unemployment shocks increase stress in counties that already had higher social and economic vulnerabilities, leading to drug abuse as a coping mechanism. Furthermore, the results provide evidence about the robustness of my findings by showing that the effects are not systematically different across counties with higher or lower pre-pandemic mortality or higher or lower initial pandemic intensity. I find some mixed and weaker evidence for access to (mental) healthcare and chronic pain risk as supplementary mechanisms.

6.3 What Causes the Difference Between Urban and Rural Counties?

My results point to important differences in how the COVID-19 unemployment shock translated into drug mortality in urban and rural counties: I find a significant positive effect of the instrumented unemployment shock on drug mortality in rural counties and no significant effect in urban counties. Notably, I observe a rise in drug mortality, which is similar in size in urban and rural counties (see Appendix Figure A.5 which illustrates average drug mortality rates in rural and urban counties). In this subsection, I aim to summarize my results and provide additional context from the literature to explain the differences in the mechanisms between urban and rural counties. I will focus on the following two factors: (i) differential effects of the unemployment shock on economic stress, (ii) pre-existing economic vulnerabilities, including access to healthcare. I will also briefly discuss the role of labor market recovery as a driver of divergent mechanisms, as well as supply-side factors affecting the drug market.

Differential impact of the unemployment shock. To recap, I find that the instrumented unemployment shock is significantly associated with various measures of economic distress, such as heightened housing cost burden and increased food insecurity, both in the complete sample and when restricted to rural counties. Social vulnerability, a composite index of various demographic and economic vulnerabilities, only increases in rural counties in response to the shock. Additionally, the effect on food insecurity was marginally larger in rural counties. This suggests that the translation of the unemployment shock to economic stress was larger in rural counties. I find suggestive evidence that access to unemployment benefits and credit was more constrained in rural counties, which could potentially explain the stronger translation of the unemployment shock to economic stress. This is in line with the literature which has found that rural counties have fewer safety nets and are more likely to turn to drug abuse in response to economic shocks (Monnat, 2019). Structural differences in access to safety nets are thus potential reasons for the differential impact.

Other than access to unemployment benefits and credit markets, idiosyncratic differences in the labor market may also play a role in the differential impact of the unemployment shock. For instance, rural counties may have more concentrated labor markets or a larger share of the population employed in the informal economy, understating the actual unemployment shock. However, I do not find that the effect of the unemployment shock on drug mortality is significantly mediated by measures of labor market recovery or flexibility, suggesting that these factors are not the main drivers of the observed differences. Additionally, urban labor markets experienced larger and more persistent unemployment shocks over a large portion of the pandemic. The shock structure is, therefore, unlikely to be the main driver of the differences in drug mortality between urban and rural counties, as we would expect the opposite effect.

Pre-existing vulnerabilities. In addition to the smaller effect on economic stress in urban counties, differences in pre-existing vulnerabilities and a hypothesized non-linear relationship between economic stress and drug consumption contribute to the different mechanisms in urban and rural counties. My heterogeneity analysis points to pre-existing social vulnerabilities, such as poverty and mental distress, being important risk factors for the effect of the unemployment shock on drug mortality. Rural counties consist of populations with higher pre-existing social vulnerabilities, such as elevated rates of (persistent) poverty, lower education levels, and lower median incomes compared to urban counties (Andreyeva and Wang, 2023).[29] An equal increase in economic stress may thus lead to a larger increase in drug abuse as a coping mechanism in rural counties, as higher pre-existing vulnerabilities decrease the threshold where economic disruptions lead to existential stress. These changes in the intensive margin of economic stress are likely understated in various measurements, such as food insecurity and housing cost burden, which are measured as percentages of the total population.

Another pre-existing vulnerability in rural counties is limited access to healthcare services. Notably, I do not find significant evidence that access to mental health providers in rural counties mediated the effect. These findings go against the alternative hypotheses that social isolation or healthcare access in rural counties were driving effects for divergent mechanisms. My findings are, however, only suggestive, and the literature finds mixed evidence. Previous qualitative studies have highlighted resource gaps in the provision of mental health treatment between urban and rural counties (Pullen and and Oser, 2014; Johnson et al., 2018). Many treatment services during the pandemic have offered telehealth alternatives (Mark et al., 2022) which may alleviate some rural disadvantages in healthcare access but may also induce additional barriers such as worse broadband access in rural counties (Hirko et al., 2020; Pierce and Stevermer, 2023; Ramesh et al., 2023). In addition to mental health counselling, response times of emergency vehicles and access to drug abuse support groups are worse in rural areas (Alruwaili and Alanazy, 2022; Fredericksen et al., 2024). Data fidelity is low, making it difficult to establish clear, causal results.

Structural social vulnerabilities, such as a higher risk of social isolation and stigma around drug abuse, may additionally fuel the risk of drug mortality in rural counties. My findings when regressing intermediate measures of social isolation on the unemployment shock suggest that social isolation is not a first-order driver of the effect. However, it may still play a role in the mechanism. In particular, social isolation measures during the pandemic are likely imprecise for smaller, rural counties, and aggregate social isolation likely increased across the

[29] See also Table 1.

US.[30]

Additionally, urban counties are more exposed to highly lethal synthetic opioids (Zoorob, 2019; Althoff et al., 2020; Spencer, 2022). The increased lethality of the drug supply introduces more unpredictable overdose deaths,[31] which can mask the expected pathway of stress-induced consumption gradually leading to drug mortality.

To summarize, differences in the effect of the unemployment shock on drug mortality between rural and urban counties are not attributable to a single cause but depend on a complex interaction of contextual factors. Notably, population characteristics and structural factors seem to be key differences rather than differences in the shock structure itself. Higher pre-existing vulnerabilities, in combination with a larger effect of the unemployment shock on the intensive margin of economic stress due to limited safety nets, culminate in a larger effect of the unemployment shock on drug mortality in rural counties. Further research and higher fidelity data on the drug supply and barriers to accessing unemployment benefits are needed to better understand the intrinsic differences and policy options for reducing the sensitivity of drug mortality to economic shocks in rural counties and aggregate drug mortality more broadly.

7 Conclusion

The start of the COVID-19 pandemic in 2020 saw a simultaneous rise in unemployment and a steep increase in drug mortality. Using a shift-share instrument and regular regressions, this paper finds that the average treatment effect of the unemployment shock on drug mortality is plausibly zero. However, within rural counties, there is a significant effect of the unemployment shock on drug mortality, which can explain approximately 28% of the increase in drug mortality rates observed in rural counties.

Estimating heterogeneous effects by pre-pandemic county characteristics provides evidence for a mechanism whereby rural counties with heightened economic stress pre-pandemic, limited credit access, and weaker social support networks experience increased economic hardship which culminates in drug abuse and drug mortality. My findings paint a complex picture of the relationship between unemployment shocks and drug mortality during the COVID-19 pandemic. Effects are heterogeneous, and effect sizes depend on an interplay between pre-existing vulnerabilities, social support, and frictions in accessing financial aid.

[30] My measures of social isolation are proxies based on survey results and mobility data. Survey results for these risk factors are usually model-based allocations of state survey results. These estimates are noisier in rural counties due to sample size concerns.

[31] E.g., unintentional drug consumption or first-time use may immediately lead to mortality, thus changing overall mortality trends in ways that confound the unemployment mechanism.

The policy implications of my findings are that decreasing barriers towards accessing unemployment benefits and other financial aid, as well as a general improvement in economic vulnerabilities in rural areas, are important levers for interrupting the mechanism by which unemployment shocks translate into drug mortality in periods of heightened economic and social uncertainty, such as the COVID-19 pandemic. Reduced initial economic stress, access to credit and larger supply-side changes affecting drug mortality may have masked the unemployment to drug abuse mechanism in urban counties. A better understanding of changes in the drug supply and effective policy measures to combat the opioid epidemic are needed to reduce aggregate drug mortality in the US.

My findings should be carefully interpreted within the context of the COVID-19 pandemic. In particular, my findings suggest that within this environment, larger unemployment shocks causally lead to higher drug mortality within economically and socially vulnerable rural counties with limited social support structures and reduced access to financial aid. Due to the extraordinary circumstances, the external validity of my findings outside of the US and the particular period at hand is limited. Simultaneous disruptions to healthcare systems, heightened economic uncertainty, and social isolation may cumulatively increase stress, thus leading to an upper bound of estimated effects in rural counties.

On the other hand, particularly in urban counties, large extensions of unemployment benefits attenuated the stress effect of the unemployment shock and heightened stress for the general population, which does not face unemployment, could increase drug mortality of the "control population", potentially leading to a lower bound of the estimated effect. Given the unique circumstances of the COVID-19 pandemic, whether my estimated effect constitutes an upper or lower bound outside of the pandemic is ambiguous. Nevertheless, increased future risk of health pandemics (Marani et al., 2021) and the potential for other large economic shocks which coincide with increased distress and uncertainty, such as natural disasters or global conflicts, highlight the relevance of my findings for future policy design.

The pandemic-related simultaneous shocks also provide challenges for clean identification. The identification assumption in this paper is violated if there are unobservable time-varying factors at the county level that influence drug mortality and are correlated with the initial industry composition or the extent to which the initial unemployment shock affected different industries on a national level. Notably, pandemic-specific policies are absorbed by state-year fixed effects, and I account for dynamic COVID-19 intensity control, greatly reducing the number of potential confounders that might drive my results.

My paper highlights the need for further academic research into the causes of the drug epidemic in the US and the relevant policies that may help combat it. One key area of research is to better understand how my findings during the COVID-19 pandemic generalize to other contextual settings. For instance, repeating the analysis in the context of general cyclical unemployment or large previous economic downturns such as the 2008-2009 recession may help establish the robustness of my findings. To guide future policy design, understanding frictions in obtaining financial aid in rural counties is an important avenue for protecting vulnerable communities which are most prone to turning to drug abuse to cope with economic stress. Understanding these frictions may additionally help alleviate poverty in rural counties. My findings suggest that this could have a direct effect on reducing drug mortality in times of heightened social and economic uncertainty. Additionally, to better understand the recent surge in US drug mortality, understanding the role of the drug supply and the fentanyl epidemic is crucial. Due to various endogeneity concerns, new and innovative approaches must be tested. In particular, understanding the effects of reductions in drug testing during the pandemic (Niles et al., 2021) is a largely unexplored and promising avenue for further exploration. This is a complex task due to various difficulties encountered in measuring illicit drug supply. Obtaining more granular data and plausibly exogenous variation in the fentanyl supply could help understand to what extent the lethality of the drug supply may interact with economic shocks to affect overall drug mortality.

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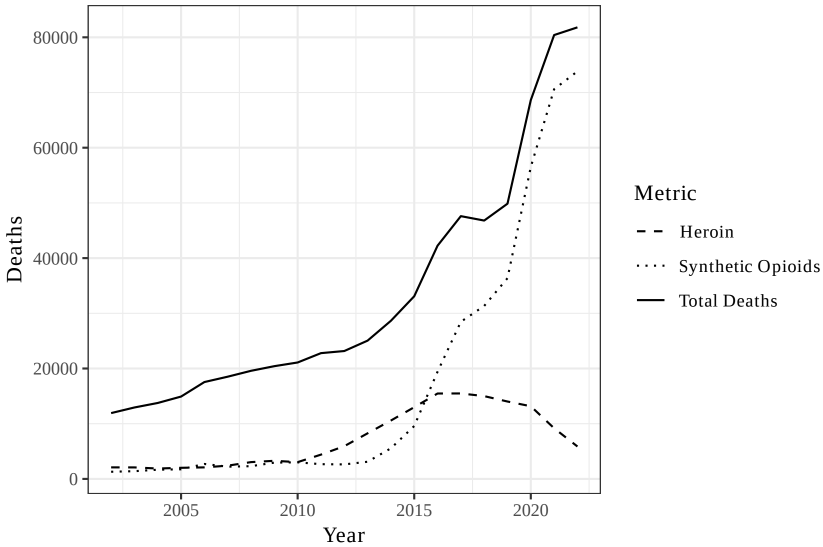
A Additional Tables and Figures

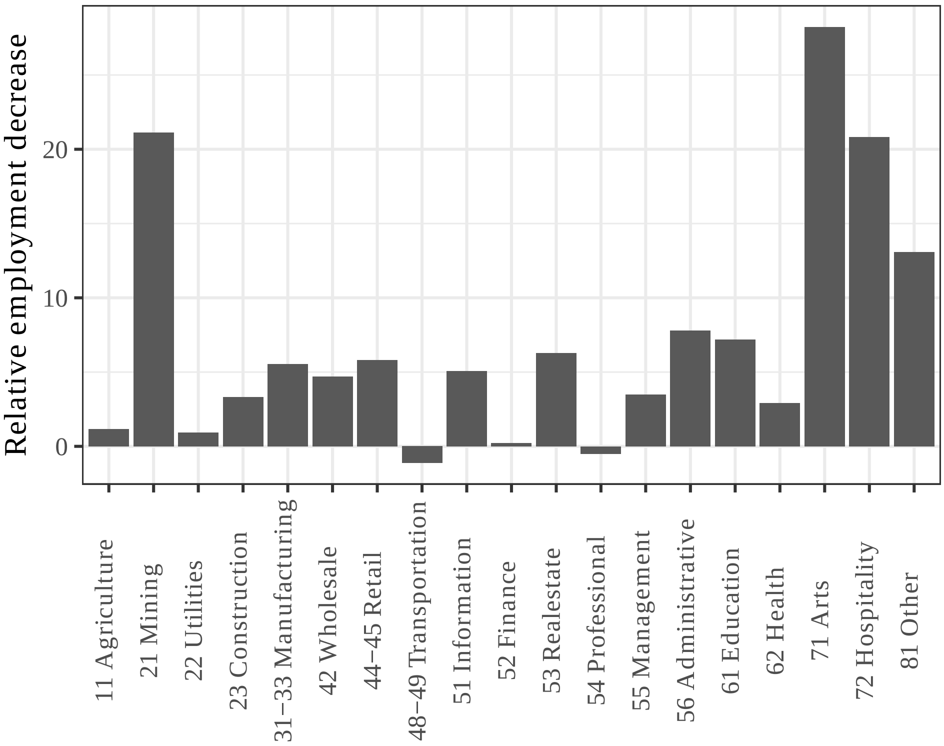
A.I Descriptive Tables and Figures

Figure A.1: Opioid-Specific Drug Overdose Deaths From 2002 to 2022



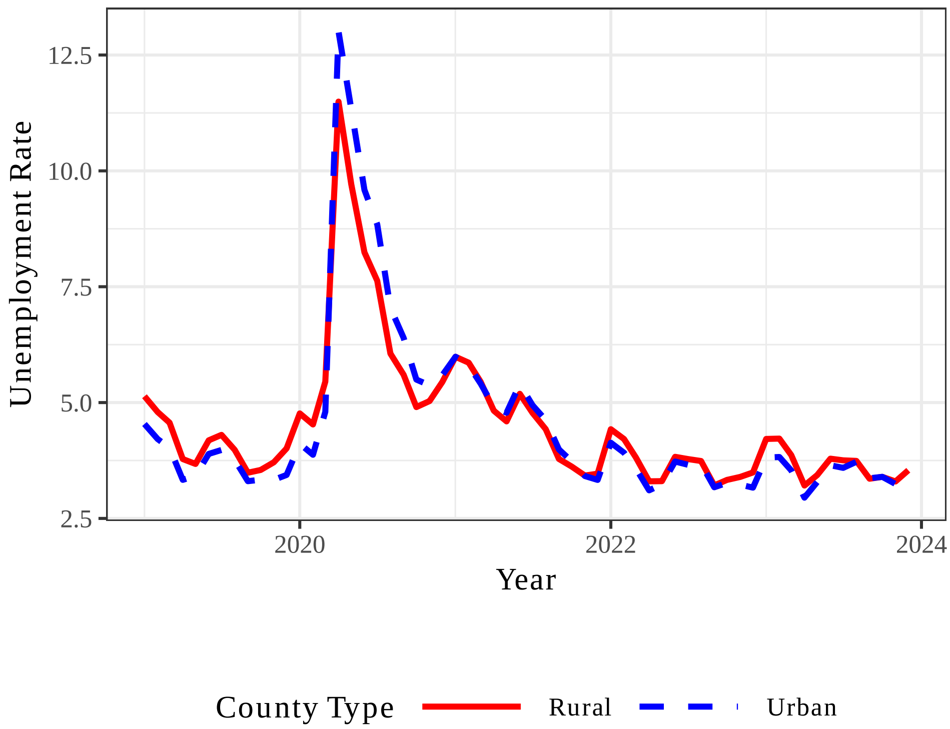
Notes: This figure shows annual US drug overdose death totals from opioids, heroin, and synthetic opioids from 2002 to 2022. Data source: Spencer et al. (2024)

Figure A.2: Relative Decreases in Annual Employment for 2-Digit NAICS Industries Between 2019 and 2020



Notes: This figure shows the relative changes in annual employment between 2019 and 2020 for all 2-digit NAICS industries on a national level. The figure is based on annual QCEW industry-level employment data. The figure shows that various industries saw relatively large declines in employment, with the largest declines in the arts and hospitality sector.

Figure A.3: Mean Unemployment for Rural and Urban Counties



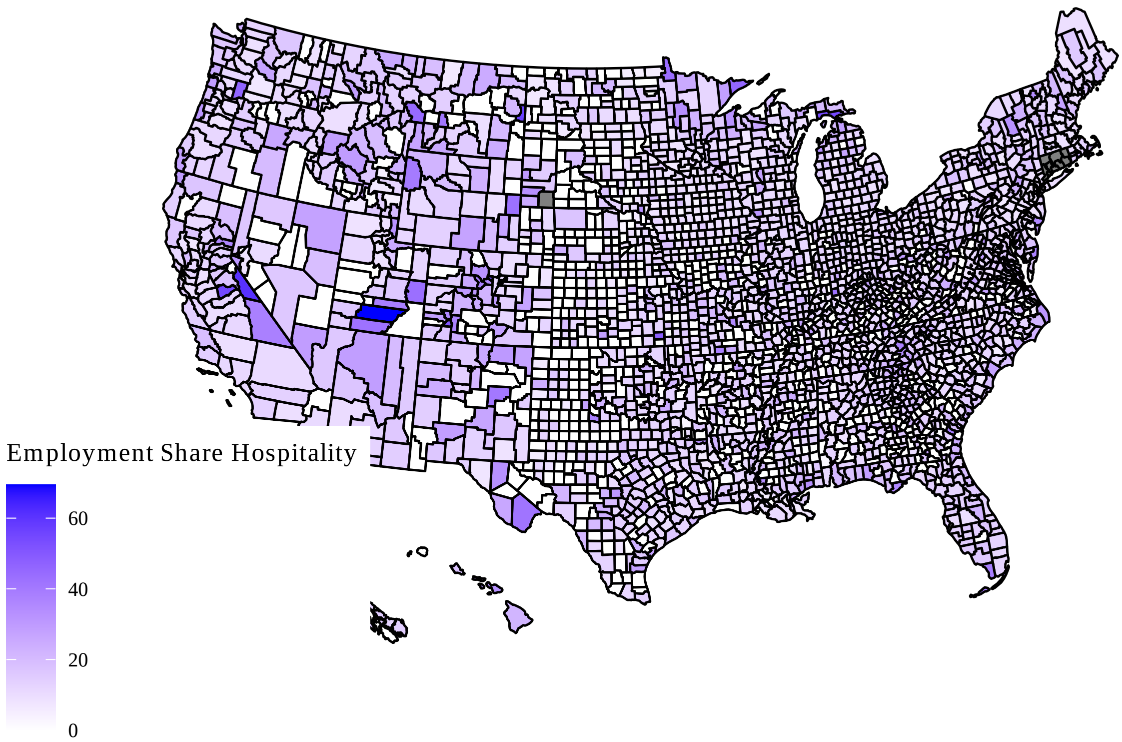
Notes: This figure shows monthly mean unemployment rates from 2019 to 2024 for rural and urban counties, separately. Rural counties are defined as nonmetro counties based on rural-urban continuum codes. The figure shows that unemployment rates were higher in urban counties from the beginning of the COVID-19 pandemic up to the middle of 2021, with a large spike in unemployment in April 2020. The figure is based on monthly unemployment data from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) program.

Table A.1: Descriptive Statistics of Employment Variables in Urban and Rural Counties

	Rural	Urban	Difference
Panel A: Employment			
Spring 2020 Unemp. Shock	5.29	7.44	2.15***
2019 Annual Unemp.	4.10	3.71	-0.38***
2020 Annual Unemp.	6.57	7.12	0.55***
2021 Annual Unemp.	4.62	4.69	0.08
2019 Labor Force Part.	56.56	61.48	4.92***
Panel B: Industry Composition			
Empl. Share Hospitality and Food	9.78	11.81	2.02***
Empl. Share Manufacturing	18.43	15.20	-3.23***
Empl. Share Oil, Gas, Mining	2.43	0.69	-1.74***
Empl. Share Agriculture	4.06	1.54	-2.53***
Industry Empl. Concentration Index	0.14	0.08	-0.06***
N	1974	1165	
Total Population (million)	46.04	282.26	

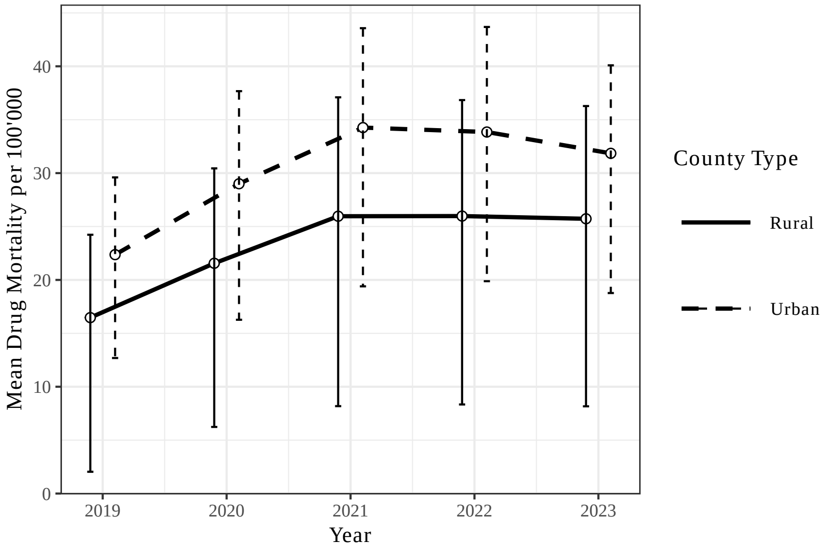
Notes: Rural counties are defined as nonmetro counties by Rural-Urban continuum codes. Spring 2020 unemployment shock is defined as the difference between average unemployment from April to June 2020 to February 2020 unemployment. The industry employment concentration index is a Herfindahl-Hirschman index calculated as the squared sum of employment shares in all 3-digit NAICS industries in 2019. Data Source: Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS) program and QCEW. The last column shows the difference between average values of rural and urban counties with stars indicating the p-value of a t-test testing the null hypothesis that the means of rural and urban counties are equal. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Figure A.4: Share of Total Employment in Hospitality and Food Services Industry in 2019



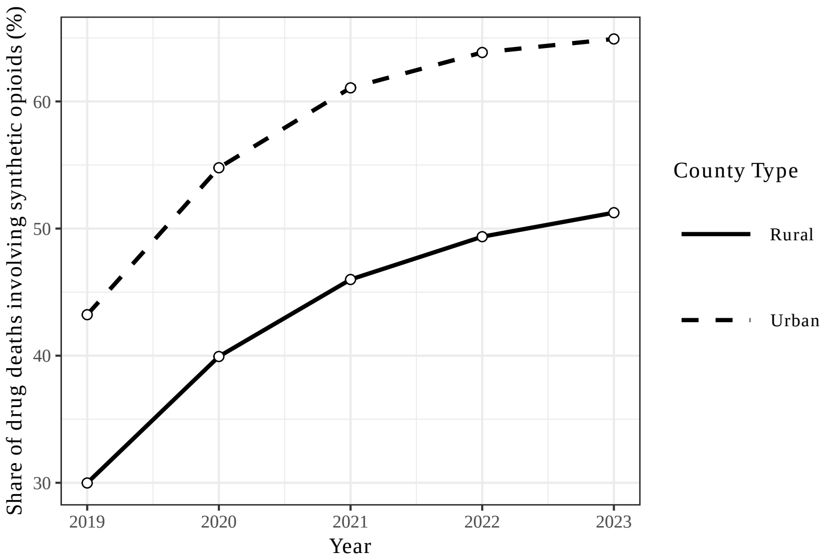
Notes: This figure shows the 2019 share of total employment which was in the hospitality and food services industry. The figure is based on annual QCEW industry-level employment data. Hospitality and food services is defined as NAICS codes 72. The figure shows that the hospitality and food services industry is a relatively large employer in many counties, with a median share of 10% of total employment.

Figure A.5: Mean Annual Drug Mortality for Urban and Rural Counties



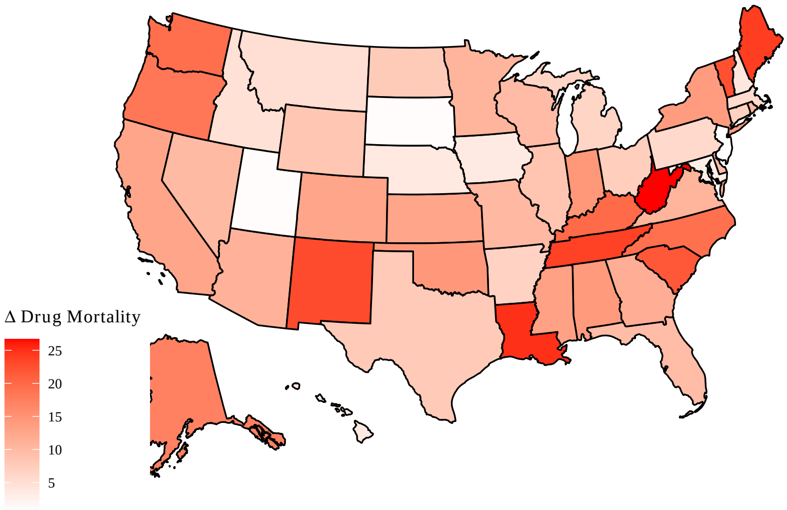
Notes: This figure shows the average annual drug mortality from 2019 to 2023 for urban and rural counties, respectively. The vertical line indicates the interquartile range. The figure is based on annual drug mortality data from CDC Wonder, with drug mortality defined as deaths with ICD-10 codes X40-44, X60-64, X85, and Y10-Y14. Rural counties are defined as nonmetro counties based on rural-urban continuum codes. Observations are unweighted. This figure shows that both urban and rural counties have seen increases in drug mortality, with urban counties having higher average drug mortality rates throughout the sample period.

Figure A.6: Share of Drug Deaths Involving Synthetic Opioids for Urban and Rural Counties



Notes: This figure shows the share of deaths involving synthetic opioids for urban and rural counties, respectively. The figure is based on annual drug mortality data from CDC Wonder with synthetic opioid deaths classified as ICD-10 code T40.4. Rural counties are defined as nonmetro counties based on rural-urban continuum codes. Observations are population-weighted. This figure shows that both urban and rural counties have seen increases in synthetic opioid deaths, with urban counties having a higher share of synthetic opioid deaths throughout the sample period.

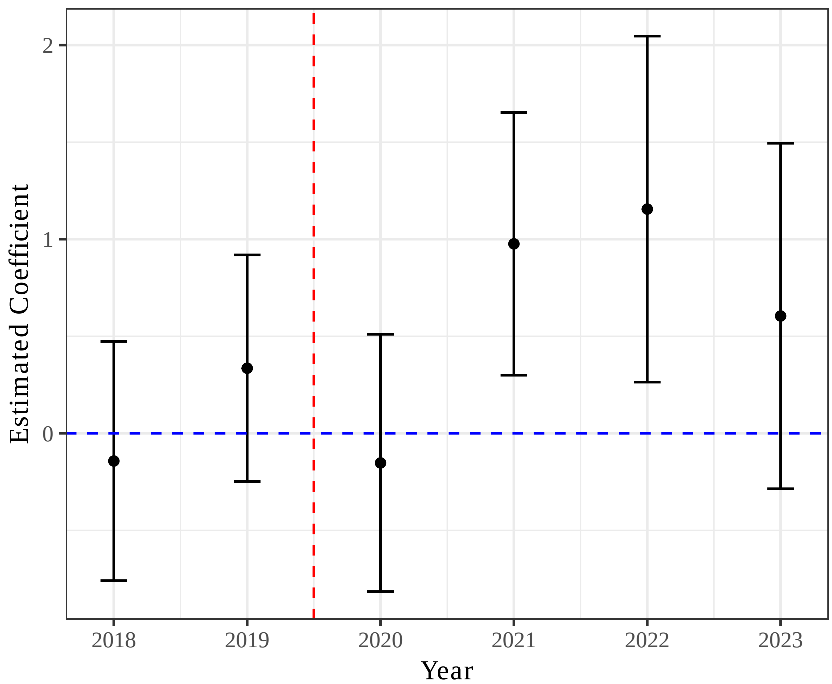
Figure A.7: Changes in Annual Drug Mortality Between 2019 and 2022



Notes: This figure shows the changes in annual drug mortality between 2019 and 2022 on a state level. The figure is based on annual drug mortality data from CDC Wonder, with drug mortality defined as deaths with ICD-10 codes X40-44, X60-64, X85, and Y10-Y14. Δ Drug Mortality is measured as the difference in drug mortality per 100,000 population between 2022 and 2019. The figure shows that all states have seen an increase in drug mortality, with the largest increases in the Northeast and select states in the Midwest and South.

A.II Additional Results

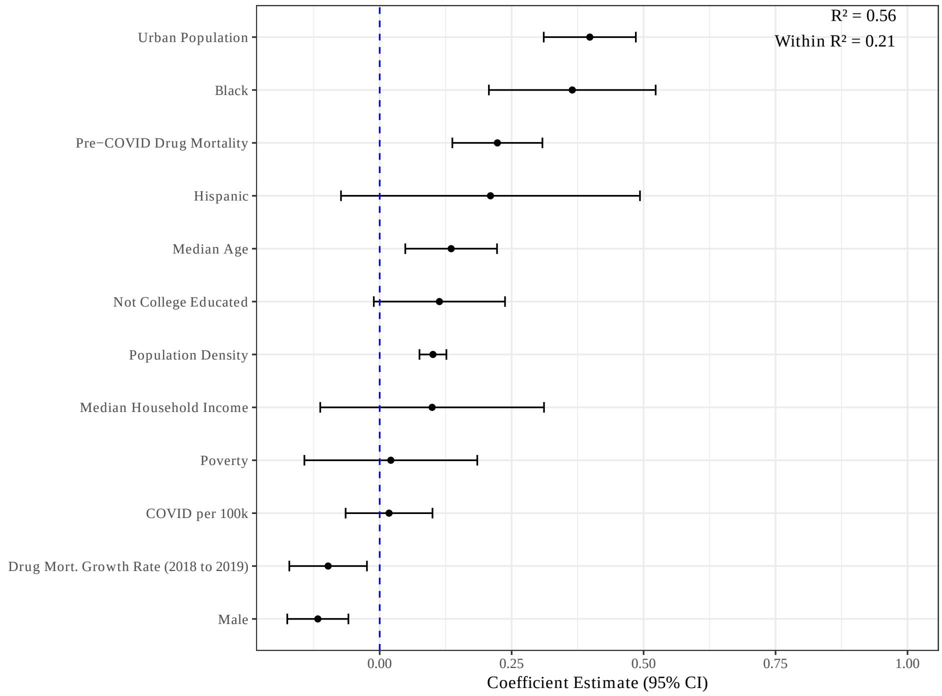
Figure A.8: Event Study Specification of the Effect of the COVID-19 Unemployment Shock on Drug Mortality (Rural Counties)



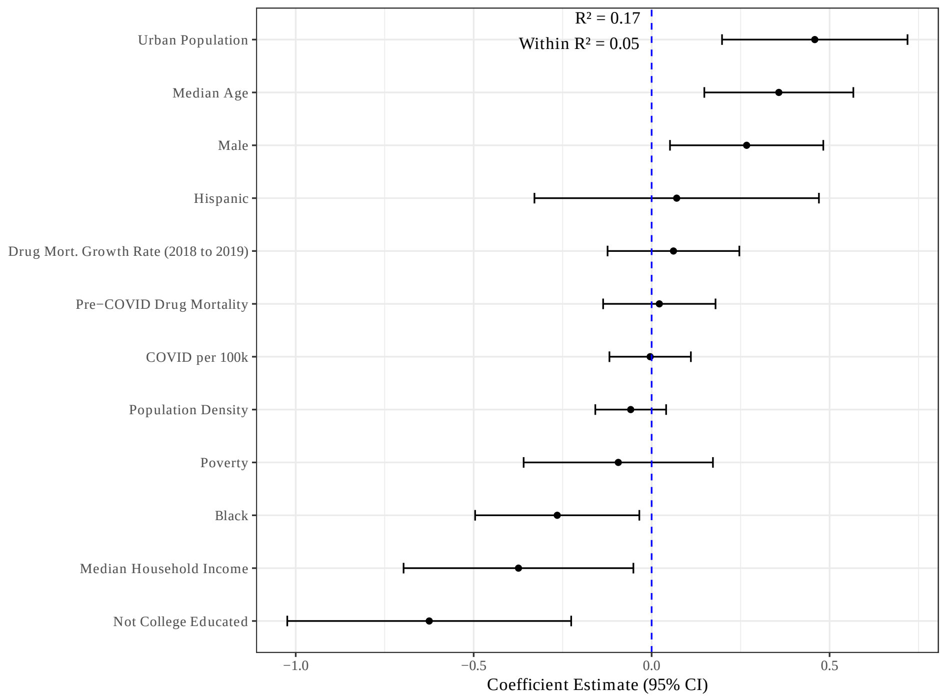
Notes: This figure shows the coefficients when estimating equation (4.4) separately for each calendar year. 95% confidence intervals are based on standard errors clustered at the state level. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes.

A.III Robustness Checks

Figure A.9: Coefficient Estimates When Regressing Unemployment Shock on 2019 Characteristics



(a) Annual County-Level Unemployment Shock



(b) Shift-Share Instrument

Notes: This figure shows the coefficients when regressing the actual annual unemployment shock on a county-level (Figure A.9a) or the predicted annual unemployment shock based on the shift-share instrument (Figure A.9b) on various observable characteristics fixed at 2019 levels. Observable characteristics are transformed to z-scores to ease interpretation. 95% confidence intervals are based on standard errors clustered at the state level.

Table A.2: Risk Factors as Predictors of County Level Industry Employment Shares

	Oil, Gas, Mining	Manufacturing	Hospitality & Food	Arts
Pre-COVID Mortality	0.14	-0.80**	1.08***	0.19***
Poverty (%)	0.84***	-4.34***	-2.24***	-0.57***
Spring 2020 COVID Cases	-0.54**	2.16***	-0.02	0.05
Pre-COVID Unemployment	-0.82***	-0.21	1.30***	0.16***
Pre-COVIDLabor Force Participation	-0.48**	2.60***	-0.42	-0.13*
Mental Distress	-1.08***	5.17***	1.21***	0.17**
High School Only (%)	0.30	5.76***	-3.05***	-0.63***
R^2	0.02	0.17	0.12	0.09

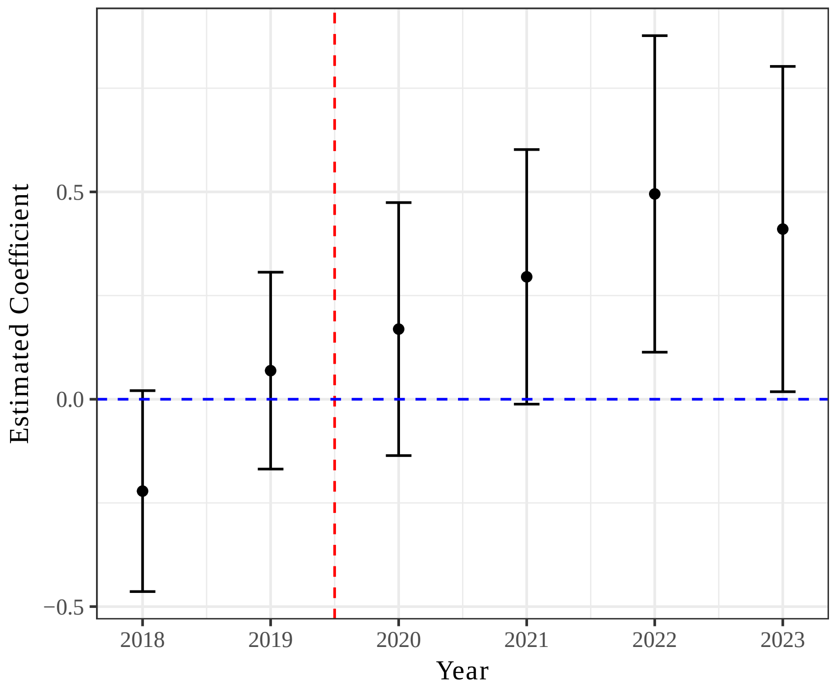
Notes: In all regressions, the dependent variable is the county-level industry employment share in 2019 of the respective industry. Covariates are transformed to z-scores to ease interpretation. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A.3: Impact of Unemployment Shock on Drug Mortality: Alternative Instrument Definitions (Rural Counties)

	Oil, Gas, Mining	Manufacturing	Hospitality & Food	Arts
	(1)	(2)	(3)	(4)
Unemployment Shock	1.133***	0.954***	0.171	1.322***
	(0.308)	(0.302)	(0.380)	(0.466)
N	5804	5804	5792	5804
R^2	0.62	0.62	0.63	0.62
Within R^2	0.41	0.41	0.42	0.4
First Stage F-stat	719.92	607.23	518.57	406.06

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and a full set of controls as specified in Appendix B.I. In each column, the unemployment shock is instrumented by the shift-share instrument which ignores the industry mentioned in the column header. Additionally, each column controls for baseline (2019) employment in the excluded industry. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Figure A.10: Effect of Unemployment Shock on Drug Mortality Separately Estimated for Each Calendar Year, OLS



Notes: This figure shows the coefficients when separately estimating an OLS regression of county-level drug mortality on the initial Covid-19 unemployment shock between February and April 2020 for each calendar year. 95% confidence intervals are based on standard errors clustered at the state level.

Table A.4: Characteristics of Compliers and Non-Compliers

	Non-Complier	Complier	Difference
Panel A: Demographics			
Median Age	42.64	42.89	0.25
Pct. With Less Than High-School Degree	12.39	12.98	0.59
Population Density	41.25	47.55	6.31
Panel B: Economic Characteristics			
Median Household Income (k USD)	50.98	50.08	-0.91
Poverty Pct.	15.54	16.09	0.55
Unemployment Rate	4.22	4.13	-0.09
Social Vulnerability Index	0.52	0.54	0.02
Panel C: Healthcare Access			
Physician Density	0.09	0.09	-0.00
Mental Health Provider Density	156.79	134.20	-22.59*
Panel D: Employment			
Spring 2020 Unemp. Shock	5.26	5.41	0.15
2019 Annual Unemp.	4.12	4.02	-0.09
2021 Annual Unemp.	4.62	4.60	-0.02
Empl. Share Hospitality and Food	10.01	8.89	-1.12*
Empl. Share Manufacturing	17.84	20.76	2.92**
Industry Empl. Concentration Index	0.14	0.13	-0.01
N	1578	395	
Total Population (million)	35.66	10.38	

Notes: Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Compliers are defined as counties where the absolute difference between the predicted unemployment shock and the actual unemployment shock is in the bottom 20 percentile of the distribution. Non-compliers are defined as counties which are not compliers. The table shows the mean of the respective variable for compliers and non-compliers, respectively. The last column shows the difference between average values of compliers and non-compliers with stars indicating the p-value of a t-test testing the null hypothesis that the means of compliers and non-compliers are equal. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Dynamic Controls

Table A.5: Impact of Unemployment Shock on Drug Mortality, Dynamic Controls, All Counties

	OLS	Reduced Form	IV
	(1)	(2)	(3)
Unemployment Shock	0.095	-	-0.143
	(0.155)	-	(0.228)
Predicted Unemployment	-	-0.052	-
	-	(0.084)	-
N	9305	9305	9305
R^2	0.77	0.77	0.77
Within R^2	0.68	0.68	0.68
First Stage F-stat	–	–	1939.51

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. Observations are weighted by population size. Controls are, where useful and available, time-dynamic. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A.6: Impact of Unemployment Shock on Drug Mortality, Dynamic Controls, Rural Counties

	OLS	Reduced Form	IV
	(1)	(2)	(3)
Unemployment Shock	0.387**	-	0.879***
	(0.147)	-	(0.269)
Predicted Unemployment	-	0.222***	-
	-	(0.067)	-
N	5840	5840	5840
R^2	0.64	0.64	0.63
Within R^2	0.42	0.42	0.42
First Stage F-stat	–	–	830.68

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. Observations are weighted by population size. Controls are, where useful and available, time-dynamic. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Alternative Instrument Definitions

Table A.7: Alternative Instrument Definition: Using Year-Over-Year Changes in National Industry Employment

	OLS (All)	OLS(Rural)	Reduced Form (All)	Reduced Form (Rural)	IV (All)	IV (Rural)
	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment Shock	0.102	0.400***	-	-	-0.095	0.980***
	(0.159)	(0.147)	-	-	(0.201)	(0.273)
Predicted Unemployment	-	-	-0.052	0.354***	-	-
	-	-	(0.111)	(0.097)	-	-
N	9305	5840	9305	5840	9305	5840
R^2	0.76	0.63	0.76	0.62	0.76	0.62
Within R^2	0.66	0.42	0.66	0.41	0.66	0.41
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time frame	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023
First Stage F-stat	–	–	–	–	1715.87	691.8

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. Observations are weighted by population size. The unemployment shock is defined as the change in unemployment from February to April 2020. The instrument is constructed using the year-over-year change in national industry employment. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A.8: Alternative Instrument Definition: Using Year-Over-Year Changes in National Industry Employment and Annual Changes in Unemployment As Unemployment Shock

	OLS (All)	OLS(Rural)	Reduced Form (All)	Reduced Form (Rural)	IV (All)	IV (Rural)
	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment Shock	0.505	1.257***	-	-	0.009	1.803***
	(0.347)	(0.343)	-	-	(0.461)	(0.503)
Predicted Unemployment	-	-	-0.052	0.354***	-	-
	-	-	(0.111)	(0.097)	-	-
N	9299	5840	9305	5840	9299	5840
R^2	0.76	0.63	0.76	0.62	0.76	0.63
Within R^2	0.67	0.42	0.66	0.41	0.66	0.42
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Time frame	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023	2021-2023
First Stage F-stat	–	–	–	–	2020.91	1133.01

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. Observations are weighted by population size. The unemployment shock is defined as the year-over-year change in unemployment on the county level from 2019 to 2020. The instrument is constructed using the year-over-year change in national industry employment. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A.9: Controlling for Pre-Pandemic Hospitality Services Employment

	Control Hospitality Employment
	(1)
Unemployment Shock	0.967***
	(0.357)
Predicted Unemployment	-
	-
N	5840
R^2	0.62
Within R^2	0.41
Controls	Yes
Time frame	2021-2023
First Stage F-stat	601.67

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. Observations are weighted by population size. In addition to the usual controls, I control for the pre-pandemic share of employment in hospitality services while allowing for a custom coefficient of hospitality employment in each calendar year. The instrument is constructed as outlined in Section 4 using all industries. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table A.10: Impact of Unemployment Shock on Drug Mortality: Alternative Instrument Definitions (Rural Counties)

	Accomodation (721)	Food Services (722)
	(1)	(2)
Unemployment Shock	0.773*	0.477*
	(0.453)	(0.274)
N	5228	5630
R^2	0.63	0.63
Within R^2	0.42	0.42
First Stage F-stat	246.68	903.01

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and a full set of controls as specified in Appendix B.I. In each column, the unemployment shock is instrumented by the shift-share instrument which ignores the industry mentioned in the column header. Additionally, each column controls for baseline employment in the excluded industry. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Regional Variations in Estimated Effects

Table A.11: Impact of Unemployment Shock on Drug Mortality, Excluding One Division at a Time

	East South Central	Mountain	West South Central	Pacific	South Atlantic	East North Central	West North Central	New England	Middle Atlantic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment Shock	0.821***	0.870***	0.963***	0.945***	0.797***	0.945***	0.902***	0.955***	0.981***
	(0.282)	(0.308)	(0.300)	(0.311)	(0.289)	(0.275)	(0.290)	(0.265)	(0.261)
N	5117	5196	4895	5648	5018	5051	4373	5744	5678
R^2	0.64	0.63	0.63	0.61	0.61	0.62	0.62	0.62	0.63
Within R^2	0.42	0.42	0.43	0.4	0.42	0.39	0.42	0.41	0.41
First Stage F-stat	736.42	659.42	677.01	638.42	759.86	946.77	644.61	753.5	839.1

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column is estimated when excluding all counties who are in the state corresponding to the specific division. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Observations are weighted by population size. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

A.IV Heterogeneity Analysis in the Full Sample

I re-estimate the key heterogeneity dimensions identified in my rural sample analysis in the complete sample.

Table A.12: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Select Dimensions in the Full Sample

	Social Associations	Poverty Pct	Mental Distress Index	Mental Care Provider Density	Social Vulnerability Index
	(1)	(2)	(3)	(4)	(5)
Unemployment Shock	0.026	0.020	-0.156	-0.187	-0.191
	(0.237)	(0.245)	(0.236)	(0.217)	(0.245)
Unemp. shock × Heterogeneity	0.329	0.742	0.286***	0.066	0.151
	(0.207)	(0.477)	(0.095)	(0.068)	(0.328)
N	9305	9305	9305	8645	9305
R^2	0.78	0.77	0.77	0.78	0.77
Within R^2	0.68	0.67	0.67	0.68	0.68
First Stage F-stat	1075.21	971.95	970.88	923.37	1069.05

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

A.V Double Heterogeneity

I run a double-heterogeneity setup whereby I account for pre-pandemic poverty percentage and a secondary heterogeneity dimension simultaneously. This allows me to investigate whether the presumed economic vulnerability effect persists when accounting for these alternative mechanisms.

Table A.13: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Double Heterogeneity

	Injury Deaths	Mental Health Provider Density	Social Associations	State Economic Support	Living Alone Pct.
	(1)	(2)	(3)	(4)	(5)
Unemployment Shock	1.013***	0.987***	0.973***	0.988***	1.041***
	(0.337)	(0.332)	(0.334)	(0.345)	(0.320)
Unemp. shock × Heterogeneity	0.773**	0.827*	0.787**	0.816**	0.964**
	(0.383)	(0.438)	(0.361)	(0.377)	(0.371)
Unemp. shock × 2nd Heterogeneity	0.356***	-0.454	-0.567**	-0.231*	-0.187
	(0.125)	(0.293)	(0.256)	(0.134)	(0.118)
N	5840	5273	5840	5840	5840
R^2	0.63	0.63	0.62	0.63	0.63
Within R^2	0.41	0.41	0.4	0.41	0.4
First Stage F-stat	338.02	349.44	365.72	349.08	351.68

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. The first heterogeneity dimension is pre-pandemic poverty percentage, transformed to a Z-score. The second heterogeneity dimension in each column is constructed by normalising the variable mentioned in the column name. The model is estimated while controlling for the heterogeneity dimension, including a pure effect of the unemployment shock and an interaction effect of the unemployment shock and each heterogeneity dimension. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

B Additional Data Descriptions

B.I Set of Control Variables

Control variables are fixed at 2019 values unless otherwise specified. Some variables contain missing values. In this case, a dummy flag variable is introduced for missing observations.

Demographic Controls

- *Population Density*: Population per square mile. Data from US Census Bureau, Population Estimates 2019
- *Median Age*: Data from US Census Bureau, County Population by Characteristics 2019
- *Percentage Urban Population*: Percentage of the population living in urban areas (US Census Bureau, County Population by Characteristics 2019)
- *Ethnic Composition*: Percentage of the population identifying as black and hispanic, respectively (two separate control variables). Data from US Census Bureau, County Population by Characteristics 2019
- *Male Percentage*: Data from US Census Bureau, County Population by Characteristics 2019
- *Relative Net Migration*: Net population migration inflow in 2019 relative to population size. Data from US Census Bureau, County-to-County Migration Flows

Economic Controls

- *Not College Educated Pct*: Percentage of the population with less than a college degree, Data from US Census Bureau, American Community Survey from 2018 to 2022, 5-year estimates
- *Median Income*: County-level median household income in 2019. Data from Small Area Income and Poverty Estimates Program (SAIPE)
- *Poverty Rate*: Percentage of population living in poverty in 2019. Data from Small Area Income and Poverty Estimates Program (SAIPE)
- *Pre-pandemic social vulnerability index*: Social vulnerability index percentile ranking in 2019. These rankings are computed based on percentiles for 16 socioeconomic indicators such as poverty, education levels; household characteristics such as single-parent households; racial and ethnic minorities; housing type and infrastructure data. Data

from CDC/ATSDR Social Vulnerability Index (Centers for Disease Control and Prevention, 2025a)

- *Percentage Uninsured*: Percentage of the population without health insurance in 2019. Data from US Census Bureau, Small Area Health Insurance Estimates (SAHIE)
- *Number of social associations*: Number of social associations per 1000 population, including religious organizations, civic organizations, and other non-business associations in 2019. Data from County Business Patterns, accessed via County Health Rankings
- *Annual average unemployment rate*: County-level unemployment rate in 2019. Data from Bureau of Labor Statistics, LAUS
- *Trend in unemployment rate*: Absolute change in unemployment rate average over 2010 to 2012 compared to average over 2017 to 2019. Data from Bureau of Labor Statistics, LAUS
- *Labor Force Participation Rate*: Labor force in 2019 divided by population aged 16 or above in 2019, data from Bureau of Labor Statistics, LAUS and US Census Bureau, Population Estimates
- *Trend in Labor Force Participation Rate*: Absolute changes in labor force participation rate from 2010 to 2019. Labor force participation is defined as above.

Drug-related Controls. In general, drug mortality data is from CDC WONDER with drug mortality defined based on ICD-10 codes X40-X44, X60-X64, X85, and Y10-Y14, as the number of drug overdose deaths per 100,000 population.

- *2019 Drug Mortality Rate*
- *2018 Drug Mortality Rate*
- *2017 Drug Mortality Rate*
- *Longer-term Drug Mortality Trend*: Absolute change in drug mortality rates from averages over 1999 to 2003 to averages over 2010 to 2016
- *Recent Drug Mortality Growth*: Relative change in drug mortality from 2018 to 2019
- *Initial Average Drug Mortality*: Average drug mortality rate between 1999 and 2003

- *Opioid Prescriptions in 2010* CDC Opioid Prescription Data for 2010 and 2015. Estimates of the number of opioid prescriptions dispensed in the United States based on a sample of a subset of pharmacies, representing 88 percent of prescriptions in the United States. (Schuchat et al., 2017)

- *Opioid Prescriptions in 2015*: See above

Dynamic COVID-19 Controls:

- *COVID-19 Deaths, annually*: Data from The New York Times

- *COVID-19 Cases, annually*: See above.

Other Controls

- *State-Year Fixed Effects*

- In heterogeneity analysis, whenever the heterogeneity dimension was not previously in the set of controls, I additionally control for the heterogeneity dimension

B.II Data for Heterogeneity Dimensions and Intermediate Outcomes

I list the variable definitions and sources for variables used as heterogeneity dimensions or intermediate outcomes. Data sources for heterogeneity dimensions which are already included as controls in the basic levels are introduced in subsection B.I and not restated.

Heterogeneity Dimensions

- *Pre-pandemic Drug Mortality*: Drug mortality rates in 2019. Data from CDC Wonder. Drug mortality is defined as the number of drug overdose deaths per 100,000 population, based on ICD-10 codes X40-44, X60-64, X85, and Y10-Y14.

- *Pre-pandemic Opioid Prescriptions*: Opioid prescriptions per capita in 2019. Data from CDC

- *Pre-pandemic Mental Distress*: Percentage of the population reporting frequent mental distress in 2019. Data based on CDC places, model-based county-level estimates based on survey responses (Centers for Disease Control and Prevention, 2025b)

- *Single-person Households*: Percentage of the population living in single-person households. Data from US Census Bureau, Population and Housing Unit Estimates, 2019.

- *Physical Injury Death Rates*: Number of injury deaths per 100,000 in 2019. Data from National Center for Health Statistics - Mortality Files accessed through County Health Rankings.

- *Mental Health Provider Density*: Number of mental health providers per 100,000 population in 2019. Data from CMS, National Provider Identification access through County Health Rankings

- *Broadband Access*: Percentage of the population with access to broadband internet in 2019. Data from US Census Bureau, American Community Survey, 5-year estimates

- *Employer-sponsored health insurance*: Percentage of the population with employer-sponsored health insurance in 2019. Data from US Census Bureau, American Community Survey, 5-year estimates

- *State Economic Support*: A measure of relative generosity of state-level economic support from June to December 2020. Data from Oxford COVID-19 Government Response Tracker (Hale et al., 2021)

- *Living Alone Percentage*: Percentage of the population living alone in 2019. Data from US Census Bureau, American Community Survey, 5-year estimates

- *State Policy Stringency*: A measure of stringency of state-level COVID-19 social isolation policies during 2020. Data from Oxford COVID-19 Government Response Tracker (Hale et al., 2021)

- *2020 Unemployment Persistence*: Average unemployment rate of 2020 divided by initial unemployment shock, defined as change in unemployment rate from February to April 2020. Data from Bureau of Labor Statistics, LAUS

- *2022 Unemployment*: Annual 2022 average unemployment rate. Data from Bureau of Labor Statistics, LAUS

- *Employment Share in Largest Industry*: Relative share of total workforce employed in largest single 3-digit NAICS industry in 2019

- *Industry HHI 2019*: Herfindahl-Hirschman index of employment share in 3-digit NAICS industries in 2019. The HHI is defined as the sum of the squared employment shares of each industry.

- *UI Replacement Rate*: State-level estimates of relative unemployment insurance replacement rates in 2020. Data from Ganong et al. (2020)

- *Credit Constraint Percentage*: Composite measure of credit constraints based on credit over-utilisation, delinquency history, subprime credit scores, and no access to credit cards or home equity credit line, 2019 estimates. Data from Scott et al. (2025)

- *Credit Insecurity Index*: 2019 composite measure of credit insecurity based on percentage of adult population with credit file and credit score, credit utilisation rates, delinquencies and other credit access measures. Data from Scott et al. (2025)

Intermediate Outcomes

- *Social Vulnerability Index Ranking 2022:* These rankings are computed based on percentiles for socioeconomic indicators such as poverty, education levels; household characteristics such as single-parent households; racial and ethnic minorities; housing type and infrastructure data. Data from CDC/ATSDR Social Vulnerability Index (Centers for Disease Control and Prevention, 2025a)

- *Healthcare Checkup*: Percentage of the population reporting having had a healthcare check-up in 2021. County-level estimates based on survey responses, year 2021. Data from CDC PLACES estimates (Centers for Disease Control and Prevention, 2025b)

- *Uninsured Adults*: Percentage of the population without health insurance in 2019, 2021 and 2022, by income levels. Data from US Census Bureau, Small Area Health Insurance Estimates (SAHIE)

- *Debt-to-Income Ratio*: Household-level debt-to-income ratio in 2021. Data from Federal Reserve, Z.1 Financial Accounts of the United States.

- *Food Insecurity Percentage*: Model-based estimates of percentage of population with inadequate access to food based on survey responses from the Current Population Survey about food insecurity and socioeconomic characteristics. Data from Feeding America, Map the Meal Gap

- *Google Mobility Data*: Google mobility data for retail and recreation, grocery and pharmacy, parks, transit stations, workplaces, and residential areas. The data is provided as a percentage change from the baseline, which is defined as the median value for the corresponding day of the week during the 5-week period from January 3 to February 6, 2020. Data from Google.

- *Feeling Lonely*: Percentage of the population reporting to have felt lonely. County-level estimates based on survey responses, year 2021. Data from CDC PLACES estimates (Centers for Disease Control and Prevention, 2025b)
- *Lacking Support*: Percentage of the population reporting to feel like they are lacking support. County-level estimates based on survey responses, year 2021. Data from CDC PLACES estimates (Centers for Disease Control and Prevention, 2025b)

C Theoretical Model

Conditions for entry equilibria.

For simplicity, as these quantities are initially fixed, we introduce the notation $A_i \equiv \alpha_i + \beta S_i^2$.

Proposition 1. *The foreign firm decides to enter market i if*

$$\frac{\left(A_i + \gamma c_i^d - 2\gamma c_i^f\right)^2}{9\gamma} \geq F_i \tag{C.1}$$

and the domestic firm does not employ limiting pricing, which requires

$$\left[\frac{\gamma c_i^f + 2\sqrt{\gamma F_i}}{\gamma} - c_i^d\right]\left(A_i - \gamma c_i^f - 2\sqrt{\gamma F_i}\right) \geq \frac{\left(A_i + \gamma c_i^f - 2\gamma c_i^d\right)^2}{9\gamma}.$$

Additionally, both firms produce positive quantities if

$$A_i > 2\gamma c_i^d - \gamma c_i^f, \tag{C.2}$$

$$A_i > 2\gamma c_i^f - \gamma c_i^d \tag{C.3}$$

and the foreign firm cannot produce a quantity leading to a corner solution for the domestic firm, requiring

$$A_i > 2\gamma c_i^d - \gamma c_i^f \text{ and } A_i \leq \gamma c_i^f,$$

The equilibrium quantities are then given by

$$q_i^d = \frac{\alpha_i + \beta_i S_i^2 - 2\gamma c_i^d + \gamma c_i^f}{3}, \quad q_i^f = \frac{\alpha_i + \beta_i S_i^2 - 2\gamma c_i^f + \gamma c_i^d}{3}. \tag{C.4}$$

Proof. Notably, below we will repeatedly check first-order conditions. These conditions are necessary and sufficient for optima as profits are quadratic and strictly concave in own quantities.

We write the aggregate demand in form of an inverse demand function as

$$p_i = \frac{\alpha_i + \beta_i S_i^2 - Q_i}{\gamma}$$

and introduce the notation $A_i \equiv \alpha_i + \beta_i S_i^2$.

Entry and Cournot competition. If both firms decide to enter, fixed costs of the foreign supplier have been spent. Consequently, only the variable profit portion is relevant for subsequent decision making. The first order condition for each firm $j \in \{d, f\}$, taking the quantities of the other firm as given, is

$$\frac{\partial \pi_i^j}{\partial q_i^j} = \frac{\partial p_i}{\partial q_i^j} q_i^j + (p_i - c_i^j) = -\frac{q_i^j}{\gamma} + \frac{\alpha_i + \beta_i S_i^2 - Q_i}{\gamma} - c_i^j = 0$$

We denote by q_i^j the quantity of the firm and by q_i^{-j} the quantity of the other firm. Plugging in $Q_i = q_i^j + q_i^{-j}$, we solve for q_i^j, leading to the best response function:

$$\frac{1}{2}\left[A_i - \gamma c_i^j\right] - \frac{1}{2} q_i^{-j} = q_i^j$$

Using the analoguous result for the quantity q_i^{-j} and plugging in, we obtain

$$q_i^j = \frac{1}{2}\left[A_i - \gamma c_i^j\right] - \frac{1}{4}\left(A_i - q_i^j - \gamma c_i^{-j}\right)$$

which we can solve for

$$q_i^j = \frac{\alpha_i + \beta_i S_i^2 - 2\gamma c_i^j + \gamma c_i^{-j}}{3}$$

The corresponding market price is then given by

$$p = \frac{A_i + \gamma c_i^d + \gamma c_i^f}{3\gamma}$$

The variable profit of the foreign firm is then given by

$$\tilde{\pi}_i^f = \frac{\left(A_i + \gamma c_i^d - 2\gamma c_i^f\right)^2}{9\gamma}$$

which yields the foreign entry condition

$$\frac{\left(A_i + \gamma c_i^d - 2\gamma c_i^f\right)^2}{9\gamma} \geq F_i.$$

Positivity of the equilibrium quantities further requires the technical conditions that

$$A_i > 2\gamma c_i^d - \gamma c_i^f \text{ and } A_i > 2\gamma c_i^f - \gamma c_i^d$$

No entry. If no entry occurs, the local producer acts as a monopolist. The first-order condition is given by

$$\frac{d\pi_i^d}{dp_i} = (A_i - \gamma p_i) - \gamma\left(p_i - c_i^d\right) = 0$$

which we can solve for the profit-maximising price

$$p_i^* = \frac{A_i + \gamma c_i^d}{2\gamma}$$

and the corresponding quantity, using market-clearing,

$$Q_i^* = A_i - \gamma p_i^* = \frac{A_i - \gamma c_i^d}{2}.$$

Entry deterrence through exclusionary pricing. Alternatively, the domestic supplier may decide to set a price which deters entry, which is known as limit pricing. Entry deterrence requires that the foreign firm is not able to cover its fixed costs and the domestic firm finds the limit pricing more profitable than outcomes under Cournot competition.

We have derived the best response function as

$$\frac{1}{2}\left[A_i - \gamma c_i^f\right] - \frac{1}{2}q_i^d = q_i^f.$$

We plug this into the (total) profit function of the foreign firm,

$$\pi_i^f = \left(\frac{A_i - q_i^d - \frac{1}{2}\left[A_i - \gamma c_i^f - q_i^d\right]}{\gamma} - c_i^f\right)\frac{1}{2}\left[A_i - \gamma c_i^f - q_i^d\right] - F_i.$$

Entry deterrence requires setting a quantity such that the total profit of the foreign firm is weakly negative. Thus, we simplify the above expression and solve the weak inequality for

q_i^d:

$$\frac{\left[A_i - \gamma c_i^f - q_i^d\right]^2}{4\gamma} \leq F_i$$

yielding the roots

$$q_i^d = A_i - \gamma c_i^f + 2\sqrt{\gamma F_i} \quad \text{or} \quad q_i^d = A_i - \gamma c_i^f - 2\sqrt{\gamma F_i}.$$

The profit of the foreign firm is negative between the two roots. If the monopoly quantity lies between the two roots, the firm will choose the monopoly quantity, and this monopoly quantity is sufficient for entry deterrence. Otherwise, the firm will choose the bound value, maximizing its own profit, which will generally be the smaller root. This quantity is nonnegative if $\alpha_i + \beta_i S_i^2 \geq \gamma c_i^f + 2\sqrt{\gamma F_i}$. Under this limit quantity, the foreign firm will not enter the market, and the market price will be given by

$$p_i = \frac{\gamma c_i^f + 2\sqrt{\gamma F_i}}{\gamma}$$

leading to a profit of

$$\pi_i^d = \left[\frac{\gamma c_i^f + 2\sqrt{\gamma F_i}}{\gamma} - c_i^d\right]\left(A_i - \gamma c_i^f - 2\sqrt{\gamma F_i}\right)$$

For the threat of exclusionary pricing to be credible and this quantity to be a subgame perfect Nash equilibrium, the domestic firm must be better off than under Cournot competition. This is the case if

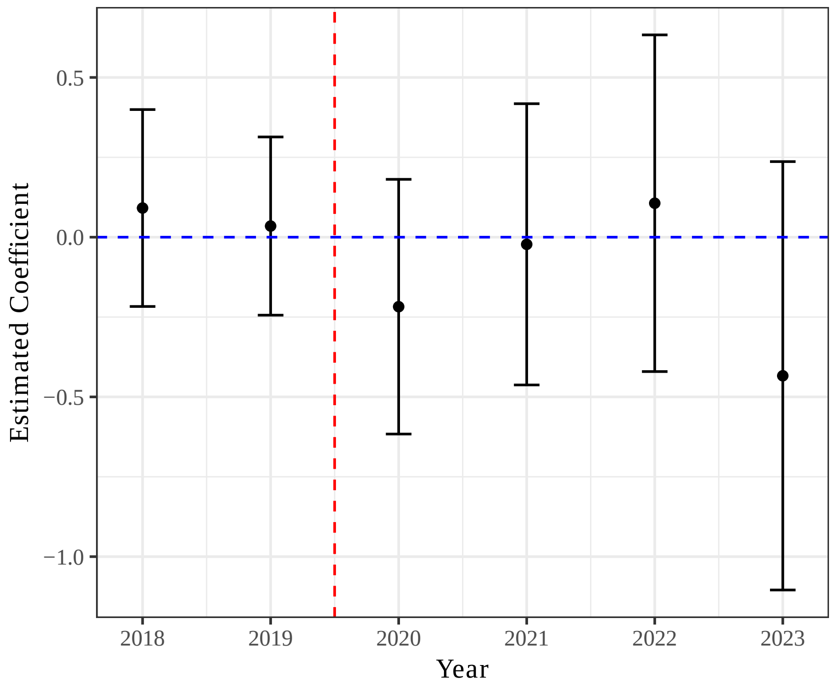
$$\left[\frac{\gamma c_i^f + 2\sqrt{\gamma F_i}}{\gamma} - c_i^d\right]\left(A_i - \gamma c_i^f - 2\sqrt{\gamma F_i}\right) \geq \frac{\left(A_i + \gamma c_i^f - 2\gamma c_i^d\right)^2}{9\gamma}$$

□

D Robustness Checks in the Full Sample

I repeat certain robustness checks from the main text, which were restricted to the rural sample, in the full sample.

Figure D.1: Event Study Specification of the Effect of the COVID-19 Unemployment Shock on Drug Mortality



Notes: This figure shows the coefficients when estimating equation (4.4) separately for each calendar year. 95% confidence intervals are based on standard errors clustered at the state level.

Table D.1: Risk Factors as Predictors of County-Level Industry Employment Shares

	Oil, Gas, Mining	Manufacturing	Hospitality & Food	Arts
Pre-COVID Mortality	0.06	-1.55***	0.78***	0.17***
Poverty (%)	0.91***	-3.56***	-1.76***	-0.57***
Spring 2020 COVID Cases	-0.35***	0.70***	-0.19	0.01
Pre-COVID Unemployment	-0.62***	-0.19	0.92***	0.11**
Pre-COVIDLabor Force Participation	-0.52***	2.57***	-0.53***	-0.22***
Mental Distress	-1.18***	4.76***	1.04***	0.11*
High School Only (%)	0.50***	5.69***	-2.45***	-0.57***
R^2	0.03	0.18	0.11	0.10

Notes: In all regressions, the dependent variable is the county-level industry employment share in 2019 of the respective industry. Covariates are transformed to *z*-scores to ease interpretation. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table D.2: Impact of Unemployment Shock on Drug Mortality: Alternative Instrument Definitions

	Oil, Gas, Mining	Manufacturing	Services	Arts
	(1)	(2)	(3)	(4)
Unemployment Shock	-0.044	-0.148	-0.570*	-0.264
	(0.248)	(0.210)	(0.331)	(0.266)
N	9266	9266	9251	9266
R^2	0.76	0.76	0.76	0.76
Within R^2	0.66	0.67	0.66	0.66
First Stage F-stat	1870.35	1520.36	671.3	1084.22

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and a full set of controls as specified in Appendix B.I. In each column, the unemployment shock is instrumented for using the shift-share instrument which ignores the industry mentioned in the column header. Additionally, each column controls for baseline employment in the excluded industry. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table D.3: Impact of Unemployment Shock on Drug Mortality, Excluding One Division at a Time

	East South Central	Mountain	West South Central	Pacific	South Atlantic	East North Central	West North Central	New England	Middle Atlantic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment Shock	-0.084	-0.111	-0.282	-0.174	-0.093	-0.165	-0.156	-0.127	-0.001
	(0.240)	(0.274)	(0.216)	(0.277)	(0.302)	(0.235)	(0.240)	(0.232)	(0.229)
N	8213	8466	7895	8894	7541	7994	7454	9128	8855
R^2	0.77	0.77	0.76	0.77	0.74	0.76	0.76	0.76	0.76
Within R^2	0.68	0.67	0.68	0.67	0.63	0.66	0.66	0.66	0.66
First Stage F-stat	1736.14	1471.67	1722.82	1664.15	1669.69	1996.55	1668.81	1893.63	1794.57

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column is estimated when excluding all counties that are in the state corresponding to the specific division. Observations are weighted by population size. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

E Additional Heterogeneity Analyses and Intermediate Outcomes

E.I Economic Vulnerability

I measure economic vulnerability using the percentage of the population living in poverty and a survey-based measure of pre-pandemic frequent mental distress. I also consider a composite index of social vulnerability constructed by the CDC, which includes various demographic and economic vulnerabilities. The index is constructed based on relative rankings in 16 different characteristics measuring socioeconomic status, household characteristics, ethnicities, housing type and transportation and measures a percentile of social vulnerability risk.

In addition to the tables presented in the main text, I provide additional intermediate outcomes and descriptive figures showing differential access to credit markets and unemployment benefits in rural and urban counties.

Table E.1 shows the results of regressing the instrumented unemployment shock on debt-to-income ratios (on a county-level) in 2021, while controlling for pre-pandemic debt-to-income ratios. The results show a significant positive association between the unemployment shock and debt-to-income ratios in urban counties but not in rural counties.

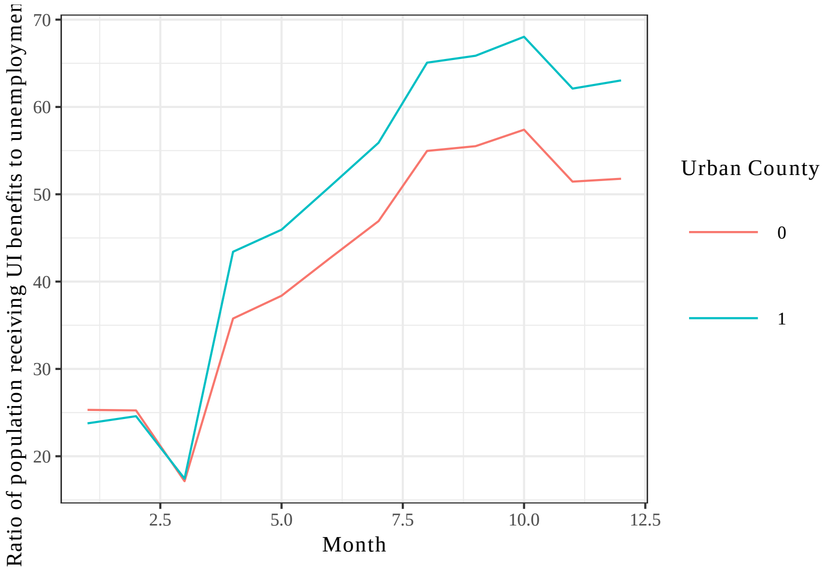
Table E.1: Impact of Unemployment Shock on Debt-to-Income Ratio

	Δ Debt to Income Ratio	
	All	Rural
Unemployment Shock	0.028***	0.008
	(0.008)	(0.007)
N	3100	1945
R^2	0.96	0.89
Within R^2	0.95	0.86
First Stage F-stat	671.99	272.23

Notes: In all regressions, the dependent variable is the 2021 debt-to-income ratio in the county. In each regression, the unemployment shock is instrumented for using the shift-share instrument. Observations are weighted by population size. All regressions include state fixed effects and the full set of controls as specified in Appendix B.I. I additionally control for pre-pandemic debt-to-income ratios. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Figure E.1 additionally shows the relative share of the population receiving unemployment benefits in California, using the ratio of the number of unemployment claimants per week over the total population, divided by the unemployment rate. This measure is used as a proxy for access to unemployment benefits. The figure shows that rural counties have a significantly lower ratio than urban counties, indicating that rural counties have less access to unemployment benefits.

Figure E.1: Unemployment Benefit Takeup in Rural and Urban Counties in California



Notes: This figure shows the ratio of the number of unemployment claimants per week over the total population, divided by the unemployment rate. I use this measure as a proxy for access to unemployment benefits. The figure shows the ratio for urban and rural counties separately. Due to data availability, the figure is restricted to counties in the state of California.

E.II Labor Market Recovery and Unemployment Benefits

I hypothesize that unemployment persistence increases the effect of the unemployment shock on drug mortality, as a continuous spell in unemployment with less generous unemployment benefits is likely to accumulate stress, increasing the risk of drug abuse. Similarly, less flexible labor markets may impede the likelihood of re-employment thus also leading to accumulated stress.

As a first-order approximation, I employ the annual 2022 employment rate as a proxy for the persistence of the initial unemployment shock as well as the ratio of annual 2020 unemployment to Spring 2020 unemployment as a measure of persistence throughout 2020. The evaluation of such a heterogeneity dimension is more complicated as I use a post-shock variable to define heterogeneity, contrary to my other heterogeneity results above. The literature suggests that higher unemployment persistence may also be driven by drug abuse, potentially confounding my estimates. I also consider the extent of unemployment benefits. These measurements are constructed on the state level, using the estimated replacement rate based on Ganong et al. (2020). Finally, I use the labor market concentration in specific industries, measured by a Herfindahl-Hirschman index of industry employment in 2019, as a measure of labor market flexibility.

Table E.2 shows the results for the above-discussed heterogeneity dimensions. I do not detect a significant effect. This effect may partially be caused by the post-shock measurement of unemployment persistence. Similarly, I do not find a significant effect of unemployment benefits. This insignificant finding may be attributed to the lack of variation in the data due to the extent of federal support payments over the first pandemic year. Industry concentration measures also fail to yield a significant effect, although both the coefficients on industry shares and on the Herfindahl-Hirschman Index are positive and close to the 10% significance level. My measurements of unemployment persistence and unemployment benefits are noisy proxies. Nevertheless, my null findings seem to indicate that labor market flexibilities between rural counties were not a significant mediator of the effect of the unemployment shock on drug mortality, despite theoretical support for the hypothesis.

Table E.2: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Labor Markets

	2020 Unemployment Persistence	2022 Unemployment	Empl. Share Largest Industry	Industry HHI	UI Replacement Rate
	(1)	(2)	(3)	(4)	(5)
Unemployment Shock	0.873***	0.852***	0.582*	0.646**	0.859***
	(0.265)	(0.266)	(0.321)	(0.300)	(0.272)
Unemp. shock × Heterogeneity	-1.100	0.148	0.288	0.497*	0.062
	(0.780)	(0.155)	(0.194)	(0.291)	(0.106)
N	5840	5840	5804	5804	5840
R^2	0.64	0.63	0.64	0.64	0.63
Within R^2	0.42	0.42	0.42	0.42	0.42
First Stage F-stat	436.33	421.81	418.82	439.21	471.46

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

E.III Social Isolation

As proxies for higher social isolation risk, I use measurements of pre-pandemic broadband access, pre-pandemic mental distress, the share of households living alone, and the Oxford state policy stringency index.

Table E.3: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Social Isolation

	Broadband Access	Mental Distr.	Living Alone Pct	State Policy String.
	(1)	(2)	(3)	(4)
Unemployment Shock	0.965***	1.009***	0.867***	0.909***
	(0.317)	(0.367)	(0.272)	(0.271)
Unemp. shock × Heterogeneity	-0.454*	0.675***	-0.220	-0.096
	(0.249)	(0.223)	(0.204)	(0.185)
N	5840	5840	5840	5840
R^2	0.63	0.63	0.63	0.63
Within R^2	0.41	0.41	0.42	0.42
First Stage F-stat	544.67	419.83	463.55	428.51

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

I also measure the association between survey-based outcomes, such as the percentage of respondents indicating feelings of loneliness or a lack of support, and the instrumented unemployment shock. These survey outcomes are based on the CDC PLACES project which allocates state-level survey results to county-level data based on statistical models. The survey data is based on the Behavioral Risk Factor Surveillance System (BRFSS). These model-based allocations are likely noisy for smaller, rural counties and should be interpreted with caution.

Table E.4: Impact of Unemployment Shock on Survey-Based Measures of Social Isolation

	(1)	(2)	(3)	(4)
	Feeling Lonely		Lacking Support	
	All	Rural	All	Rural
Unemployment Shock	-0.091	0.025	-0.026	0.068
	(0.073)	(0.045)	(0.055)	(0.046)
Mean	34.19	34.4	25.74	25.78
N	2377	1518	2377	1518
R^2	0.7	0.79	0.9	0.9
Within R^2	0.37	0.35	0.69	0.66
First Stage F-stat	520.43	193.97	520.43	193.97

Notes: In all regressions, the dependent variable is the respective column header. In columns (1) and (2), the dependent variable is the share of respondents indicating that they feel lonely. In columns (3) and (4), the dependent variable is the share of respondents indicating that they feel a lack of support. In each regression, the unemployment shock is instrumented for using the shift-share instrument. All regressions include state fixed effects and the full set of controls as specified in Appendix B.I. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Finally, I measure social isolation using a proxy based on Google mobility data. The data compares relative changes in time spent at home or at retail and recreational locations compared to the pre-pandemic time period. This measure is a proxy for social isolation, as it indicates the percentage of the population which is not able to leave their home for work or leisure activities. I find no significant effect of the unemployment shock on this measure, suggesting that social isolation was not a first-order driver of the effect in rural counties. In urban counties, the unemployment shock had a negative effect on time spent alone. This effect can likely be attributed to telework.

Table E.5: Impact of Unemployment Shock on Mobility Data

	(1)	(2)	(3)	(4)
	Time at Residential		Visit Retail or Recreation	
	All	Rural	All	Rural
Unemployment Shock	-0.125**	0.018	1.179***	1.367**
	(0.048)	(0.065)	(0.416)	(0.639)
Mean	7.47	5.97	−12.31	−11.15
N	1766	793	2560	1469
R^2	0.88	0.63	0.69	0.24
Within R^2	0.81	0.39	0.5	0.07
First Stage F-stat	443.78	163.49	575.52	249.29

Notes: In all regressions, the dependent variable is the respective column header. In columns (1) and (2), the dependent variable is the relative change in time spent at home compared to the pre-pandemic period. In columns (3) and (4), the dependent variable is the change in the time spent at retail or recreational locations compared to the pre-pandemic period. In each regression, the unemployment shock is instrumented for using the shift-share instrument. All regressions include state fixed effects and the full set of controls as specified in Appendix B.I. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

E.IV Healthcare Access

I proxy for health insurance access using state-level information on the percentage of the population on employer-sponsored healthcare plans. This measures a proxy of the potential number of newly unemployed which lose access to their health insurance. I additionally consider a post-shock variable, namely, the percentage of low-income population losing access to health insurance from 2019 to 2020. Lastly, I proxy for self-medication risk using the pre-pandemic injury death rate. Higher injury deaths indicate an increased risk of physical harm and thus an increased risk of using pain medicine such as opioids – particularly if regular healthcare access is limited.

As a secondary channel, I consider the capabilities of accessing mental health services during the pandemic. The rationale behind this mediating channel is that access to professional mental health may reduce the stress associated with the unemployment shock, reducing the risk of drug abuse. I employ two measurements: the population density of mental health providers and broadband access, which facilitates access to telehealth during the pandemic.

Table E.6: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Healthcare

	Δ Low-income uninsured rate	Employer-sponsored Health Insurance	Injury Death Rate	Mental Care Provider Density	Broadband Access
	(1)	(2)	(3)	(4)	(5)
Unemployment Shock	0.860***	0.884***	0.950***	0.856***	0.965***
	(0.316)	(0.277)	(0.320)	(0.261)	(0.317)
Unemp. shock × Heterogeneity	0.724***	-0.079	0.940**	-0.445	-0.454*
	(0.232)	(0.189)	(0.466)	(0.273)	(0.249)
N	5840	5840	5840	5273	5840
R^2	0.63	0.63	0.63	0.64	0.63
Within R^2	0.41	0.42	0.41	0.43	0.41
First Stage F-stat	437.08	417.43	439.45	434.58	544.67

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Table E.7: Impact of Unemployment Shock on Healthcare Utilisation

	(1)	(2)	(3)	(4)
	Health Checkup		Increase Uninsured Adults Pct.	
	All	Rural	All	Rural
Unemployment Shock	0.112	0.022	0.045*	0.070**
	(0.081)	(0.054)	(0.023)	(0.028)
Mean	72.44	71.98	−0.43	−0.43
N	3035	1924	3102	1947
R^2	0.93	0.94	0.43	0.45
Within R^2	0.3	0.33	0.13	0.15
First Stage F-stat	542.43	264.67	646.01	270.88

Notes: In columns (1) and (2), the dependent variable is the percentage of the population that reported having a general health checkup in 2021. In columns (3) and (4), the dependent variable is the change in the percentage of the adult population without health insurance from 2019 to 2021. In each regression, the unemployment shock is instrumented for using the shift-share instrument. All regressions include state fixed effects and the full set of controls as specified in Appendix B.I. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

E.V Other

I investigate heterogeneous effects by pre-pandemic drug mortality and the intensity of the initial COVID-19 wave.

Table E.8: Impact of Unemployment Shock on Drug Mortality With Heterogeneous Effects – Other

	Spring 2020 COVID-19 Cases	2019 Drug Mortality
	(1)	(2)
Unemployment Shock	0.877***	0.851***
	(0.256)	(0.312)
Unemp. shock × Heterogeneity	-0.019	1.028
	(0.220)	(0.780)
N	5840	5840
R^2	0.63	0.62
Within R^2	0.42	0.39
First Stage F-stat	433.55	422.91

Notes: In all regressions, the dependent variable is annual drug mortality per 100,000. All regressions include state-year fixed effects and the full set of controls as specified in Appendix B.I. In all columns, the unemployment shock is instrumented for using the shift-share instrument. Each column's heterogeneity dimension is constructed by normalizing the variable specified in the column name (z-score). The model is estimated while controlling for the heterogeneity dimension, including both the direct effect of the unemployment shock and the interaction effect between the unemployment shock and the heterogeneity dimension. Sample is restricted to rural counties, defined as nonmetro counties based on rural-urban continuum codes. Standard errors are clustered at the state level. Statistical significance: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.10$.

Declaration of Use of AI Assistance

I declare that I have used AI assistance in the preparation of this thesis. AI tools were used to assist with the following tasks:

- *Proofreading and grammar checking*: Checking and correcting grammar and spelling (Grammarly, Google Gemini 2.5 pro, ChatGPT o4-mini(-high))
  - Example prompt: "Please check the following paragraph for grammar and spelling errors and suggest corrections."
- *Data sourcing*: Finding data sources for county-level data on heterogeneity dimensions, controls and intermediate outcomes as well as generating code for transforming unstructured data into tabular data (ChatGPT o4 and Google Gemini 2.5 pro)
  - Example prompt: "I want to measure the persistence of unemployment and the access to unemployment benefits during the covid-19 pandemic on a county-level. There is no nation wide dataset which contains this information but I am considering using state level UI claims as a proxy for the amount of unemployed people receiving assistance, and exhaustion rates or similar as a proxy for duration of unemployment. Your task is the following: Look through all state level websties (either the state itself or the state labor / workforce department) and see what data is available on a county-level about unemployment duration, unemyplomenet benefits recipiency, unemployment benefits exhaustion etc. Data should ideally be available on at minimum a yearly level from 2020 to 2023."
- *Checking mathematical derivations*: Checking mathematical derivations and proofs for correctness (ChatGPT o4-mini(-high))
  - Example prompt: "I attach the model setup for a microeconomic model of drug consumption and my derived equilibrium conditions. Your task is to check the correctness of the derivations and proofs."
- *General feedback*: Providing general feedback on the structure and content of the thesis (ChatGPT o4-mini(-high) and Google Gemini 2.5 pro)
  - Example prompt: "I attach my master thesis in Economics. Give an initial assessment of the thesis including strengths and weaknesses and grade it on the Swiss grading scale, taking into account the expectation of a master's level economics thesis. Give suggestions for refinements."

Statement of Authorship for a Written Thesis at the Department for Economics at the University of Zurich

I declare that this work titled

COVID-19, Unemployment, and Drug Mortality in the US

has been composed by myself, and describes my own work, unless otherwise acknowledged in the text. This work has not been and will not be submitted for any other degree or the obtaining of ECTS points at the University of Zurich or any other institution of higher education. All sentences or passages quoted in this paper from other people's work have been specifically acknowledged by clear cross-referencing to author, work and page(s). Any illustrations which are not the work of the author have been used with the explicit permission of the originator and are specifically acknowledged. I understand that failure to specifically acknowledge all used work amounts to plagiarism and will be considered grounds for failure and will have judicial and disciplinary consequences according §7ff of the 'Disziplinarordnung der Universität Zürich' as well as §27 of the 'Rahmenverordnung für das Studium in den Bachelor- und Master-Studiengängen der Wirtschaftswissenschaftlichen Fakultät der Universität Zürich'. With my signature I declare the accuracy of these specifications.

Ralf Blöchlinger, 19-612-548

Zurich, July 19, 2025