The Indirect Effects of Opioid Supply on U.S. Suicide and Alcohol-Related Deaths

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Abstract

We provide evidence that drug abuse resulting from the opioid epidemic contributed to rising non-drug suicide (NDS) and alcohol-related mortality (ARM) rates during the last 20 years. A relationship between the opioid epidemic and these non-drug deaths can exist due to either the interaction between drug abuse and suicidal tendencies or alcohol abuse, or healthcare and social service shortages generated from the substantial burden of treating drug abuse and overdose cases. Our analysis exploits three well-established opioid medication supply shifters shortly before the opioid epidemic broke out that are used in recent economics studies: triplicate laws, cancer mortality, and Social Security disability insurance benefits. Results suggest that the opioid epidemic contributed 30-60% to NDS and ARM increases through 2019. This sheds light on additional adverse secondary effects of the opioid epidemic.

Keywords: Opioid Epidemic, Suicide, Alcohol-Related Mortality

JEL Classification: I12, I18, I31

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1 Introduction

U.S. mortality rates due to non-drug suicide (NDS) and alcohol-related mortality (ARM) have risen for nearly all of the past 25 years. National NDS rates increased by over 50% between 2000 and 2020, while ARM rates more than doubled during this period (see Figure 1). Case and Deaton (2015) identified rising trends in suicide broadly and alcohol-liver disease mortality that coincided with rising trends in deaths from drug overdoses. They argued that these are the result of adverse changes in social and economic factors for specific population segments in the U.S. and named them "deaths of despair". In line with the deaths of despair hypothesis, several studies have found that demand-side factors contributed to rising suicide and/or ARM rates, including declines in religiosity (Giles et al., 2023), a decline in the social safety net available to disadvantaged families (Lichtman-Sadot and Slusky, 2024), or a depreciation of the minimum wage (Dow et al., 2020).

We offer a novel different explanation for rising NDS and ARM rates that is supply-driven rather than demand-driven: the opioid epidemic. We show that the opioid epidemic contributed significantly and substantially to these non-drug deaths. We exploit three well-established opioid medication supply shifters: triplicate laws, cancer mortality, and Social Security disability insurance (SSDI) benefits. First, triplicate prescription regulation at the state level posed a major obstacle to sales of opioid drugs. Alpert et al. (2022) show that this resulted in reduced opioid drug marketing efforts among pharmaceutical companies. Second, Arteaga and Barone (2026) argue that pharmaceutical companies targeted markets with higher cancer mortality rates for opioid medication advertising efforts, under the assumption that the wide use of opioid medication among cancer patients will facilitate the penetration of opioid medication treatment to other patients. Third, Evans and Lieber (2023) assume that higher shares of adults treated for pain resulted in greater opioid medication penetration at the local level. They proxy for higher shares of adults in pain with the share of the adult population with SSDI at the county level. All three studies show that these opioid medication supply shifters caused increased drug mortality during the opioid epidemic.

We apply the same empirical difference-in-differences (DID) frameworks in Alpert et al. (2022), Arteaga and Barone (2026), and Evans and Lieber (2023) to NDS and ARM mortality data at the respective geographic level of these studies (states, commuting zones, or counties). Our event-study designs utilize regional variation in these supply shifters prior to the opioid epidemic interacted with annual indicators to test for rising mortality rates after 1996, when OxyContin, a leading opioid

prescription drug, was approved by the FDA. We corroborate our results with an instrumental variable (IV) strategy that evaluates the effect of drug use on NDS or ARM rates. The endogenous variable is drug use at the relevant local level (state, CZ, or county) and the instrument is the corresponding supply shifter (non-triplicate status, cancer mortality, or SSDI rates).

We hypothesize two potential channels for a positive relation between increased opioid supply and higher NDS and ARM rates. First, public health and psychiatric studies show that drug abuse can intensify suicidal tendencies or alcohol abuse.[1] Second, increased drug abuse can burden local medical and social services, resulting in a shortage of resources to treat mental health or alcohol abuse problems. We will refer to the former as the "interaction channel" and the latter as the "healthcare shortage channel". This version of our working paper does not provide a clear indication which of the two channels is driving our results, but our goal is to empirically rule out or establish dominance of each of these channels in future versions of this study.[2]

Our project helps explain the causes of a major public health concern that has taken the lives of hundreds of thousands of Americans over the past 25 years. We find that the supply shifters we evaluate increased NDS and ARM rates by as much as 4 deaths per 100,000 population ages 25-64, which translates into contributing 28-60 percent to the increase in NDS and ARM rates over the last two decades. This represents a shift in the common notion in the existing literature that NDS or ARM are purely demand-driven. Our paper argues otherwise, at least for the period during the opioid epidemic. The notion that NDS and ARM are driven only by demand-side factors is so ingrained that studies wishing to rule out a correlation between potential opioid supply shifters and demand factors evaluated NDS or ARM rates as a placebo/robustness check (Alpert et al., 2022; Arteaga and Barone, 2026; Evans and Lieber, 2023). Our findings not only demonstrate a causal relationship between opioid medication supply shifters and NDS and ARM rate increases during the opioid epidemic, but also explain why robustness checks in past literature that were intended to rule out such causal effects differ from our analyses. Lastly, our project also contributes to a growing literature that shows the downstream effects of the opioid epidemic on the labor market (Cohle and Ortega, 2023; Mukherjee et al., 2025), crime (Bondurant et al., 2018; Dave et al., 2021;

[1]For public health and psychiatric literature with evidence on the interaction between drug abuse and suicidal tendencies, see: Connery et al. (2020); Hesse et al. (2020); Poorolajal et al. (2016); Yuodelis-Flores and Ries (2015); Ashrafioun et al. (2017); Rizk et al. (2021). For the interaction between drug and alcohol abuse, see: Witkiewitz and Vowles (2018); Hser et al. (2017); Price et al. (2011).

[2]We acknowledge that a correlation between opioid supply and NDS and ARM can also exist due to drug overdose being a competing risk to NDS or ARM - specifically, if there is an increase in one, this may offset the other; However, given the positive relationship we find, we do not focus on this channel, as its existence would only imply that our results may be biased downward.

Smart and Reuter, 2022; Wen et al., 2017), family (Buckles et al., 2023; Bullinger et al., 2022), and voting patterns (Arteaga and Barone, 2026). We focus on mortality, an essential factor for assessing the overall adverse effects of the opioid epidemic.

2 Background

In the last few years, deaths resulting from drug overdoses have exceeded 100,000 annually, and death rates are nearly ten times the rates of the 1990's. Scholarly attempts to explain this surge are based on demand and supply-side hypotheses. Demand-side explanations conjecture that adverse changes in social and economic factors have led certain segments of the population to drug use (Case and Deaton, 2015, 2017; Pierce and Schott, 2020; Giles et al., 2023). Supply-side hypotheses focus on changes in the availability of opioid prescriptions as driving forces for increased drug mortality.

The introduction of the prescription opioid OxyContin in 1996 has been established as a major supply factor that increased drug mortality rates. Alpert et al. (2022) use variation in state-based triplicate prescription programs. These posed a major obstacle to sales of opioid drugs and resulted in reduced marketing efforts for opioid pain medication among pharmaceutical companies. Alpert et al. (2022) show that non-triplicate states had substantially greater increases in drug overdose deaths following the introduction of OxyContin in 1996, as opposed to triplicate states.[3] Arteaga and Barone (2026) similarly argue that greater marketing efforts on behalf of pharmaceutical companies increased drug mortality post-1996. Their proxy for the extent of marketing efforts is the cancer mortality rate as of 1996 at the commuting zone level, based on pharmaceutical companies' internal records documenting that these markets were easier to penetrate due to wider usage of opioid medication among cancer patients. Evans and Lieber (2023) argue that the increased supply of opioid medication did not only result from greater marketing efforts but also from changes in prescribers' behaviors. The authors show that this is strongly correlated with SSDI coverage, as this implies higher concentrations of individuals with pain, thus facilitating the shift toward increased prescription of pain medication. They therefore exploit county-level variation in SSDI coverage as of 1990. Taken as a whole, all three studies exploit regional variation in opioid medication supply shifters at the beginning of or shortly before the opioid epidemic. Using these shifters within a difference-in-differences framework, all three studies demonstrate how supply factors contributed substantially to rising drug mortality rates.

[3]Triplicate states are: California, Idaho, Illinois, New York, and Texas.

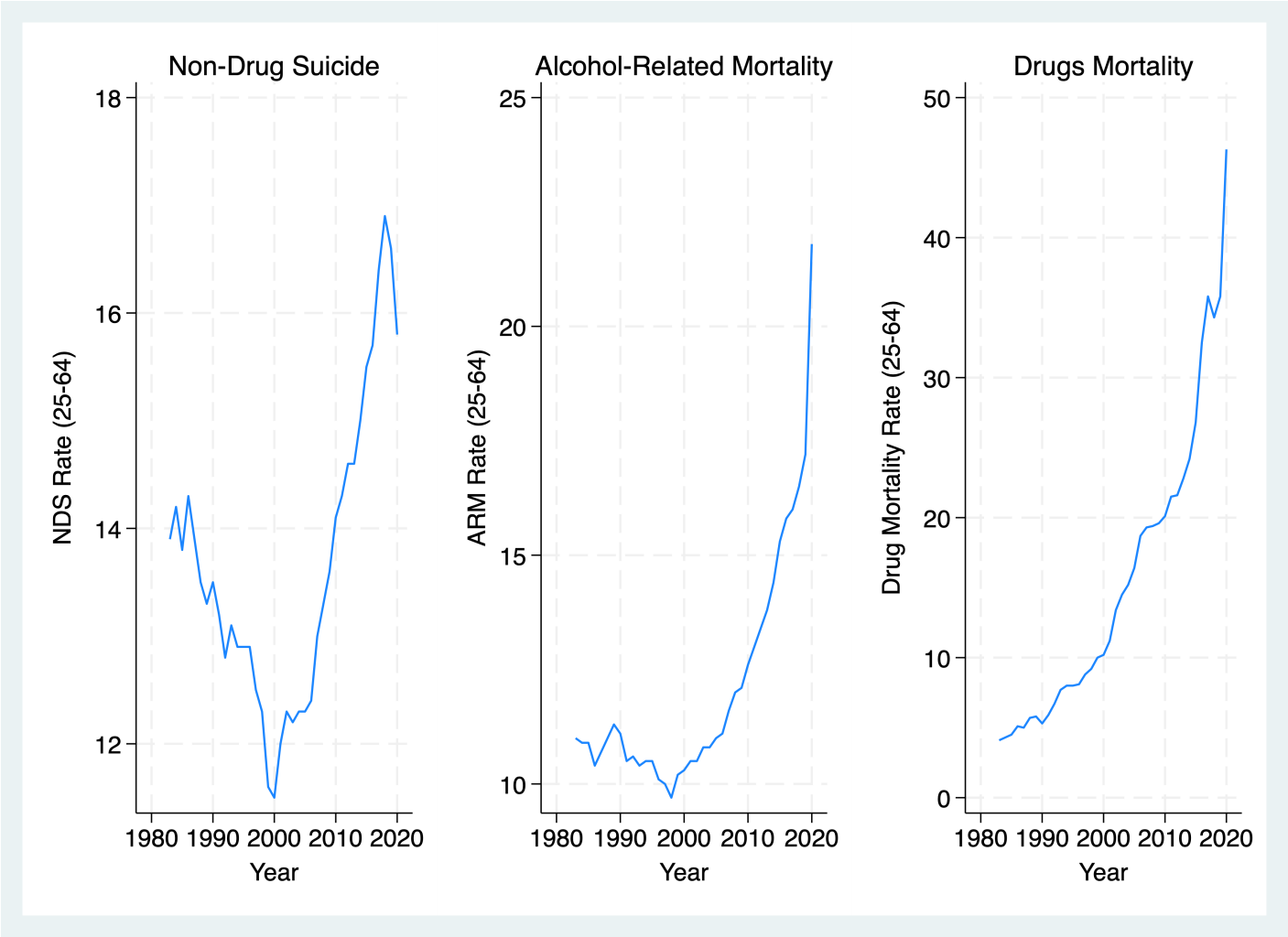
Additional studies have evaluated other supply-side factors that could have affected opioid or drug abuse. These include prescription drug monitoring programs (PDMP) (Buchmueller and Carey, 2018), or physician and insurance incentives and practices (Barnett et al., 2017; Schnell and Currie, 2018; Khan et al., 2019; Powell et al., 2020; Eichmeyer and Zhang, 2022, 2023).

Increases in non-drug suicide (NDS) or alcohol-related mortality (ARM) evolved simultaneously with increased drug mortality (Case and Deaton, 2015). Figure 1 demonstrates this. NDS and ARM rates began to increase sharply in 2000 or 1999, respectively, and by 2020 this represented a nearly 50% increase for NDS or more than a doubling for ARM. While these increases were substantial, drug-mortality increases were much more dramatic.

Two economic studies explore opioid supply-side factors and their effects on suicide. Eichmeyer and Zhang (2023) use administrative veterans' health data and find that assignment to a primary care provider with a greater tendency to prescribe opioid medication results in higher risks of long-term opioid use or opioid abuse disorder, increased rates of depression diagnosis, and suggestive evidence of increased suicide attempts. The latter analysis is presented as secondary to the main analysis concerning opioid abuse outcomes, and no further discussion is provided on the potential implications of these findings with regard to the interaction between increased access to opioid medication and suicidal behavior. Borgschulte et al. (2018) evaluate the effects of PDMP, a policy tool that established databases that track individual drug prescriptions and has been shown to be effective in decreasing opioid abuse (Buchmueller and Carey, 2018). The authors find that PDMP implementation decreased suicides when there were more drug treatment centers available locally to enable a path out of drug dependency once supply was disrupted. While Borgschulte et al. (2018) tie a supply-side factor (PDMP) with suicide rates, the dependency of this result on the extent of drug treatment centers available implies that this channel is still not entirely supply-driven. In both Borgschulte et al. (2018) and Eichmeyer and Zhang (2023) the suicide mortality rates include drug-related suicide, so one cannot rule out the possibility that some of the effects observed are mechanical - i.e. limiting the supply or access to drugs also results in reduced drug-related suicidal attempts. Our study focuses on non-drug-related causes of death (NDS and ARM), as we wish to isolate the effects of increased drug supply on mortality as a secondary outcome that cannot be argued to be mechanical.

As drug mortality grew more severe over the years, its sources changed as well. At the start of the opioid epidemic, most drug deaths were due to prescription opioid abuse. However, by 2010, the

Figure 1: National Mortality Rates for Ages 25-64



Notes: The figure plots the national annual death rates for non-drug suicides (NDS), alcohol-related mortality (ARM), and drug overdose for ages 25-64 during 1983-2020. See footnote 4 for ICD codes used for each cause of death.

vast majority of drug overdose deaths were due to illicit drugs, such as heroin and fentanyl (Alpert et al., 2018; Evans et al., 2019; Ruhm, 2019; Cutler and Glaeser, 2021). Nevertheless, prescription opioid supply shifters that prevailed at the start of the opioid epidemic still affected drug mortality even 20 years later (Alpert et al., 2022; Arteaga and Barone, 2026), thereby demonstrating the long-lasting effects these factors had on drug mortality even after opioid medication was no longer the major contributor to drug mortality. These early supply factors were instrumental in shaping drug mortality for many years thereafter. Consequently, our analysis also focuses on this time span.

3 Data

3.1 Mortality Data

Our analyses use restricted-use mortality data that identifies causes of death using ICD-9 or ICD-10 codes and includes geographic identifiers at the state or county level. The data are from the Center for Disease Control and Prevention (CDC) National Center for Health Statistics (NCHS). For alcohol-related mortality (ARM), we choose all ICD codes that relate the cause of death to alcohol, rather than concentrating on chronic liver disease conditions. This matches our underlying hypothesis - that opioid and drug abuse led to greater alcohol abuse, and as such, any cause of death related to alcohol could have increased as a result of greater supply of opioid medication. We therefore follow the ICD codes used in Arteaga and Barone (2026) and Evans and Lieber (2023), rather than those used in Alpert et al. (2022) and Case and Deaton (2015).[4] Our deceased span ages 25-64, as in other research examining morbidity related to these causes of death (Case and Deaton, 2022). We also utilize restricted-use mortality data to follow Arteaga and Barone (2026) and construct cancer mortality rates for the entire population at the commuting zone (CZ) level as of 1996.[5] Our time span for the analysis is 1983-2019.

3.2 Population Counts

The data on population counts come from the Survey of Epidemiology and End Results (SEER). We use these data for the following purposes: (i) to construct the denominators at the state, CZ,

[4]Causes of death are based on ICD-9 codes for years prior to 1999 and on ICD-10 for years from 1999 onward. Non-Drug Suicide ICD-9 codes: E950.6-E959. Non-Drug Suicide ICD-10 codes: X65-X84. Alcohol related mortality ICD-9 codes: 2971.3, 303, 350.0, 357.5, 425.5, 535.3, 571.0-571.3, 790.3, E860. Alcohol related mortality ICD-10 codes: E24.4, F10, G31.2, G72.1, I42.6, K29.2, K70, K85.2, K86.0, O35.4, Q86.0, X45, X65, Y15.

[5]Cancer ICD-9 codes are: 140-208 and 210-239, excluding 162 as specified in Table B1 in Arteaga and Barone (2026).

or county level for adult mortality rate measures, SSDI recipient rates, or opioid pill usage rates; (ii) to construct control variables at the state, CZ, or county level for the share of the population of a particular age or demographic group.

3.3 Social Security Disability Insurance (SSDI) Data

We follow Evans and Lieber (2023) here. Specifically, we define for each county the share of those 18-64 on SSDI as the ratio of the number of individuals on SSDI in December 1989 from Moore (2020)'s compiled data and the corresponding 18-64 population from the SEER population for 1990.

3.4 Opioid Use Data

We use data on the legal supply of opioids from the Drug Enforcement Agency's (DEA) Automation of Reports and Consolidated Orders System (ARCOS) from Arteaga and Barone (2026). This contains distribution records of all Schedule II drugs by active ingredient (e.g. morphine, oxycodone) at the 3-digit ZIP code from 1997-2018. The primary variable we use is the per capita prescription opioids, per year. This is calculated as the sum of oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine in morphine-equivalent doses (60 morphine-equivalent mg).

3.5 Summary Statistics

Table 1 presents summary statistics for each analysis - Triplicate, Cancer Mortality, and SSDI - by column. Populations-weighted mean mortality rates per 100,000 residents ages 25-64 are across states, CZ's, or counties for the Triplicate, Cancer Mortality, and SSDI analysis, respectively. The population-weighted mortality rates presented are Non-Drug Suicide, Alcohol-Related Mortality, and Drug Overdose, as discussed in Section 3.1. In addition, the population-weighted means and minimum and maximum values of the explanatory variable are presented for each analysis.

The "Number of Clusters" listed at the bottom of each column is the number of states, CZ's, or counties, in the Triplicate, Cancer Mortality, and SSDI analysis, respectively. Alaska and Hawaii counties are excluded from the Cancer Mortality and SSDI analyses, as county-level population measures were not available for these states prior to 1990 or 2000, respectively. Furthermore, the Cancer Mortality analysis excludes CZ's with a population under 20,000, and the SSDI analysis

excludes counties with a population under 10,000.[6]

Table 1: Summary Statistics

	Triplicate Analysis	Cancer Mortality Analysis	SSDI Analysis
Non-Drug Suicide	13.638	13.660	13.580
	(3.593)	(4.910)	(6.818)
Pre-1996 Non-Drug	13.041	13.079	13.015
Suicide	(2.799)	(4.059)	(6.203)
Alcohol-Related Mortality	11.909	11.916	11.866
	(4.710)	(6.216)	(8.047)
Pre-1996 Alcohol-Related	10.391	10.442	10.368
Mortality	(3.363)	(4.905)	(7.152)
Drug Overdose	15.785	15.757	15.864
Mortality	(11.765)	(13.220)	(15.076)
Pre-1996 Drug Overdose	6.950	6.930	7.016
Mortality	(3.389)	(4.573)	(6.315)
Mean Explanatory Variable	0.692	201.131	1.814
	(0.466)	(41.671)	(0.735)
Minimum Exp. Variable	0.00	84.32	0.30
Maximum Exp. Variable	1.00	378.66	8.88
Number of Observations	1,887	22,792	88,208
Number of Clusters	51	616	2,384

Notes: All means are populations-weighted across states, CZ's, or counties for the Triplicate, Cancer Mortality, and SSDI analysis, respectively, with standard deviations in parentheses. Mortality rates are per 100,000 residents ages 25-64. The Triplicate Analysis explanatory variable is an indicator for non-triplicate states - all states except for California, Idaho, Illinois, New York, and Texas. The Cancer Mortality Analysis explanatory variable is the CZ-level cancer mortality rate per 100,000 population (for all ages) as of 1996. The SSDI Analyses explanatory variable is the rate per 100,000 population ages 18-64 receiving SSDI benefits at the county level as of 1990. The Cancer Mortality and SSDI samples exclude Alaska and Hawaii counties. Cancer Mortality and SSDI samples exclude CZ's with population under 20,000 or counties with population under 10,000 as of 1996. Number of Clusters refers to the number of states, CZ's, or counties for each respective analysis. Data is for 1983-2019.

4 Regression Specifications

All three analyses explore changes in NDS and ARM rates after 1996, when OxyContin was introduced, as a function of exogenous determinants for local opioid supply: non-triplicate status at the state level (as of 1996), the cancer mortality rate at the CZ level (as of 1996), and SSDI receipt rate at the county level (as of 1990). Our specifications are event-study analyses with mortality

[6] Arteaga and Barone (2026) similarly exclude CZ's with populations under 20,000. Although Evans and Lieber (2023) do not apply a sample restriction based on county population levels, we apply this restriction to maintain consistency across the three analyses. Our SSDI analysis is not sensitive to this population restriction (recall that our regression specifications are population-weighted).

rates as the dependent variables and the unit of observation is at the year and respective local level (state, CZ, or county).

The following outlines the state-level regression specification for the Triplicate event study analysis:

$$y_{sy} = \alpha_s + \gamma_y + \Sigma_{k \neq 1995} \beta_k \times (Non - Triplicate)_s \times 1(y = k) + X_{sy} + \epsilon_{sy} \quad (1)$$

where y_{sy} represents the mortality rate per 100,000 residents ages 25-64 in state s in year y. The specification includes state (α_s) and year (γ_y) fixed effects. The $(Non - Triplicate)_s$ indicator is equal to one for states that had a non-triplicate status as of 1996 and it is interacted with a full set of year fixed effects, excluding 1995. Equation 1 also includes time-varying state-level control variables (X_{sy}): white population shares, the shares of the populations ages 18-29, 30-49, 50-64, above 65 years, and log of population. Standard errors are clustered at the state level and regressions are weighted by state-level populations for ages 25-64.

The following outlines the CZ or county-level regression specification for the Cancer Mortality or SSDI event-study analysis:

$$y_{cy} = \mu_c + \theta_{sy} + \Sigma_{k \neq 1995} \pi_k \times (ExpVariable)_c \times 1(y = k) + X_{cy} + \epsilon_{cy} \quad (2)$$

where y_{cy} represents the mortality rate per 100,000 residents ages 25-64 in CZ or county c in year y. The specification includes CZ or county fixed effect (μ_c) and state-year fixed effects (θ_{sy})[7] that control for time-varying state-level policies. The $(ExpVariable)_c$ is either the 1996 cancer mortality rate at the CZ level or the 1990 SSDI receipt rate among ages 18-64 at the county level. It is interacted with a full set of year fixed effects, excluding 1995. X_cy are time-varying CZ or county-level demographics, as specified for equation 1. For the Cancer analysis, we follow Arteaga and Barone (2026) and also include the contemporaneous cancer mortality rate for the entire population. Standard errors are clustered at the CZ or county level and regressions are weighted by CZ or county populations for ages 25-64.

[7]For the state-year fixed effects in the cancer mortality analysis, we follow Arteaga and Barone (2026) and associate with each CZ the state that has the largest population share within the CZ.

5 Results

5.1 Event Study Results

We present the results of event-study analyses for all three sources of exogenous variation in the supply of opioid medication in Figure 2. Columns 1, 2, and 3 present results for the Triplicate, Cancer Mortality, and SSDI analysis, respectively. In the top panel, the dependent variable is the NDS rate for ages 25-64, while in the bottom panel, it is the ARM rate for ages 25-64. All regression results are graphical representations of all β_k or π_k estimates from equations 1 or 2, respectively, along with vertical lines representing their 95 percent confidence intervals.

The results exhibit increases in NDS and ARM mortality rates beginning in 1996, or several years thereafter, in localities that experienced a greater supply of opioid medication due to one of our three exogenous supply shifters - non-triplicate status, greater cancer mortality, or greater SSDI recipient rates. The effects also appear to persist more than 20 years after the introduction of OxyContin and increase in magnitude. The ARM SSDI analysis is an outlier with inconclusive results, due to a strong pre-existing trend in the 1980's and a positive "effect" that is only apparent beginning around 2008, with the caveat that it is difficult to interpret this increase as an effect because of the pre-exiting trend.

In the Triplicate Analysis, the results suggest that non-triplicate states experienced an increase of roughly two in the NDS and ARM death rates per 100,000 population post-1996, in comparison to triplicate states. This is in comparison to an estimated increase of roughly 8 in the drug mortality rate for the same age group and regression specification, as presented in Appendix Figure A1.[8]

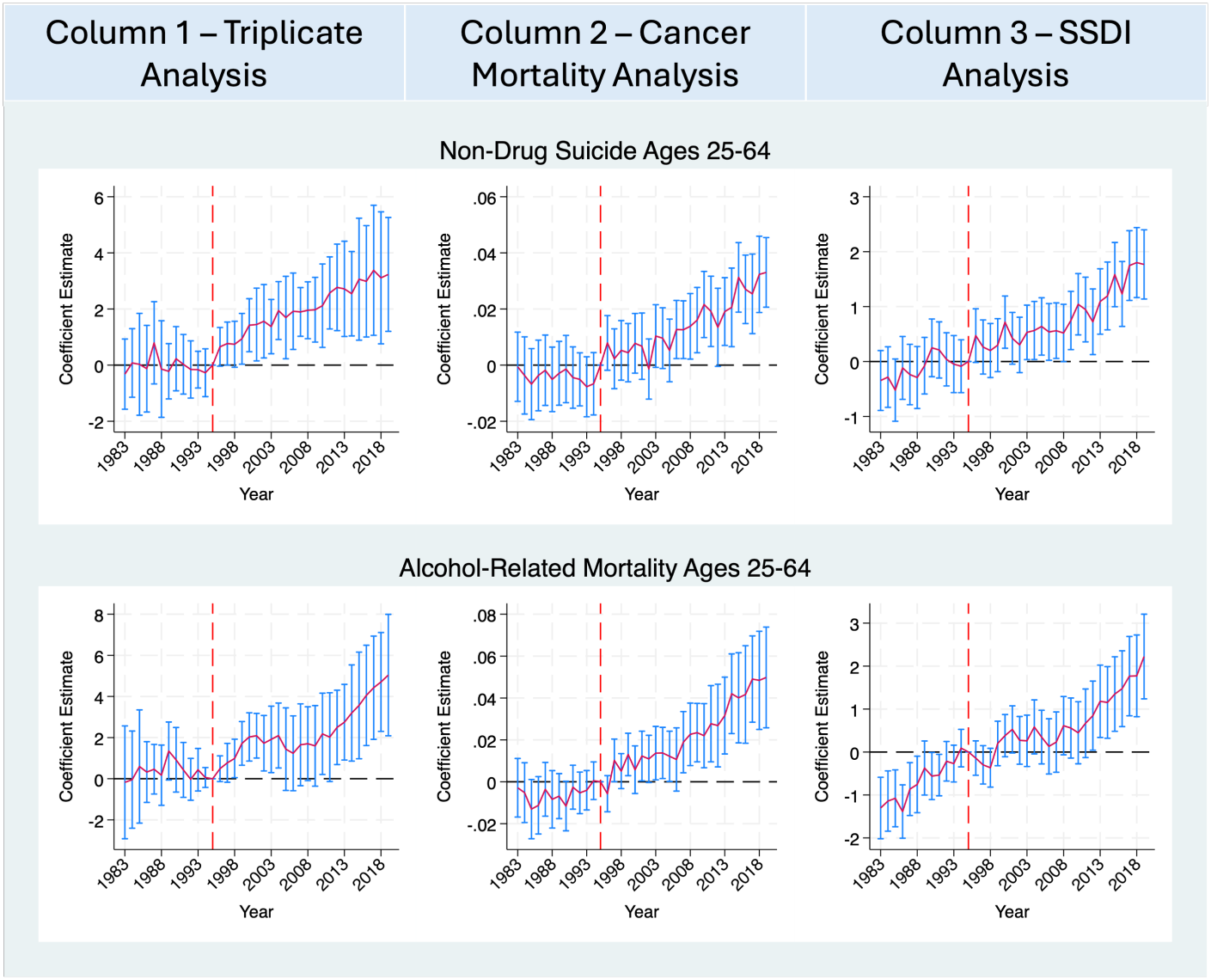
For the Cancer Mortality Analysis, the estimates in Figure 2 suggest that for each additional unit increase in the CZ 1996 cancer mortality rate, NDS and ARM rates increased by roughly 0.015 and 0.023, respectively.[9] With a mean 1996 CZ cancer mortality rate of 200 (see Table 1), this implies an increase of roughly 3-4.6 deaths from each mortality measure per 100,000 residents. In Appendix Figure A1, the corresponding point estimate for drug mortality is 2.5-3.5 times larger.

Lastly, the SSDI Analysis estimates in Figure 2 suggest an increase of roughly 0.8 NDS deaths per 100,000 residents at the county level for each additional 1990 SSDI recipient per 100,000 residents ages 18-64. The estimated effect of each 1990 SSDI recipient per 100,000 residents on drug mortality

[8]Difference-in-differences estimates for the entire post-1996 period (not presented) are 2.08, 2.04, and 7.9 for NDS, ARM, and drug mortality, respectively.

[9]The exact point estimates are from a difference-in-differences analysis (not presented) with a single post-1996 estimate.

Figure 2: Event Study Analyses - Opioid Supply Shifters and Non-Drug Suicide / Alcohol-Related Mortality



Notes: The figure plots for each analysis the coefficient estimates β_k or π_k from equations 1 or 2, respectively, along with the 95 percent confidence intervals. In column 1, the coefficient estimates represent the increase in the mortality rate among non-triplicate states relative to triplicate states and the base year, 1995. In column 2, each coefficient estimate measures the change in the mortality rate for each unit increase in the local 1996 cancer mortality rate, relative to the base year, 1995. In column 3, each coefficient estimate measures the change in the mortality rate for each unit increase in the local 1990 SSDI recipient rate, relative to the base year, 1995.

post-1995 in Appendix Figure A1 is six times larger. With a mean SSDI rate of 1.8 (see Table 1), this suggests that greater opioid-medication supply resulting from increased SSDI coverage increased NDS rates by an average of 1.44. We refrain from interpreting the ARM SSDI coefficient estimates in Figure 2 due to their inconclusiveness primarily because of the pre-treatment period upward trend.

Overall, all three analyses suggest that the estimated effect of greater opioid-medication supply at the beginning of the opioid epidemic on NDS and ARM is roughly 17-40% of the estimated effects on drug overdoses. The varying opioid-medication supply shifters increased NDS and ARM rates during 1996-2019 by 1.4-3.4 and 2-4.6, respectively. The increase in NDS and ARM rates observed in Figure 1 for the period 2000-2019 is 5.1 and 7.5 deaths per 100,000 population, respectively. Taking these estimated increases in NDS and ARM in response to each of the supply shifters and dividing them by the respective mortality increase suggests that the contribution of opioid-medication supply shifters to rising NDS rats was 29-60%, while for rising ARM rates, it was 28-61%.

5.2 Instrumental Variable Results

Our analyses exploit three exogenous opioid-medication supply shifters. The underlying hypothesis is that these shifters increased the local supply of opioid medication, thereby increasing opioid and drug abuse. The relationship between 1996 cancer mortality rates or 1990 SSDI recipient rates and opioid-medication consumption has already been demonstrated in Arteaga and Barone (2026) and Evans and Lieber (2023), respectively, using Drug Enforcement Administration's (DEA) Automation of Reports and Consolidated Orders System (ARCOS) data (see Section 3.4 for data description). We use the ARCOS data to further establish our hypothesis: that the increase in opioid medication consumption, which led to increased opioid and drug abuse, also led to increased suicidal tendencies and alcohol abuse, thereby increasing mortality from these causes. We can thus apply an instrumental variable (IV) approach. Our dependent variable is the NDS and ARM rate. Our endogenous variable is the annual morphine-equivalent doses prescribed per capita, and our instrument is one of the three opioid supply shifters - non-triplicate status at the state level, 1996 CZ cancer mortality rate, or 1990 county SSDI recipient rate.

We note that ARCOS data is only available beginning in 1997. As such, our empirical specification for the IV framework treats 1997 as the base year and assesses changes in opioid medication

consumption or mortality relative to that base year, as a function of the explanatory variable (i.e. the opioid medication supply shifter). Our regression specifications follow closely equations 1 and 2 from Section 4, with population weights at the relevant locality level, standard errors clustered at the relevant locality level, and the fixed effects and control variables that are outlined in the equations for each analysis.

We present our IV results in Table 2 for each of the three instruments we use in their respective column. The first panel presents the first-stage regressions. The coefficient estimates are positive and statistically significant with F-statistics exceeding the minimal value required for the explanatory variable to be a valid instrument. The second-stage regression results are presented in the second column. They all suggest that each additional morphine-equivalent dose prescribed for each person causes 1 more NDS/ARM death per 100,000 in the population.

The third panel of Table 2 presents second-stage regression results when the drug mortality rate, rather than the NDS and ARM mortality rate, is the dependent variable. This allows us to compare the magnitude of the effect of opioid-medication usage between NDS and ARM mortality and drug mortality. We expect the drug mortality effect to be greater in magnitude than the NDS and ARM mortality effect because this is a more direct effect, whereas the NDS and ARM mortality is a secondary effect. Indeed, panel 3 of Table 2 presents effects on drug mortality that are 2.5-5 times greater than the effects on NDS and ARM mortality.

The fourth and fifth panels of Table 2 present OLS regression estimates for the relationship between opioid-medication usage and NDS and ARM or drug mortality, respectively. For NDS and ARM rates, the estimates are small and statistically insignificant. This is in contrast to the much larger and statistically significant IV estimates in the second panel. While the OLS drug mortality estimates are positive and statistically significant, they are substantially smaller than the IV estimates for drug mortality. The sharp difference between the IV and OLS estimates suggests that opioid medication usage is also correlated with factors that negatively affect mortality. For example, opioid-medication usage may be higher in more advantageous localities, and drug, suicide and ARM rates are greater among disadvantaged low-educated populations (Case and Deaton, 2017). This highlights the value of the IV approach in estimating a causal relationship between opioid-medication usage and mortality.

Table 2: Opioid Pill Usage and Non-Drug Suicide & Alcohol-Related Mortality - IV Approach

	Triplicate Analysis	Cancer Mortality Analysis	SSDI Analysis
Stage 1 Regressions - Opioid Use Per Person as Dependent Variable			
Explanatory Variables X Post-1997	2.782***	0.0146***	0.837***
	(0.544)	(0.00357)	(0.188)
F-Statistic	26.17	16.76	19.86
Stage 2 Regressions - Non-Drug Suicide & Alcohol-Related Mortality as Dependent Variable			
Opioid Use Per Person	1.020**	1.042*	1.083***
	(0.428)	(0.533)	(0.380)
Stage 2 Regressions - Drug Mortality as Dependent Variable			
Opioid Use Per Person	2.724***	3.154***	5.101***
	(0.819)	(0.834)	(1.044)
OLS Regressions - Non-Drug Suicide & Alcohol-Related Mortality as Dependent Variable			
Opioid Use Per Person	0.149	0.00503	-0.0112
	(0.0980)	(0.0447)	(0.0127)
OLS Regressions - Drug Mortality as Dependent Variable			
Opioid Use Per Person	0.967***	0.526***	0.159***
	(0.267)	(0.103)	(0.0587)
Mean NDS & ARM Rate	25.63	25.62	25.49
Mean Opioid Use Per Person	7.789	7.775	7.788
Mean Drug Mortality Rate	18.99	18.94	19.06
Observations	1,068	12,936	50,064

Notes: All regression are population-weighted. Point estimates are presented with standard errors clustered at the state, CZ or county level in parentheses. Mortality rates are per 100,000 residents ages 25-64. The Triplicate Analysis explanatory variable is an indicator for non-triplicate states - all states except for California, Idaho, Illinois, New York, and Texas. The Cancer Mortality Analysis explanatory variable is the CZ-level cancer mortality rate per 100,000 population (for all ages) as of 1996. The SSDI Analyses explanatory variable is the rate per 100,000 population ages 18-64 receiving SSDI benefits at the county level as of 1990. The Cancer Mortality and SSDI analyses exclude Alaska and Hawaii counties. Number of Clusters refers to the number of states, CZ's, or counties for each respective analysis. Data is for 1997-2017 (drug use data only available beginning 1997).

5.3 Reconciling Our Results with Other NDS and ARM Analyses

Our three explanatory variables are used in three separate studies evaluating the effects of opioid-medication supply shifters on drug mortality (Alpert et al., 2022; Arteaga and Barone, 2026; Evans and Lieber, 2023). These studies perform supplementary analyses that evaluate the effects of their supply shifters on other causes of death, including NDS or ARM rates. The effects of opioid-medication supply shifters on NDS or ARM are either ruled out or deemed inconclusive in these studies. This is in contrast to our results. We compare our analysis with the analysis in these studies to explain these differences.

In a supplementary analysis, Alpert et al. (2022) perform the triplicate analysis for NDS and chronic liver condition (CLD) mortality. Their results demonstrate a statistically significant increase in NDS and CLD mortality in non-triplicate states beginning shortly after 1996, similar to column 1 in Figure 2 above. However, their analysis exhibits pre-existing upward trends among non-triplicate states during the 1980's. This could imply that increases in NDS and CLD mortality among non-triplicate states post-1996 are due to varying trends prior to the introduction of OxyContin in 1996.

Our triplicate event study analysis - as presented in column 1 of Figure 2 - differs from the Alpert et al. (2022) NDS and CLD event study analysis in three aspects: first, our mortality rates are for ages 25-64, rather than the entire population; second, we include time-varying control variables in our regression specification - namely, demographic and age-group population shares at the state level - whereas the Alpert et al. (2022) NDS and ARM event-study results do not include control variables; third, we use a broader set of alcohol-related causes of death and limit the liver conditions to just those related to alcohol.[10] It turns out that the first two factor, the population base and use of control variables in the regression specification, are critical for whether pre-existing trends arise in the triplicate event-study analysis.

In Appendix Figure B1 we present a replication of the Alpert et al. (2022) event-study analysis for NDS and CLD mortality for all ages with and without control variables in the top and bottom panels, respectively. The results demonstrate that the inclusion of control variables eliminates all pre-existing trends. In Appendix Figure B2, we present our own triplicate analysis (for age group 25-64, rather than the entire population) without control variables. When comparing the results

[10]More specifically, Alpert et al. (2022) focus on ICD-9 codes K70 and K73-74, whereas we do not include ICD-9 codes K73-74. Their ICD-10 codes are 571.0-571.9, whereas ours do not include 571.4-571.9. We also have ICD codes that are alcohol-related causes of death that are not included in Alpert et al. (2022) - see footnote 4 for details.

in Appendix Figure B2 with the results in column 1 of Figure 2, it is clear that for age 25-64 mortality rates, the triplicate event-study analysis is not sensitive to the inclusion of demographic or age-group population share control variables. Taken as a whole, we conclude that the pre-existing trends found in the Alpert et al. (2022) triplicate NDS and CLD analysis are due to shifts in the population shares of either the young (< 25) or elderly (65+) in triplicate vs. non-triplicate states, and these age groups likely have different NDS or CLD rates. Our analysis focuses on ages 25-64, the primary age group experiencing increases in "despair" mortality (i.e., drug mortality, NDS, and ARM) during the past 20-25 years (Case and Deaton, 2015; Giles et al., 2023; Ruhm, 2024). As such, the pre-existing trends found in the Alpert et al. (2022) triplicate NDS and CLD analysis are not relevant to our analysis.

Arteaga and Barone (2026) exploit 1996 cancer mortality rates at the CZ level to establish how the opioid epidemic shaped voting patterns. In a supplementary event study analysis, using a very similar specification to our equation 2, they show no relationship between 1996 cancer mortality rates and NDS mortality (Figure D1 in Arteaga and Barone (2026)). No ARM analysis is presented. The NDS result in Arteaga and Barone (2026) is at odds with our results for NDS mortality in column 2 of Figure 2. However, the age range for the mortality rates in the Arteaga and Barone (2026) analysis is 20+, whereas our age range is 25-64. An expansion of the age group potentially affected by the opioid epidemic to elderly and younger adults can distort the true effects that opioid and drug abuse had on the more relevant target population - prime-age adults who are 25-64.

Evans and Lieber (2023) exploit variation across counties in SSDI receipt rates as their opioid-medication supply shifter and demonstrate how this affected drug mortality. Here too the authors perform a supplementary analysis with NDS or ARM rates as dependent variables within an event study framework. Their results demonstrate some positive and statistically significant effects of 1990 SSDI coverage on NDS mortality, and the magnitude of these effects are only slightly smaller than the magnitudes observed in column 3 of Figure 2. However, the age range for the NDS and ARM measures in Evans and Lieber (2023) is 18+, so as in Arteaga and Barone (2026), we believe a more expansive age range that includes the elderly and young adult population produced less precise estimates for the effects of SSDI recipient rates on NDS rates.

6 Concluding Remarks

We have demonstrated the substantial role of supply factors in rising NDS and ARM rates during the first two decades of the 21^{st} century. This is a major shift in the prevailing notion that NDS and ARM are entirely demand-driven.

In upcoming drafts, we intend to empirically establish the underlying mechanisms driving the positive relationship between opioid supply and NDS and ARM rates. Our two leading potential explanations are the interaction channel, due to drug abuse intensifying suicidal tendencies or alcohol abuse, and the healthcare-shortage channel, due to shifting resources away from mental health or alcohol abuse treatment to address the opioid epidemic.

We note that while our analysis only covers mortality, our mortality measures are representative of the wider prevalence of alcohol abuse and mental health issues. Our results therefore help to understand the devastating social and economic consequences of greater drug abuse, whether demand or supply-driven.

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Appendices

Appendix A Drug Mortality Event-Study Results

Although our analysis focuses on NDS and ARM rates for a relatively wide age range - adults ages 25-64 - it is still a narrower age range than what is analyzed for drug mortality in the studies we rely on (Alpert et al., 2022; Arteaga and Barone, 2026; Evans and Lieber, 2023). We present results for drug mortality with the relevant opioid-medication supply shifters for our narrower age range (25-64) in Appendix Figure A1. The results are consistent with the findings in the original studies, demonstrating drug mortality increases in response to increased supply of opioid medication, which intensify in later years. This validates the choice of our population base and our empirical strategy, which may vary slightly from that used in the studies we rely on.[11]

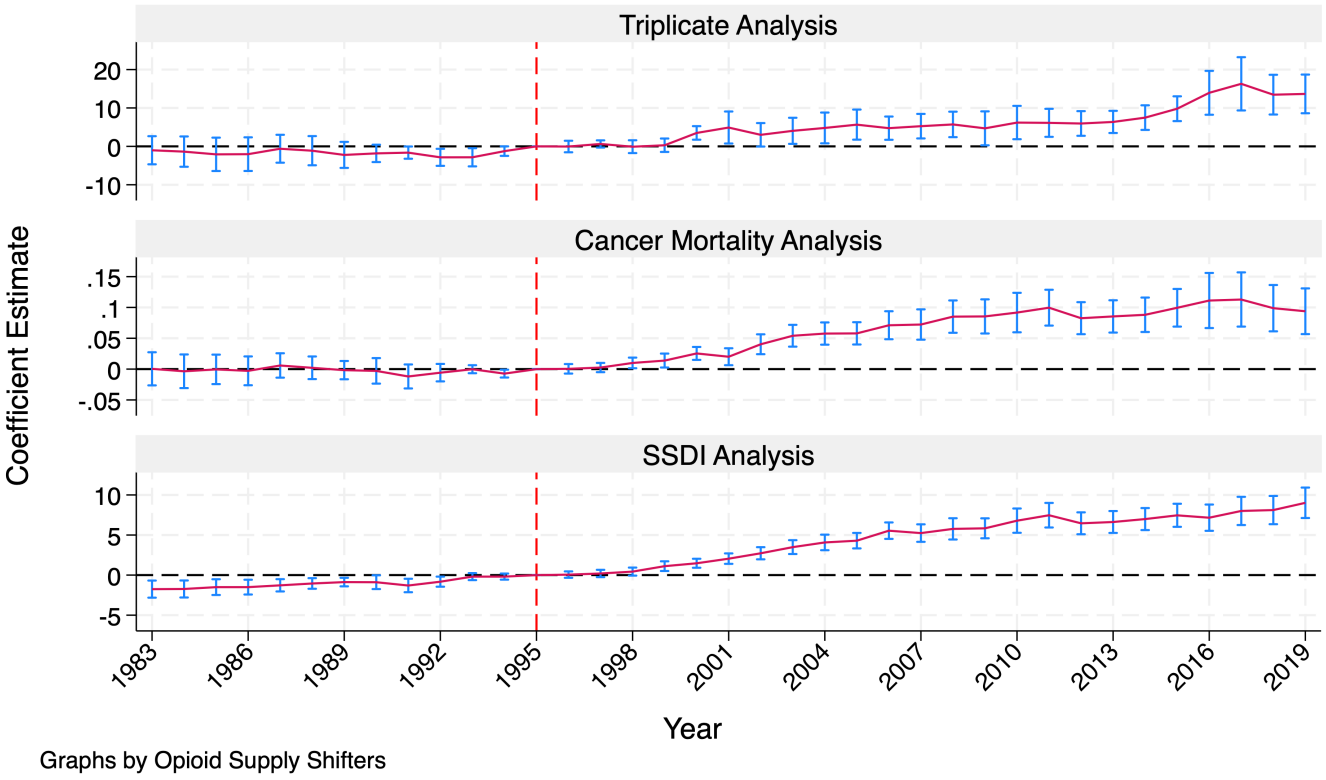
Appendix B Concern for Pre-Trends in the Triplicate Event-Study Analysis

Appendix Figure B1 presents the triplicate event study analysis in Alpert et al. (2022) - using mortality rates for the entire population rather than ages 25-64, as in our analysis, and for chronic liver disease (CLD) conditions, rather than alcohol-related (ARM) mortality, as in our analysis - with and without control variables. The control variables are age group and demographic population shares (at the state level), as specified for equation 1. The top panel - without the control variables - is supposed to be an exact replication of the results presented in Figure A17 in Alpert et al. (2022). The bottom panel repeats this analysis but adds to the regression specification the control variables.

The results presented in Appendix Figure B1 suggest that once population shares for age groups and other demographics are accounted for, there is no evidence of pre-existing trends in the 1980's. This suggests that the pre-existing trends are due to changes in the demographic composition of triplicate vs. non-triplicate states during the 1908's, and that these demographic groups likely differ in their NDS or CLD mortality rates.

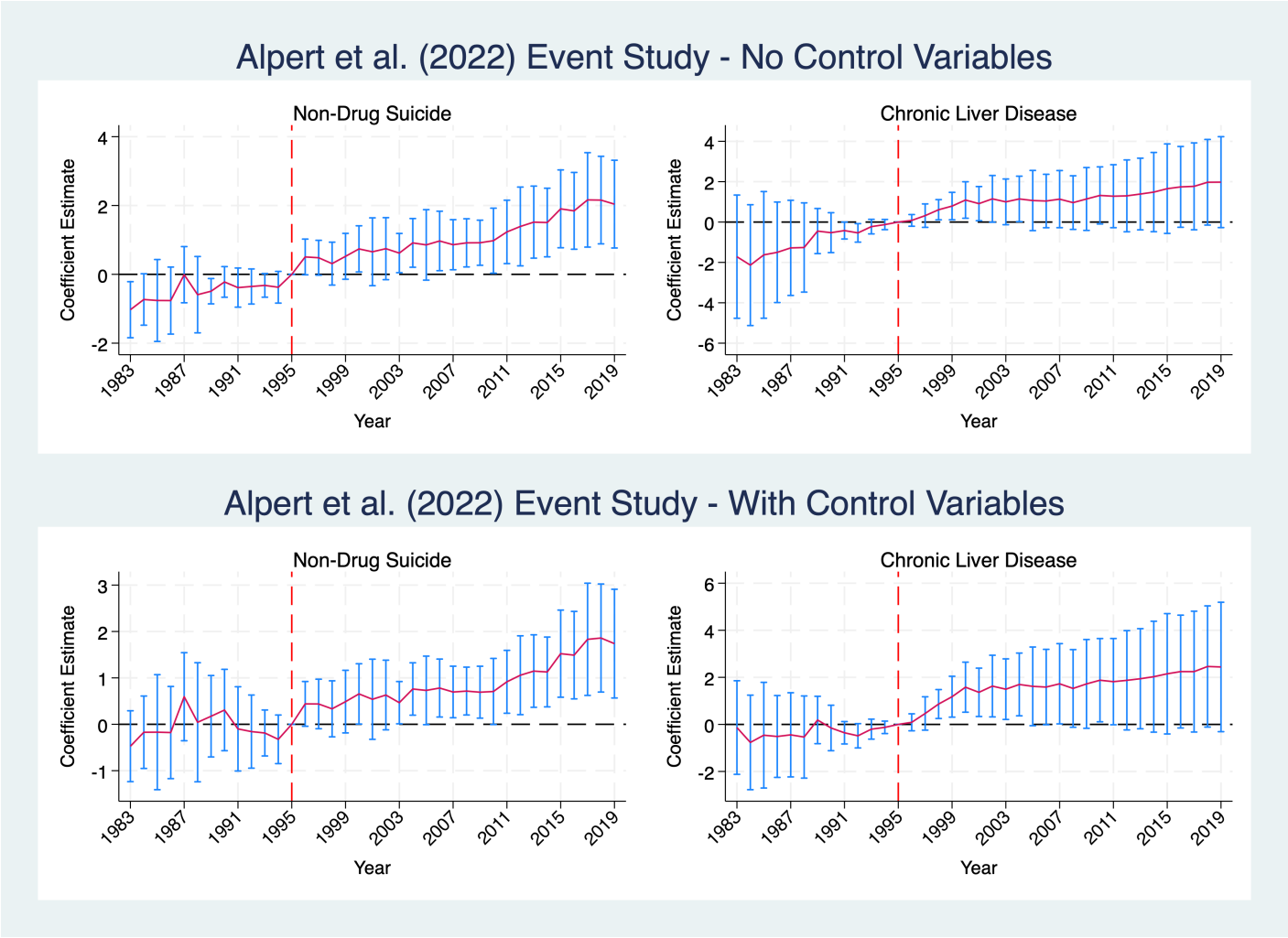
[11] For example, we use the same set of control variables for all three analyses, despite each of the studies choosing a different set of control variables. Moreover, for the SSDI analysis, we include state-year fixed effects (in addition to county fixed effects) to maintain consistency with our Cancer regression specification, but Evans and Lieber (2023) only have county and year fixed effects in their regression specification.

Appendix Figure A1: Event Study Analysis - Opioid Supply Shifters and Drug Mortality



Notes: The figure plots for each analysis the coefficient estimates β_k or π_k from equations 1 or 2, respectively, along with the 95 percent confidence intervals. The dependent variable is the drug mortality rate for ages 25-64 at the state, CZ, or county level in the top, middle, or bottom panel, respectively. Standard errors are clustered at the state, CZ, or county level in the top, middle, or bottom panel, respectively. Number of observations and means are as presented in the summary statistics in Table 1.

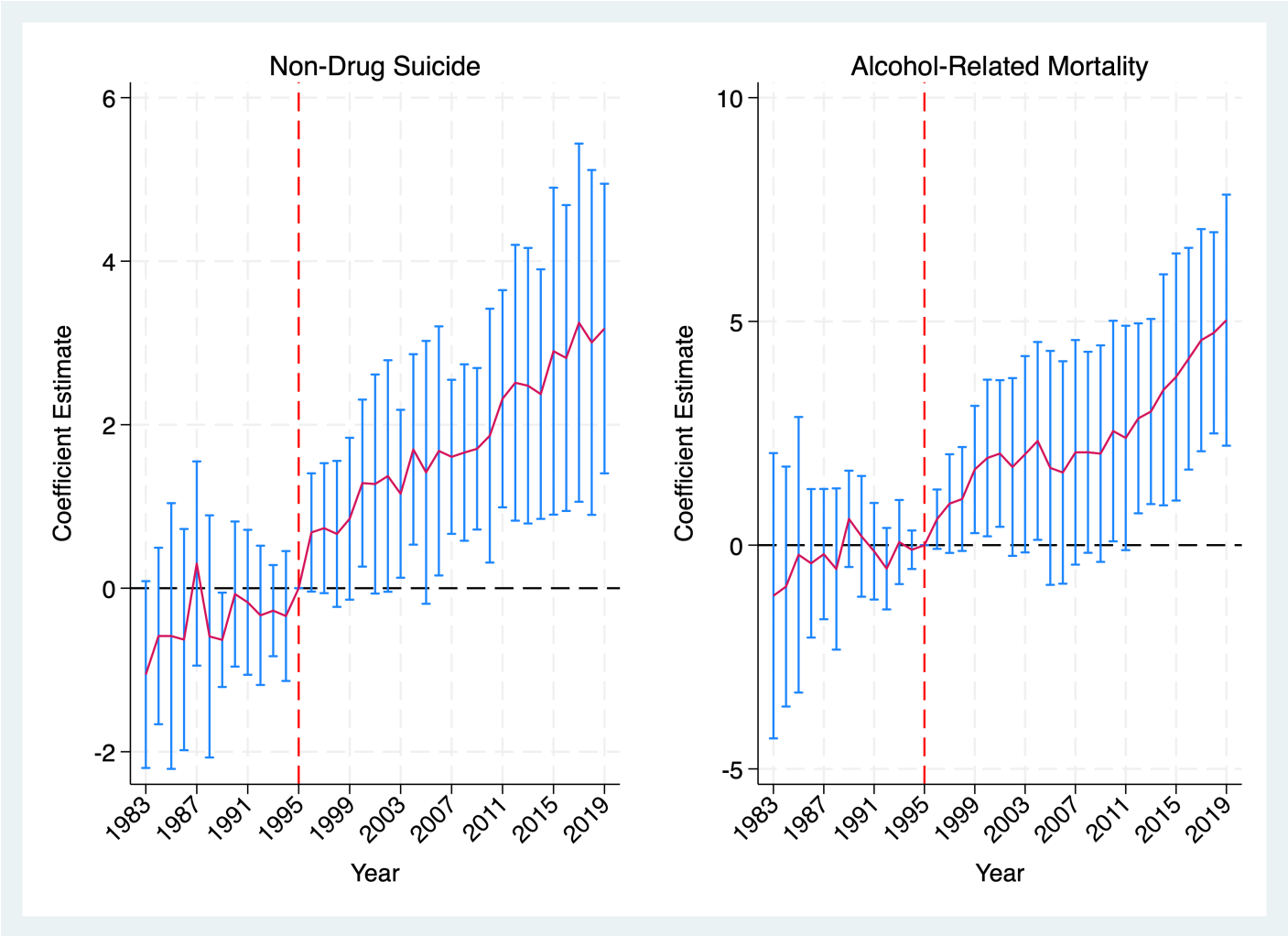
Appendix Figure B1: - Triplicate Event Study Analyses in Alpert et al. (2022) with or without Control Variables



Notes: The figure plots for each analysis the coefficient estimates β_k from equation 1, along with the 95 percent confidence intervals. The dependent variables are NDS or CLD mortality rates for the entire population at the state level. Standard errors are clustered at the state level. Control variables are as specified for equation 1.

We evaluate whether our event study analysis - which uses mortality rates for ages 25-64, rather than for the entire population - is also sensitive to the inclusion of control variables. Appendix Figure B2 presents the triplicate event study results without control variables, and these should be compared to the results in column 1 of Figure 2. In both cases, there is no evidence of pre-existing trends. As such, we conclude that the pre-existing trends in the NDS and CLD analysis in Alpert et al. (2022) are driven by compositional demographic changes in the younger (< 25) and elderly (65+) populations that are included in the Alpert et al. (2022) analysis but excluded from our analysis.

Appendix Figure B2: - Triplicate Event Study Analyses without Control Variables



Notes: The figure plots for each analysis the coefficient estimates β_k from equation 1, along with the 95 percent confidence intervals, but control variables (X_{sy}) are not included in the regression specification. The dependent variables are NDS and ARM rates for ages 25-64 the state level. Standard errors are clustered at the state level.