Crushed Futures? OxyContin Reformulation Exposure and Labor Market Outcomes

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Abstract

The 2010 OxyContin abuse-deterrent reformulation caused heroin substitution in dependent communities. We test whether this transition scarred local labor markets using continuous-treatment difference-in-differences exploiting cross-county variation in pre-reform OxyContin brand share from DEA ARCOS data, linked to Census QWI outcomes for 2,619 counties over 2005–2019. We find no evidence of labor market damage: prime-age employment ($\beta = 0.24$, SE $= 0.25$), earnings ($\beta = 0.13$, SE $= 0.12$), separations, and hires all show insignificant, slightly positive coefficients. Crucially, elderly workers—minimally affected by opioid misuse—display the same positive pattern ($\beta = 0.38$, SE $= 0.28$), suggesting OxyContin brand share captures economic trends rather than opioid-specific variation. These results are consistent with the hypothesis that the instrument validated for pharmacological substitution may not satisfy the exclusion restriction for labor market inference.

JEL Codes: I12, J21, K32

Keywords: opioid crisis, OxyContin reformulation, labor markets, heroin substitution, reduced form

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1. Introduction

The United States lost more than 600,000 lives to opioid overdoses between 1999 and 2021, a toll that reshaped communities, strained public services, and—many fear—permanently damaged local labor markets (Centers for Disease Control and Prevention, 2022). A growing body of research documents the economic costs of opioid exposure: reduced labor force participation (Krueger, 2017), lower employment rates (Harris et al., 2020), and diminished earnings (Currie and Schwandt, 2021). These findings have motivated billions of dollars in policy responses, from litigation settlements to treatment expansions, premised on the assumption that opioid exposure causally impairs economic functioning.

Yet the causal chain from opioid exposure to labor market damage is difficult to establish. The correlation between local opioid intensity and weak labor markets may reflect reverse causality—economic distress drives substance use (Case and Deaton, 2015, 2017)—or omitted variables that jointly determine both. Credible identification requires exogenous variation in opioid exposure that is plausibly orthogonal to labor market fundamentals.

The 2010 OxyContin abuse-deterrent reformulation provides precisely such variation—or so it seems. In August 2010, Purdue Pharma replaced its original, easily crushed OxyContin tablets with an abuse-deterrent formulation that resisted crushing, snorting, and injection. Alpert et al. (2018) demonstrate that this reformulation caused dramatic substitution toward heroin in counties with higher pre-reform dependence on OxyContin brand pills, and Evans et al. (2019) show that the resulting heroin transition increased mortality. The pre-reform OxyContin brand share of oxycodone shipments has become a workhorse instrument in the opioid literature.

This paper asks whether the reduced-form relationship between pre-reform OxyContin brand share and labor market outcomes reveals employment scarring from the prescription-to-illicit transition. We construct a county-level panel linking 178.6 million DEA ARCOS opioid transaction records with Census Quarterly Workforce Indicators (QWI) employment, earnings, and worker flow data for 2,619 counties over 2005–2019. Our continuous-treatment difference-in-differences design compares labor market trajectories of counties with high versus low OxyContin brand dependence, before and after the 2010 reformulation.

The answer is no. We find no reduced-form evidence that reformulation exposure harmed local labor markets. Point estimates for prime-age (25–44) workers are uniformly positive and statistically insignificant: $\beta = 0.24$ (SE $= 0.25$) for log employment, $\beta = 0.13$ (SE $= 0.12$) for log earnings, $\beta = 0.06$ (SE $= 0.16$) for separation rates, and $\beta = 0.04$ (SE $= 0.20$) for hire rates. The 95% confidence interval for employment rules out negative effects larger than 0.24 log points—we can reject the hypothesis of substantial labor market damage with reasonable

precision.

A key diagnostic deepens this null. If OxyContin brand share captures opioid-specific variation, its post-reform effects should concentrate among demographics most vulnerable to opioid misuse: prime-age workers, particularly men. Instead, we find strikingly uniform positive coefficients across all age groups. Young workers (14–24) show a significant positive coefficient ($\beta = 0.31$, SE $= 0.12$), and elderly workers aged 65 and over—a group with minimal exposure to opioid misuse—display the largest positive point estimate ($\beta = 0.38$, SE $= 0.28$). The absence of differential effects across ages is difficult to reconcile with an opioid-specific mechanism and instead suggests that OxyContin brand share correlates with unobserved county-level economic trajectories.

These results are robust to alternative instrument windows (2008–2009 only), trimming extreme OxyContin-share counties, two-way clustering, and dropping population weights. The unweighted specification yields a statistically significant positive coefficient ($\beta = 0.16$, SE $= 0.07$), further indicating that the instrument captures economic conditions rather than opioid exposure per se.

This paper contributes to three literatures. First, we add to the growing body of work on the economic consequences of the opioid crisis (Powell, 2020; Park and Look, 2020; Ouimet et al., 2023; Doleac and Mukherjee, 2022). While prior studies document correlations between opioid prescribing and labor market outcomes, or exploit state-level policy variation such as prescription drug monitoring programs (Buchmueller and Carey, 2018; Kaestner and Ziedan, 2019), no paper has examined the reduced-form relationship between reformulation exposure and labor market outcomes. Our null reduced form is informative: it is consistent with the hypothesis that the prescription-to-illicit substitution channel, though devastating for mortality, did not measurably worsen county-level employment.

Second, we contribute to the econometric literature on instrument validity and domain boundaries (Angrist et al., 1996; Imbens, 2014; Andrews et al., 2019). The first-stage relationship between OxyContin brand share and post-reform heroin mortality is well-established by Alpert et al. (2018) and has been replicated across studies (Evans et al., 2019; Powell, 2020). We do not re-estimate this first stage; our contribution is to show that the reduced form on labor market outcomes is null. This null reduced form, combined with a strong first stage for pharmacological substitution, illustrates what we call the "instrument boundary" problem: an instrument validated for one outcome domain may not satisfy the exclusion restriction in another. This has practical implications for the increasingly common practice of repurposing natural experiments across outcome domains.

Third, we speak to the broader literature on economic "deaths of despair" (Case and Deaton, 2015, 2017; Ruhm, 2019; Pierce and Schott, 2020). Our null reduced form does not

mean that opioids are irrelevant to labor markets; it means that the specific variation induced by the OxyContin reformulation does not appear to isolate an opioid-to-labor channel. The distinction matters for policy: targeted interventions addressing the prescription-to-illicit transition may reduce mortality without commensurately improving employment, because the labor market effects of opioid exposure operate through different channels or at different intensities than the pharmacological substitution captured by this instrument.

The remainder of the paper proceeds as follows. Section 2 describes the OxyContin reformulation and the heroin substitution mechanism. Section 3 details our data sources and sample construction. Section 4 presents the empirical strategy. Section 5 reports results. Section 6 interprets the findings, and Section 7 concludes.

2. Institutional Background

OxyContin, a sustained-release oxycodone formulation manufactured by Purdue Pharma, was introduced in 1996 and became the most widely prescribed long-acting opioid in the United States. By the mid-2000s, OxyContin was also the most commonly diverted and abused prescription opioid. Its original tablet formulation could be easily crushed into powder, enabling users to defeat the sustained-release mechanism and obtain the full opioid dose immediately—by snorting or dissolving and injecting the powder (Butler et al., 2013; Cicero and Ellis, 2012).

On August 5, 2010, Purdue Pharma reformulated OxyContin with abuse-deterrent properties (ADF). The new tablets formed a viscous gel when crushed or dissolved, making them resistant to snorting and injection while maintaining the same active ingredient and therapeutic effect for oral consumption. The reformulation was approved by the FDA and replaced the original formulation on pharmacy shelves over a period of weeks. Crucially, the reformulation did not change the total supply of oxycodone—generic versions and other brands remained available—but it eliminated the highest-value route of administration for the dominant brand (Alpert et al., 2018).

The consequences were swift and severe. Alpert et al. (2018) show that counties with higher pre-reform OxyContin brand share experienced differentially larger increases in heroin deaths after 2010, consistent with users substituting from prescription opioids to heroin when their preferred formulation became abuse-deterrent. Evans et al. (2019) extend this finding, documenting that the reformulation accelerated the transition from the "first wave" of the opioid crisis (prescription opioids) to the "second wave" (heroin) and ultimately the "third wave" (synthetic fentanyl). The estimated mortality effects are large: a one-standard-deviation increase in OxyContin exposure is associated with a roughly 10% increase in heroin

death rates in the post-reform period.

The theoretical link to labor markets operates through several channels. Heroin and fentanyl use impairs workplace functioning, increases absenteeism, and raises the probability of incarceration, disability, and death—all of which reduce labor supply (Savych et al., 2019; Maclean et al., 2022). On the demand side, employers in high-exposure areas may face increased drug-testing failures, workplace accidents, and higher insurance costs, reducing hiring (Doleac and Mukherjee, 2022). These channels predict that counties more exposed to the prescription-to-illicit transition should exhibit declining employment, lower earnings, higher separations, and reduced hiring rates.

However, the labor market prediction rests on a stronger assumption than the mortality prediction. For mortality, the mechanism is direct: heroin is more lethal than prescription opioids, so substitution toward heroin mechanically increases overdose deaths. For labor markets, the mechanism requires that the marginal heroin users created by reformulation were employed or employable prior to the transition—an assumption that may not hold if the population most affected by reformulation-induced substitution was already detached from the labor force. Furthermore, labor market outcomes are influenced by many factors beyond opioid exposure, and the county-level characteristics that determined OxyContin brand share (marketing territories, distributor networks, prescriber preferences) may correlate with economic trajectories through channels unrelated to opioids.

3. Data

3.1 DEA ARCOS: Instrument Construction

The Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS) tracks every transaction of Schedule II and III controlled substances from manufacturer to retail point of sale. We use the ARCOS transaction-level data covering 2006–2012, which contains 178.6 million opioid pill transactions with information on the drug name, dosage, quantity, buyer (pharmacy, hospital, practitioner), and geographic location (Drug Enforcement Administration, 2019).

We construct the instrument—pre-reform OxyContin brand share—as follows. For each county c, we compute the share of total oxycodone pill shipments (in morphine milligram equivalents) that were OxyContin brand during the pre-reform period 2006–2009:

$$\text{OxyShare}_c = \frac{\sum_{t=2006}^{2009} \text{OxyContin MME}_{ct}}{\sum_{t=2006}^{2009} \text{Total Oxycodone MME}_{ct}} \tag{1}$$

This measure captures a county's reliance on the specific formulation affected by the refor-

mulation. Higher values indicate greater dependence on the original, crushable OxyContin tablets and thus greater exposure to the supply disruption caused by reformulation.

The instrument exhibits substantial cross-county variation: the mean OxyContin brand share is 0.106 (SD = 0.069), with an interquartile range of approximately 0.06 to 0.15. Counties in the lowest quartile average 4.3% OxyContin share, while those in the highest quartile average 19.2%. We exclude counties with population below 1,000 or zero oxycodone shipments, leaving 2,619 counties in the analysis sample.

3.2 Census QWI: Labor Market Outcomes

The Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program, provide county-quarter-level tabulations of employment, earnings, hires, and separations derived from state unemployment insurance records covering approximately 95% of private-sector employment (Abowd et al., 2009). We aggregate QWI data to the county-year level across all industries, separately by age group (14–24, 25–44, 45–64, 65+).

Our primary outcomes are: (i) log beginning-of-quarter employment (number of workers with UI-covered earnings), (ii) log average monthly earnings, (iii) quarterly separation rate (separations divided by employment), and (iv) quarterly hire rate (hires divided by employment). We focus on prime-age workers (25–44) as the primary demographic, with other age groups serving as heterogeneity checks and placebo tests.

3.3 Sample and Controls

The analysis sample spans 2005–2019, yielding 15 annual observations per county and 39,285 county-year observations. We weight regressions by county population from the 2010 American Community Survey five-year estimates. Controls include total opioid pills per capita interacted with year fixed effects, which absorbs the overall level of prescribing intensity and isolates variation in brand composition conditional on the prescribing environment.

Table 1 presents summary statistics. Counties in the highest OxyContin-share quartile have slightly smaller average employment (15,162 vs. 19,006 in Q3) and similar earnings, hire rates, and separation rates to those in the lowest quartile. This lack of strong sorting on pre-reform labor market characteristics is consistent with the instrument being driven by supply-side pharmaceutical distribution rather than labor market fundamentals.

Table 1: Summary Statistics: Pre-Reform County Characteristics (2005–2009)

	N	Emp (mean)	Earnings (mean)	Hire Rate	Sep Rate	OxyContin Share
Full Sample	2619	14,665	\$2,796	0.815	0.825	0.105
Q1 (Low share)	655	6,278	\$2,626	0.844	0.853	0.043
Q2	655	18,219	\$2,859	0.811	0.821	0.077
Q3	654	19,006	\$2,893	0.781	0.791	0.107
Q4 (High share)	655	15,162	\$2,807	0.824	0.832	0.192

Notes: Employment, earnings, hire rates, and separation rates are from QWI for prime-age workers (25–44), averaged over 2005–2009. OxyContin Share is the county-level share of oxycodone pills that were OxyContin brand, from DEA ARCOS (2006–2009 average). Quartiles based on OxyContin Share. Counties with population $< 1{,}000$ or zero oxycodone excluded.

4. Empirical Strategy

4.1 Specification

Our primary specification is a continuous-treatment difference-in-differences model:

$$Y_{ct} = \beta \left(\text{OxyShare}_c \times \text{Post}_t \right) + \alpha_c + \gamma_t + \left(\text{TotalOxy}_c \times \gamma_t \right) + \varepsilon_{ct} \tag{2}$$

where Y_{ct} is the labor market outcome in county c and year t; OxyShare_c is the pre-reform OxyContin brand share defined in equation (1); $\text{Post}_t = \mathbb{I}[t \geq 2010]$; α_c and γ_t are county and year fixed effects; and $\text{TotalOxy}_c \times \gamma_t$ absorbs year-specific effects of overall opioid prescribing intensity. Standard errors are clustered at the state level to account for within-state correlation in both the instrument and labor market shocks.

The coefficient β estimates the differential change in outcomes for a county with one-unit-higher OxyContin brand share after the 2010 reformulation, relative to the pre-reform period. Under the identifying assumption, β captures the causal effect of the prescription-to-illicit opioid transition on labor markets, because the reformulation affected counties in proportion to their dependence on the original OxyContin formulation.

We also estimate a dynamic event-study specification:

$$Y_{ct} = \sum_{k \neq 2009} \beta_k \left(\text{OxyShare}_c \times \mathbb{I}[t = k] \right) + \alpha_c + \gamma_t + \left(\text{TotalOxy}_c \times \gamma_t \right) + \varepsilon_{ct} \tag{3}$$

where the β_k coefficients trace the year-by-year relationship between OxyContin brand share and labor market outcomes, with 2009 as the omitted reference year. Pre-reform coefficients

($k < 2010$) test for differential pre-trends; post-reform coefficients ($k \geq 2010$) estimate the dynamic treatment effect.

4.2 Identification

The identifying assumption is that, conditional on county fixed effects, year fixed effects, and total opioid prescribing intensity, the OxyContin brand share is uncorrelated with county-specific labor market trends. In other words, absent the reformulation, high- and low-OxyContin-share counties would have followed parallel labor market trajectories.

This assumption is motivated by the source of variation in brand share. OxyContin's market penetration relative to generic oxycodone depended on Purdue Pharma's sales force deployment, distributor contracts, and local prescriber brand preferences—factors determined by pharmaceutical marketing strategies rather than county economic conditions (Alpert et al., 2018; Van Zee and King, 2019). Conditional on total oxycodone per capita (which absorbs the demand-side determinants of prescribing), the brand composition reflects supply-side idiosyncrasies.

We probe this assumption in three ways. First, the event-study specification (Equation (3)) reveals any differential pre-trends. Second, we test whether effects are specific to opioid-affected demographics (prime-age workers) or present uniformly across age groups—the latter would suggest the instrument captures economic trends rather than opioid exposure. Third, we vary the instrument window, sample, weighting, and clustering to assess sensitivity.

5. Results

5.1 Main Results

Table 2 reports the static difference-in-differences estimates from equation (2) for prime-age workers (25–44). Across all four labor market outcomes, the estimated effect of OxyContin reformulation exposure is positive and statistically insignificant. The employment coefficient ($\beta = 0.24$, SE $= 0.25$) implies that a county at the 90th percentile of OxyContin share (0.18) relative to the 10th percentile (0.04) experienced a differential increase of roughly $0.24 \times 0.14 = 0.034$ log points in employment—an economically negligible 3.4% increase that is statistically indistinguishable from zero.

The earnings estimate ($\beta = 0.13$, SE $= 0.12$) has a t-statistic of 1.13, modestly suggestive of higher earnings in high-exposure counties but far from conventional significance thresholds. Separation and hire rate coefficients are even smaller and less precisely estimated. The 95% confidence interval for the employment effect spans $[-0.24, 0.73]$, allowing us to rule out

Table 2: OxyContin Reformulation Exposure and Prime-Age Labor Market Outcomes

	Log Emp (1)	Log Earn (2)	Sep Rate (3)	Hire Rate (4)
OxyContin Share × Post	0.2401	0.1326	0.0620	0.0399
	(0.2470)	(0.1170)	(0.1643)	(0.1952)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	39,285	39,285	39,285	39,285
Counties	2,619	2,619	2,619	2,619

Notes: Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. OxyContin Share is the pre-reform (2006–2009) county-level share of oxycodone that was OxyContin brand from DEA ARCOS. Post = 1 for years $\geq$ 2010. All regressions control for total opioid pills per capita × year. Prime-age workers (25–44). Population-weighted. Sample: 2005–2019.

negative effects exceeding 0.24 log points—a substantively meaningful bound given that the expected negative effects from heroin substitution would need to be large to register at the county level.

5.2 Event Study

The event-study specification (Equation (3)) reveals flat pre-trends and no post-reform employment decline. Pre-reform coefficients for log employment are small, negative, and statistically insignificant: $\hat{\beta}_{-5} = -0.198$ (SE = 0.327), $\hat{\beta}_{-4} = -0.179$ (SE = 0.245), $\hat{\beta}_{-3} = -0.124$ (SE = 0.175), and $\hat{\beta}_{-2} = -0.042$ (SE = 0.086), with 2009 as the omitted reference year. The monotonic convergence toward zero is consistent with parallel pre-trends. Post-reform coefficients drift slightly positive—peaking at $\hat{\beta}_3 = 0.201$ (SE = 0.091) in 2013—before attenuating toward zero through 2019 ($\hat{\beta}_9 = 0.098$, SE = 0.187). There is no evidence of a negative break around 2010. If anything, the pattern suggests that high-OxyContin-share counties experienced slightly *better* employment trajectories, consistent with the instrument capturing local economic recovery rather than opioid damage. Table 5 in the appendix reports the full set of event-study coefficients for all four outcomes.

5.3 Statistical Power

The 95% confidence interval for the employment coefficient ranges from approximately −0.24 to 0.73 in OxyContin share units. A one-standard-deviation increase in OxyContin share (0.069) implies the design can rule out employment effects larger than approximately $0.069 \times 0.24 \approx 1.7$ percentage points at the 95% level. While this precision rules out

Table 3: Heterogeneity by Age Group: Employment Effects of Reformulation Exposure

	All Ages	Prime-Age (25–44)	Young (14–24)	Older (45–64)	Elderly (65+)
	(1)	(2)	(3)	(4)	(5)
OxyContin Share × Post	0.2290	0.2401	0.3059**	0.2167	0.3797
	(0.2108)	(0.2470)	(0.1216)	(0.2132)	(0.2801)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	39,285	39,285	39,285	39,285	39,285

Notes: Dependent variable: log employment. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. All specifications include county and year FE and control for total opioid pills per capita × year. Population-weighted. Elderly (65+) serves as a placebo group less affected by opioid misuse.

catastrophic employment collapse from the prescription-to-illicit transition, it does not exclude more modest effects of the kind documented for individual-level opioid exposure in prior work (Krueger, 2017; Harris et al., 2020). The county-level design necessarily aggregates across individuals with heterogeneous exposure, diluting any signal that may exist among directly affected workers.

5.4 Age Heterogeneity as Falsification

Table 3 presents the employment effect by age group, serving as both a heterogeneity analysis and a diagnostic test of instrument validity. If OxyContin brand share captures opioid-specific variation, its effects should concentrate among prime-age adults most vulnerable to opioid misuse and be absent among elderly workers (65+), whose opioid misuse rates are an order of magnitude lower.

The results are striking: all age groups display positive, similarly sized coefficients. Young workers (14–24) show the only statistically significant effect ($\beta = 0.31$, SE $= 0.12$, $p < 0.05$), while elderly workers exhibit the largest point estimate ($\beta = 0.38$, SE $= 0.28$). The lack of differential effects across demographics is difficult to reconcile with an opioid-specific mechanism. Instead, this pattern suggests that pre-reform OxyContin brand share is correlated with county economic trajectories—areas where OxyContin had higher market penetration may differ systematically in ways that predict economic recovery after the Great Recession. The elderly coefficient is particularly informative: there is no plausible opioid channel through which reformulation exposure would increase employment among workers aged 65 and over.

Table 4: Robustness of Employment Effects

	Baseline (1)	2008–09 IV (2)	Trimmed (3)	Unweighted (4)	Alt. Cluster (5)
OxyContin Share × Post	0.2401	0.1506	0.3692	0.1560**	0.2401
	(0.2470)	(0.1842)	(0.3463)	(0.0720)	(0.2347)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	39,285	39,195	35,355	39,285	39,285

Notes: Dependent variable: log employment, prime-age (25–44). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Column 1: baseline (state-clustered, population-weighted). Column 2: instrument from 2008–2009 only. Column 3: drops top/bottom 5% of OxyContin share. Column 4: unweighted. Column 5: two-way clustered (state × year). County and year FE throughout.

5.5 Robustness

Table 4 presents five robustness checks. Column 1 reproduces the baseline. Column 2 restricts the instrument to a narrower window (2008–2009 only), reducing potential contamination from early pre-reform market dynamics; the coefficient attenuates slightly ($\beta = 0.15$, SE $= 0.18$) but remains positive and insignificant. Column 3 trims the top and bottom 5% of the OxyContin share distribution, and the coefficient increases ($\beta = 0.37$, SE $= 0.35$), suggesting outlier counties are not driving the null. Column 4 drops population weights, yielding a significant positive coefficient ($\beta = 0.16$, SE $= 0.07$, $p < 0.05$). This result is important: the unweighted estimate—which gives equal influence to small and large counties—shows that OxyContin brand share predicts *better* employment outcomes, reinforcing the interpretation that the instrument captures economic trends rather than opioid damage. Column 5 clusters standard errors at the state-by-year level; the coefficient is unchanged and the standard error slightly smaller.

6. Discussion

6.1 Why the Null?

The OxyContin reformulation instrument is well-validated for its original purpose: identifying the causal effect of the reformulation on heroin substitution and overdose mortality (Alpert et al., 2018; Evans et al., 2019). The first-stage relationship between pre-reform OxyContin brand share and post-reform heroin deaths is strong, well-identified, and has been replicated across multiple studies. Why, then, does the reduced form on labor market outcomes show no effect?

We propose three complementary explanations. First, the exclusion restriction may not hold for labor market outcomes. The reformulation instrument isolates variation in the *composition* of opioid supply (brand vs. generic oxycodone), not the *level*. For mortality, composition matters directly: heroin is more lethal than prescription opioids, so reformulation-induced substitution mechanically increases deaths. For labor markets, the relevant variation is whether individuals are using opioids at all, not which specific opioid they use. A worker who switches from diverted OxyContin to heroin may experience similar labor market impairment under both regimes, making the reformulation uninformative for employment outcomes even if it is highly informative for mortality.

Second, the affected population may be largely disconnected from formal labor markets. Individuals who used diverted OxyContin through non-oral routes (the population most affected by the reformulation) likely exhibited severe substance use disorder. Many may have already been outside the QWI sample—unemployed, disabled, incarcerated, or working informally—before 2010. If the reformulation primarily affected people already detached from UI-covered employment, even large health consequences would not register in QWI data.

Third, the age heterogeneity results suggest that OxyContin brand share correlates with unobserved county-level economic dynamics. The uniform positive coefficients across all age groups, including elderly workers, are difficult to reconcile with an opioid-specific mechanism and instead suggest that pharmaceutical brand market shares proxy for features of the local economy (healthcare infrastructure, commercial development, proximity to distribution networks) that also predict post-recession recovery. This is a specific instance of the broader challenge identified by Goldsmith-Pinkham et al. (2020): instruments derived from supply-side variation in product markets may satisfy the exclusion restriction for health outcomes but not for economic outcomes that respond to the same underlying market characteristics.

6.2 What This Paper Does and Does Not Estimate

It is important to be precise about the estimand. This paper reports reduced-form estimates of the relationship between pre-reform OxyContin brand share and labor market outcomes. We do not estimate a two-stage least squares (2SLS) model because the age-heterogeneity evidence (Table 3) suggests the exclusion restriction may not hold in the labor market domain—using OxyContin brand share as an instrument for heroin exposure in an employment regression would require assuming that brand share affects employment only through the heroin substitution channel, an assumption our falsification test casts doubt on.

The first stage—OxyContin brand share predicting post-reform heroin deaths—is well-established by Alpert et al. (2018) and has been independently replicated by Evans et al. (2019) and Powell (2020). We take this first stage as given. Our contribution is to document

that the reduced form on labor market outcomes is null. Combined with the strong first stage for pharmacological substitution, this null reduced form implies one of two things: either the labor market effects of the prescription-to-illicit transition are small at the county level, or the instrument picks up confounding economic trends that mask any negative labor market effect. The age-heterogeneity evidence favors the latter interpretation, but our evidence is consistent with—rather than definitively proving—this conclusion.

6.3 Implications for the Opioid-Labor Literature

Our null reduced-form result has three implications. First, researchers should exercise caution when applying the reformulation instrument to labor market outcomes. The instrument is well-validated for pharmacological substitution and its mortality consequences, but this validity does not automatically extend to other outcome domains. This echoes Angrist et al.'s (1996) reminder that instrumental variables identify *local* average treatment effects: the complier population for heroin substitution (individuals who switch to heroin because of reformulation) may differ fundamentally from the complier population relevant for labor markets.

Second, the significant positive coefficient in the unweighted specification (Table 4, column 4) suggests that OxyContin brand share may introduce spurious positive bias in labor market regressions. Studies that use this instrument and find null or slightly positive labor market effects should not necessarily interpret these as evidence that opioids do not harm labor markets; rather, the instrument may simply not isolate the relevant variation.

Third, the distinction between mortality and labor market channels matters for policy. Interventions targeting the prescription-to-illicit transition—naloxone distribution, supervised consumption sites, medication-assisted treatment—may be highly effective at reducing deaths (Wen et al., 2017; Dave et al., 2019) without commensurately improving employment, because the labor market effects of opioid exposure may operate through different channels (e.g., early-career human capital disruption, employer stigma, criminal records from drug enforcement) that are not captured by the reformulation instrument.

7. Conclusion

We estimate the reduced-form relationship between the OxyContin reformulation instrument—validated for identifying heroin substitution—and county-level labor market outcomes, finding no evidence of employment scarring. The null is informative, not merely uninformative. The uniformly positive coefficients across age groups, the significant positive effect in unweighted specifications, and the absence of any break around the 2010 reformulation are collectively

consistent with the interpretation that OxyContin brand share captures local economic conditions rather than opioid-specific variation in the labor market context.

This finding delineates an "instrument boundary": the reformulation provides clean identification of pharmacological substitution but may not cleanly identify labor market effects, because the exclusion restriction that holds for mortality outcomes may not hold for employment. The opioid crisis may well damage labor markets, but the evidence for that claim must come from variation other than the brand composition of prescription opioid supply.

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A. Data Appendix

ARCOS Data Processing. The DEA ARCOS transaction data contain records of every Schedule II and III controlled substance shipment from manufacturers and distributors to retail-level buyers (pharmacies, hospitals, practitioners). We download the full transaction files for 2006–2012, totaling 178.6 million records. Each record includes the drug name, National Drug Code (NDC), dosage unit, quantity, buyer DEA registration number, and buyer ZIP code. We identify OxyContin transactions by matching on drug name fields containing "OXYCONTIN" and compute morphine milligram equivalents (MME) using standard conversion factors (1 mg oxycodone = 1.5 MME). We aggregate from ZIP code to county using the HUD ZIP-FIPS crosswalk, achieving a 94.9% match rate (2,955 of approximately 3,100 counties with any oxycodone). Counties with zero oxycodone shipments or population below 1,000 are excluded.

QWI Data Processing. We obtain QWI data for all 51 states (including DC) from the Census Bureau's LEHD public-use files stored in Azure. The data are provided at the county-quarter-age-industry level. We aggregate across industries (all private sector) and quarters (annual means) to obtain county-year-age observations. Age categories follow QWI definitions: A01 (14–18), A02 (19–21), A03 (22–24), A04 (25–34), A05 (35–44), A06 (45–54), A07 (55–64), A08 (65+). We combine A04–A05 for prime-age (25–44) and A06–A07 for older (45–64) workers. The four outcome variables are beginning-of-quarter employment (`Emp`), average monthly earnings (`EarnS`), new hires (`HirA`), and separations (`Sep`). Hire and separation rates are computed as the ratio to beginning-of-quarter employment. Employment and earnings are log-transformed.

Population Weights. County population estimates come from the 2010 American Community Survey 5-year estimates. These serve as analytic weights in the baseline specification and define the county population threshold for sample inclusion.

B. Robustness Appendix

Alternative Instrument Windows. The baseline instrument uses the 2006–2009 average OxyContin brand share. We verify robustness to a shorter window (2008–2009 only), which reduces the influence of early ARCOS data that may reflect different market dynamics. The correlation between the four-year and two-year instruments is 0.97, and results are substantively unchanged (Table 4, column 2).

Trimmed Sample. Dropping the top and bottom 5% of the OxyContin share distribution (counties with share below 0.02 or above 0.22) removes 10% of the sample and slightly increases the point estimate, confirming that extreme-exposure counties are not driving the null.

Unweighted Estimates. The significant positive coefficient in the unweighted specification ($\beta = 0.156$, $p < 0.05$) indicates that smaller counties—which receive more weight without population weighting—show a stronger positive association between OxyContin share and employment growth. This is consistent with OxyContin brand share proxying for commercial activity or healthcare infrastructure that also predicts economic recovery in rural areas.

Alternative Clustering. Two-way clustering by state and year (column 5) produces slightly tighter standard errors than state-only clustering, as expected given the modest number of time periods. The substantive conclusions are unchanged.

C. Event Study Coefficients

D. Standardized Effect Sizes

Table 5: Event Study Coefficients: OxyContin Share × Year

Event Time	Year	Log Emp (1)	Log Earn (2)	Sep Rate (3)	Hire Rate (4)
Pre-reform (parallel trends test)					
−5	2005	−0.198	0.017	−0.155	−0.145
		(0.327)	(0.132)	(0.148)	(0.192)
−4	2006	−0.179	−0.011	−0.039	−0.022
		(0.245)	(0.080)	(0.179)	(0.219)
−3	2007	−0.124	0.037	−0.034	0.039
		(0.175)	(0.049)	(0.159)	(0.205)
−2	2008	−0.042	0.075***	0.042	0.115
		(0.086)	(0.014)	(0.085)	(0.144)
−1	2009	[Reference year]			
Post-reform (treatment effects)					
0	2010	0.036**	0.026	−0.039	0.018
		(0.016)	(0.017)	(0.048)	(0.060)
1	2011	0.102**	0.113***	0.002	0.080
		(0.041)	(0.031)	(0.069)	(0.070)
2	2012	0.161**	0.168***	0.082	0.140**
		(0.071)	(0.064)	(0.065)	(0.062)
3	2013	0.201**	0.186**	0.072*	0.076*
		(0.091)	(0.088)	(0.043)	(0.046)
4	2014	0.200*	0.246**	0.112***	0.130***
		(0.115)	(0.119)	(0.037)	(0.037)
5	2015	0.176	0.213**	0.094*	0.038
		(0.132)	(0.094)	(0.054)	(0.081)
6	2016	0.127	0.145**	−0.005	−0.031
		(0.130)	(0.072)	(0.077)	(0.093)
7	2017	0.108	0.160**	−0.002	0.002
		(0.144)	(0.072)	(0.075)	(0.083)
8	2018	0.107	0.168**	−0.022	−0.015
		(0.158)	(0.073)	(0.105)	(0.100)
9	2019	0.098	0.137*	−0.047	−0.065
		(0.187)	(0.074)	(0.119)	(0.108)

Notes: Each cell reports the coefficient on OxyContin Share × $\mathbb{I}$[Year = k] from equation (3). Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Event time −1 (2009) is the omitted reference year. Pre-reform coefficients test for differential pre-trends; post-reform coefficients estimate dynamic treatment effects. All regressions include county and year FE and control for total opioid pills per capita × year. Population-weighted. Prime-age workers (25–44).

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled (Prime-Age 25–44)						
Log Employment	0.2401	0.2470	1.6084	0.0103	0.0106	Small positive
Log Earnings	0.1326	0.1170	0.1955	0.0467	0.0412	Small positive
Separation Rate	0.0620	0.1643	0.2314	0.0185	0.0489	Small positive
Hire Rate	0.0399	0.1952	0.2446	0.0112	0.0550	Small positive
Panel B: Heterogeneous (Sample Splits by Age)						
Log Emp (Prime-Age 25–44)	0.2401	0.2470	1.6084	0.0103	0.0106	Small positive
Log Emp (Older 45–64)	0.2167	0.2132	1.5408	0.0097	0.0095	Small positive

Notes: **Country:** United States. **Research question:** Does the 2010 OxyContin abuse-deterrent reformulation, which shifted opioid misuse from prescription to illicit sources, cause labor market scarring in exposed communities? **Policy mechanism:** Purdue Pharma replaced crushable OxyContin with an abuse-deterrent formulation in August 2010, eliminating the ability to crush, snort, or inject the pill; counties with higher pre-reform OxyContin brand dependence experienced larger disruptions to their divertible opioid supply, forcing users toward heroin and illicit fentanyl. **Outcome definition:** QWI log employment (beginning-of-quarter count of workers with UI-covered earnings), log average monthly earnings, quarterly separation rate, and quarterly hire rate for prime-age workers (25–44). **Treatment:** Continuous; pre-reform (2006–2009) county-level OxyContin brand share of total oxycodone pill shipments from DEA ARCOS, interacted with a post-2010 indicator. **Data:** DEA ARCOS (178.6M transactions, 2006–2012) for the instrument; Census QWI/LEHD (county-quarter-age, 2005–2019) for outcomes; ACS 5-year population estimates for weights. **Method:** Continuous-treatment difference-in-differences with county and year fixed effects, controlling for total opioid pills per capita × year; standard errors clustered at state level (43 clusters); population-weighted. **Sample:** 2,619 counties with population $\geq$ 1,000 and positive oxycodone shipments, observed 2005–2019 (39,285 county-year observations). SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-county standard deviation of OxyContin share and SD(Y) is the pre-treatment standard deviation of each outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).