The Social Transmission of Non-Infectious Diseases: Evidence from the Opioid Epidemic*

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This Version: July 25, 2022

Abstract

What drives the spread of *non-infectious* diseases? We study this question in the context of the opioid epidemic. Having many friendship links to counties with high exposure to the opioid epidemic positively correlates with overdose death rates. This correlation is not driven by physical proximity and socio-economic characteristics. To establish causality, we exploit the 2010 OxyContin reformulation and the staggered introduction of must-access Prescription Drug Monitoring Programs (PDMPs) both of which led to the unintended consequence that users switched to illegal opioids, thereby constituting shocks to illegal drug consumption that are exogenous to friendship network formation. Having more friends exposed to counties severely affected by these adverse consequences leads to higher opioid overdoses, suggesting a causal friendship network effect.

JEL Codes: I12, I18, K42

Keywords: Opioids, Opioid Epidemic, Illicit Drugs, Social Networks, Social Connectedness Index

*We thank Pierre Dubois, Paige Ouimet, Simon Rother, Konrad Stahl, and seminar participants at the 2022 EuHEA conference, the FFMMA PhD workshop and the University of Mannheim for valuable feedback.

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1 Introduction

Many *infectious* diseases such as SARS-CoV-2 occur in waves that feature exponential growth in case numbers. Even though they are not physically transmissible, *non-infectious* diseases like substance use disorder, obesity, or depression are similar in that respect as they can disseminate quickly at the population level. A natural question is: What drives the spread of *non-infectious* diseases? We study this question in the context of the opioid epidemic.

The widespread prescribing of opioid-based painkillers and illicit use of related substances have led to a severe public health crisis in the U.S. that has claimed half a million lives over the past two decades. Opioid consumption is influenced by a wide range of socio-economic factors like personal characteristics, the availability of opioids, or potential network effects induced by the social environment. In this paper, we focus on the social environment and investigate friendship network effects in the context of the opioid epidemic. Our main contribution is to document that friendship networks are an important driver of the opioid epidemic. At a general level, our results provide some first evidence on the social transmission of *non-infectious* diseases.

To study the role of friendship networks for the spread of the opioid epidemic, we construct a new measure of *Social Proximity* to the opioid epidemic based on the Social Connectedness Index (SCI) (Bailey et al., 2018b), which is used to proxy for county-to-county friendship links. The SCI is a measure of real-world friendship networks and is constructed from the universe of friendship links on Facebook. Specifically, for each county we calculate *Social Proximity* to the epidemic as the SCI-weighted sum of overdose death rates in all other counties. First, we use panel regressions to correlate *Social Proximity* with the spatial spread of the epidemic. Our analyses are conducted at the county-level to be able to analyze the evolution of the opioid epidemic at a granular level. Second, to approach causality we exploit two quasi-experiments based on the 2010 OxyContin reformulation and the staggered introduction of must-access PDMPs across different states.

We find that *Social Proximity* to the epidemic significantly correlates with opioid overdose death rates. This result holds after controlling for demographic factors, economic conditions, and unobserved heterogeneity at the county and state-by-year level. Friendship networks tend to cluster locally around individuals' places of residence (Bailey et al., 2018b). Thus, we control for the physical proximity to the epidemic. Additionally, we conduct all analyses only based on data on county-pairs that are geographically distant and find that our results generally hold. Zooming in on the specific drugs that are involved, we find the correlations to hold for total opioid overdoses, prescription opioid overdoses, heroin overdoses individually, and combined heroin and fentanyl overdoses.

While we think the results described thus far are important in the sense of allowing a better prediction of which counties are more likely to be affected by the epidemic in the future and eventually implement preventive measures, they are obviously not sufficient to make any causal statements. Establishing causal effects of friendship networks in the context of the opioid epidemic is plagued by severe endogeneity concerns. Network formation is generally endogenous to overdose measures as individuals self-select into friendship networks based on partially unobserved characteristics. Previous authors show that individuals are more likely to be friends with individuals that share similar personal characteristics along dimensions such as race, ethnicity, age, religion, education, occupation, or gender (e.g., Lazarsfeld et al., 1954, McPherson et al., 2001).

Unobserved variables might determine both network formation and overdose deaths, which would cause us to inconsistently estimate friendship network effects. Kuchler and Stroebel (2021) highlight two ways to achieve identification of peer group effects: (i) random peer group assignment and (ii) random shocks to an endogenously formed peer group. We follow the latter strategy in our empirical analysis. In particular, we leverage two policy-induced shocks to illicit drug consumption as quasi-experiments: the 2010 OxyContin reformulation and the staggered introduction of must-access PDMPs in various states. The idea of our identification strategy is as follows. Both types of interventions make it harder to access legal opioid prescriptions, thereby driving individuals into consuming illegal alternatives like heroin and fentanyl. Hence, the OxyContin reformulation as well as the introduction of must-access PDMPs constitute unintentional shocks to illegal drug consumption that are potentially orthogonal to friendship network formation.

Using county-level data, we confirm that the OxyContin reformulation is not only associated with decreases in opioid prescriptions but also with substantial substitution to illicit alternatives like heroin and fentanyl. This effect is particularly strong in counties most affected by the reformulation, i.e., counties with high OxyContin usage rates prior to the reformulation, which is consistent with prior state level evidence from Alpert et al. (2018). We document that the reformulation actually led to an increase not only in heroin and fentanyl overdose death rates, but also in total opioid overdose death rates, i.e., the positive effect of lower opioid prescriptions was compensated by the substitution with other (more dangerous) opioids. Thus, we can use the reformulation as a quasi-experimental positive shock to total opioid overdoses in general and heroin as well as fentanyl overdoses in particular and analyze whether it spreads through friendship networks.

Central for our analysis of the spatial spread of the epidemic, we then show that there indeed exists a strong network effect as counties having closer ties to other counties more affected by the 2010 OxyContin reformulation experience an adverse impact on different overdose measures in the post-2010 period. The positive effect on total opioid overdose deaths is entirely driven by heroin and fentanyl overdose deaths. Consistent with a causal mechanism, this friendship network effect is still statistically significant and economically large even when considering geographically distant counties only. We also find no evidence for differential pre-trends between high and low OxyContin network exposure counties, validating our empirical setup.

Even though we carefully control for the direct effect of the OxyContin reformulation in the above setting, one might argue that our friendship network effect is at least partially driven by the direct effect of the reformulation. To address such concerns, we exploit a second setting where there is arguably no direct effect but only—if existent—an indirect network effect. We use the staggered introduction of must-access PDMPs in 16 states between 2007 and 2015. These state level programs collect data on patients' prescription history and make this information available for physicians. Must-access PDMPs require physicians to review patients' prescription records before prescribing strong pain medication.

In our analysis we first confirm prior studies and provide further evidence showing that must-access PDMPs are not only associated with reductions in opioid prescriptions, but also lead to increases in total opioid overdose deaths; particularly, in heroin as well as fentanyl overdose deaths. Hence, we establish that must-access PDMPs represent (unintended) positive shocks to illicit drug consumption in counties of implementing states.

As a key part of our empirical analysis, we then study how these shocks propagate through friendship networks to socially proximate counties in non-implementing states. Importantly, must-access PDMP legislation in other states has no direct impact on counties in non-implementing states. Absent any direct effect of the out-of-state must-access PDMP shock, we can cleanly identify the indirect friendship network effect. Our results show that illicit drug consumption spreads through friendship networks. For example, a one standard deviation increase in friendship network exposure to out-of-state must-access PDMPs is related to an increase in combined heroin and fentanyl overdoses by 1.59 deaths per 100,000 residents. The effect size is as large as 31.5% of the sample average.

We conduct a battery of robustness tests ensuring that our results are stable. In particular, one might worry that our results are biased due to correlated information exposure of individuals and their friendship networks. For example, as friendship networks cluster locally, individuals in a friendship network likely consume the same media. If media consumption influences individuals' decisions, we might wrongly attribute its impact to friendship networks. To alleviate any such concern, we control for

DMA-by-year fixed effects. Furthermore, we also explicitly control for labor market effects, time-varying effects of income differences, and migration between counties with different exposure to the epidemic. We find that our friendship network effect is stable across all specifications.

Related Literature

Our paper contributes to three different strands of literature. A growing literature explores the economic forces shaping the opioid epidemic. PDMPs seem to prevent opioid misuse but only if they have a must-access provision (Buchmueller and Carey, 2018). However, must-access PDMPs are also associated with substantial substitution to illicit alternatives (Mallatt, 2018, Balestra et al., 2021, Kim, 2021). Several studies analyze the effects of the 2010 introduction of an abuse-deterrent version of OxyContin using state level data (Alpert et al., 2018, Evans et al., 2019, Powell and Pacula, 2021). Alpert et al. (2018) show that this supply-side intervention significantly reduced OxyContin misuse, though, at the cost of a sharp rise in heroin overdose deaths. Complementary evidence by Evans et al. (2019) suggest that the reformulation of OxyContin did not bring about a reduction in combined heroin and opioid mortality but each prevented prescription opioid death was compensated by an additional heroin death, while Powell and Pacula (2021) document sustained negative long-term consequences.

Pierce and Schott (2020) show that regions exposed to Chinese competition have experienced larger growth rates in drug overdose deaths after the 2000 trade liberalization compared with low-exposure regions. Finkelstein et al. (2018) highlight the importance of place-specific effects for prescription opioid abuse. Alpert et al. (2019) discuss the role of the introduction of OxyContin in 1996 and the associated marketing activities as a potential cause for the opioid epidemic. They show that state-based triplicate prescription programs[1] were effective in curbing the promotion of opioids and as a result triplicate states are found to have lower growth rates of opioid overdoses. We contribute to this literature in two ways. First, we confirm the findings concerning the OxyContin reformulation and must-access PDMPs using county-level data. Second, and more importantly, we identify friendship networks to be an important driver of the opioid epidemic.

We also contribute to the recently growing literature on friendship networks and social ties. Historically, research on friendship networks has been scarce due to data availability issues. Only recently, Bailey et al. (2018b) have introduced the publicly available SCI based on de-identified Facebook data, triggering more work on friendship networks. Friendship networks have been shown to influence a

[1]Triplicate prescription programs require physicians to obey special regulations when prescribing controlled substances. Practically speaking, they create three copies of the prescription. One copy is kept by the physician for her own record, the second copy is stored by the pharmacy, and the third copy is sent to the state drug monitoring institution.

wide range of economic and social outcomes including social distancing during Covid-19 (Bailey et al., 2020b), international trade (Bailey et al., 2021a), housing markets (Bailey et al., 2018a), earned income tax credit claiming behavior (Wilson, 2020), access to capital (Rehbein and Rother, 2020, Kuchler et al., 2021a), insurance decisions (Hu, 2020), or peer-effects in product adoption (Bailey et al., 2021b).

More broadly, we contribute to the literature that studies the role of friendship networks for the spread of epidemics. On a general level, the potential role of friendship networks for the spread of infectious diseases has long been recognized (e.g., Keeling and Eames, 2005, Danon et al., 2011) and networks have been explicitly used in spatial epidemiological models (e.g., Newman, 2002, Mossong et al., 2008).

The availability of new data sources on human interactions has re-sparked the interest in this line of research (Buckee et al., 2021). Using the SCI, Kuchler et al. (2021b) show that the spatial spread of Covid-19 correlates with the strength of social ties arguing that friends are likely to meet in person and transmit the disease via personal contact. We differ from these authors as we focus on explaining the spread of *non-infectious* diseases like abusive opioid consumption and the causal role of friendship networks. Eisenberg et al. (2014) study risky behavior of college roommates and fail to document significant peer effects in illicit drug consumption. In contrast, we are the first to provide evidence on the spread of illicit drug consumption through geographically close and distant friendship networks. We are not aware of any existing studies investigating the role of friendship networks for the spread of *non-infectious* diseases.

The remainder of this article is structured as follows. Chapter 2 introduces the data used in our analysis. We present our baseline results in Section 3. Then, Section 4 documents evidence from two quasi-experiments. Concluding remarks are provided in Section 5.

2 Data

We combine several data sources, including, among others, data on county-to-county Facebook links, different overdose death measures, opioid supply data, and socio-economic control variables. These datasets provide information at the county-level, which allows studying spatial dynamics at a relatively fine geographic granularity. We describe our data in the next subsections.

2.1 Social Connectedness

We proxy for county-to-county friendship links using the SCI first introduced by Bailey et al. (2018b). This measure is based on the cross-section of all U.S. Facebook friendship links as of August 2020. The raw friendship data are anonymized, aggregated at the county-pair level, and scaled by the number county-pair Facebook users

$$SCI_{ij} = \frac{\text{\# of } Friendships_{ij}}{\text{\# of } FBUsers_i \times \text{\# of } FBUsers_j}, \tag{1}$$

where i and j denote the respective county-pair. The SCI is scaled to the range 0 to 1,000,000,000 and measures the relative probability of a Facebook friendship link between a user in county i and another user in county j. Prior authors argue that Facebook provides a tool for real-world friends to interact online (Gilbert and Karahalios, 2009, Hampton et al., 2011, Jones et al., 2013, Bailey et al., 2018b) and therefore the SCI allows a unique large-scale representation of U.S. friendship networks. Evidence provided by Bailey et al. (2018a,b, 2020a,b, 2021a), Kuchler et al. (2021b), and Rehbein and Rother (2020) is consistent with this notion. Moreover, Facebook usage rates seem to be relatively stable across income groups, education groups, and racial groups (Bailey et al., 2018b), further validating the SCI as an unbiased measure of real-world friendship networks. In our analysis, we rely on a snapshot of the universe of all Facebook friendship links as of August 2020. While time-varying information on friendship links might be preferable, Kuchler et al. (2021b) argue that the SCI is extremely stable over time. Hence, concerns related to using time-constant friendship data are of minor consideration here.[2]

2.2 Mortality Data

Mortality data for the years 1999 to 2019 come from the National Vital Statistics System (NVSS) Multiple Cause of Death data base which documents death rates by geography (national, state, and county), age group, race, gender, year and month of death, weekday of death, place of death, autopsy status, and underlying and multiple cause of death. We follow the Centers for Disease Control and Prevention (CDC) guidelines in computing opioid overdoses. First, we code deaths as overdoses by using the ICD-10 external cause of injury codes X40-X44 (unintentional), X60-X64 (suicide), X85 (homicide), and Y10-Y14 (undetermined). Second, we use drug identification codes, which provide information about the substances found in the body at death. There are four drug identification codes related to opioids: T40.1 for heroin, T40.2 for natural and semisynthetic opioids (e.g., oxycodone and hydrocodone), T40.3 for methadone, and T40.4 for synthetic opioids excluding methadone (e.g., fentanyl). We follow Kim (2021) and study four different measures of overdose deaths: (i) total opioid overdoses (T40.1-T40.4), (ii) prescription opioid overdoses (T40.2+T40.3), (iii) heroin overdoses (T40.1), and (iv) combined heroin

[2]We also have access to a version of the SCI based on a snapshot as of the year 2016. Our results are robust to using this older version.

and fentanyl overdoses (T40.1+T40.4).[3] It is important to note that these death categories indicate any presence of specific drugs in the system of the death person but are not exclusive in the sense that we can attribute the death to a single drug when multiple substances are mentioned on the death certificate. However, as Alpert et al. (2018) point out, changes in the presence of a drug over time are still clearly indicative of substitution patterns. Thus, we can use these data to characterize the spatial spread of the opioid epidemic and the relative importance of specific substances over time.

Figure 1 shows national trends in overdose death rates from 1999 through 2019. Total opioid over-

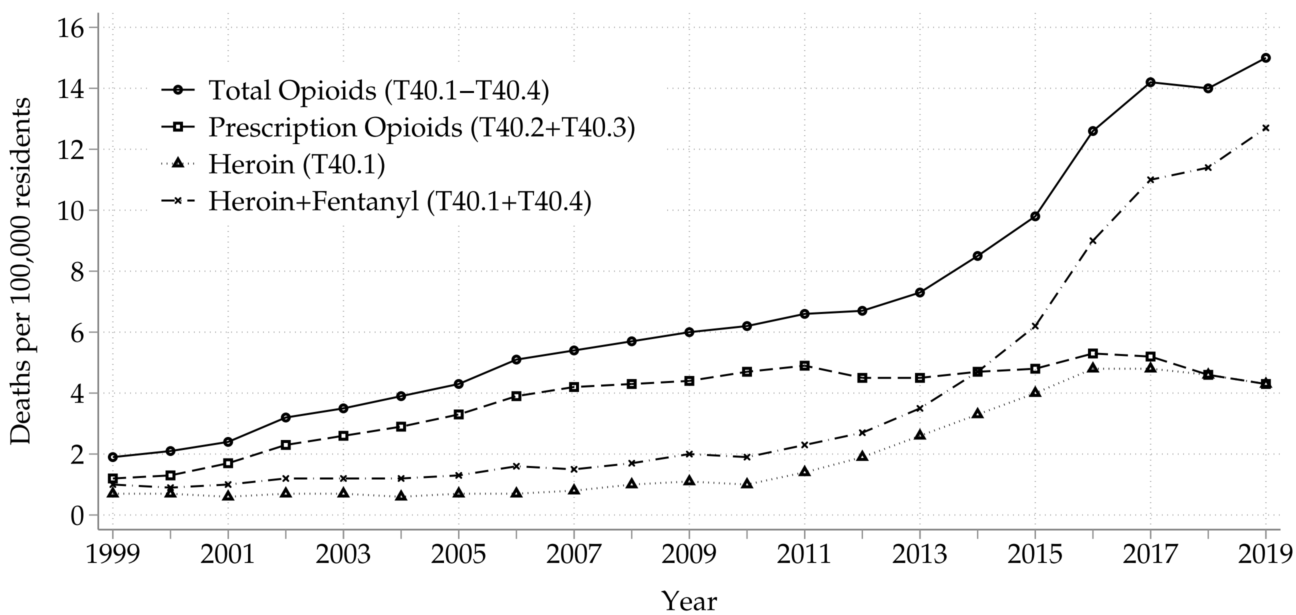

FIGURE 1—NATIONAL TRENDS IN DEATH RATES

Notes: The figure plots national trends in deaths rates from 1999 through 2019. Drug overdose death rates are identified using ICD-10 underlying cause of death codes X40-X44, X60-X64,X85, and Y10-Y14. We use the following multiple cause of death codes to measure specific drug involvement: T40.1 for heroin, T40.2 for prescription opioids containing active ingredients like oxycodone and hydrocodone, T40.3 for methadone, and T40.4 for synthetic opioids like fentanyl (excluding methadone). Data source: National Vital Statistics System (NVSS).

doses have increased from two to more than 14 deaths per 100,000 residents. We can also see the three waves of the epidemic: First, the rise in total opioid overdose deaths is mainly driven by prescription opioids, while the growth rate of prescription opioid overdose deaths has somewhat flattened out since 2010. Second, we see a strong increase in heroin overdose death rates from this time on. Third, more recently synthetic opioid overdoses (caused, e.g., by fentanyl) seem to be the main driver of the epidemic, while the growth in prescription opioid and heroin overdoses has diminished. Still, the total death toll

[3]These different overdose categories allow us to identify effects on total opioid overdoses as well as to disentangle substitution patterns among different legal (prescription opioids) and illegal substances (heroin and illegal fentanyl). We obtain qualitatively similar results when we use fentanyl overdoses as a separate category. However, we document our main results using combined heroin and fentanyl overdoses because heroin is often laced with fentanyl.

remains at historically high levels. To illustrate the geographic distribution of the opioid epidemic, Figure 2 shows a heatmap of total opioid overdose deaths (T40.1-T40.4) between 1999 and 2019. Darker

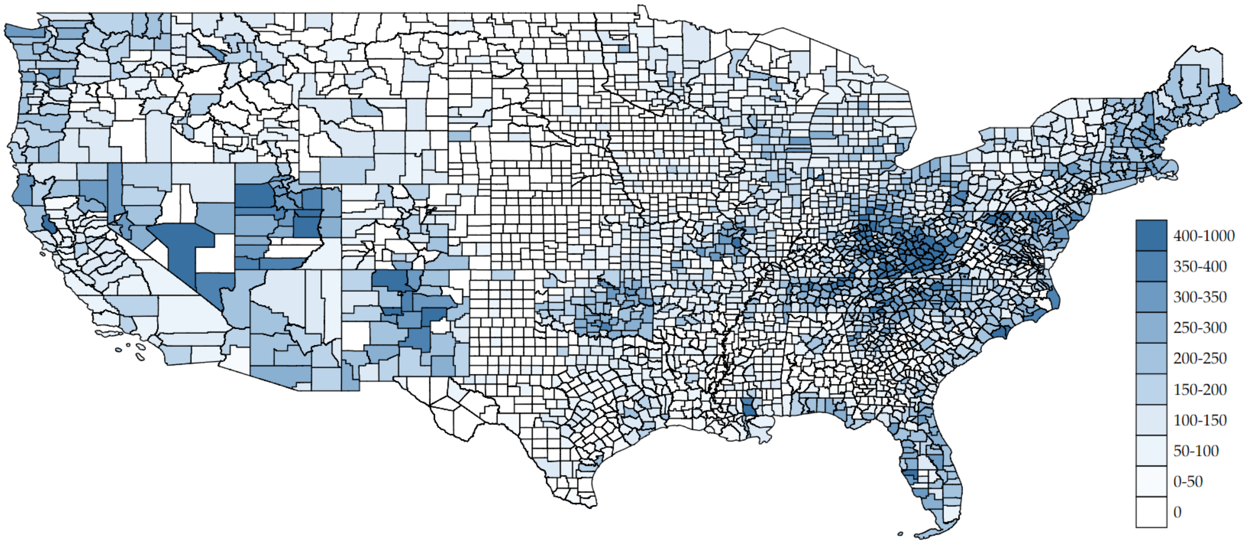

FIGURE 2—GEOGRAPHIC DISTRIBUTION OF TOTAL OPIOID OVERDOSE DEATHS BY COUNTY

Notes: The figure illustrates the geographical distribution of the total number of opioid overdose deaths per 100,000 residents between 1999 and 2019. The data is obtained from the National Vital Statistics System (NVSS). Total opioid overdose deaths are identified using ICD-10 underlying cause of death codes X40-X44, X60-X64,X85, and Y10-Y14 together with the drug identification codes T40.1-T40.4.

areas have seen higher overdose death rates. The map documents substantial geographic heterogeneity in the distribution of opioid overdose deaths. Opioid overdose deaths seem to cluster in Rust Belt counties and communities of the North East. In those regions, up to 1,000 opioid overdose deaths per 100,000 residents have been recorded between 1999 and 2019, indicating that approximately 1% of the population has died from opioid overdoses. However, we also observe high death rates in counties in other regions. We show that friendship networks are useful for understanding the dynamics of the spatial spread of the epidemic.

2.3 Further Covariates and Summary Statistics

We gather data on additional control variables from multiple sources. Total population counts and population counts per age as well as racial groups come from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program. These demographic variables are provided at the county-level with an annual frequency. In order to proxy for regional economic conditions, we obtain yearly county unemployment rates (Bureau of Labor Statistics) and real per capita income (Bureau of Economic Analysis). County-pair physical distances are gathered from the NBER county distance database. Additional socio-demographic variables are obtained from the Atlas of Rural and Small-Town America provided through the Department of Agriculture.

Merging all different data sources results in our final dataset which contains 65,289 county-year observations from 1999 to 2019. Overall, we cover 3,109 out of 3,243 U.S. counties. Table 1 reports pooled summary statistics for our final dataset. The average county in our sample has roughly 100,000 res-

TABLE 1—POOLED SUMMARY STATISTICS

	Mean	SD	Q25	Median	Q75	Min	Max
Overdose Deaths							
Total Opioids (T40.1-T40.4)	5.09	8.41	0.00	1.40	7.23	0.00	139.75
Prescription Opioids (T40.2+T40.3)	3.04	5.70	0.00	0.00	4.28	0.00	115.09
Heroin (T40.1)	0.72	2.41	0.00	0.00	0.00	0.00	58.45
Heroin + Fentanyl (T40.1+T40.4)	2.05	5.39	0.00	0.00	1.71	0.00	133.40
General Connectedness							
Share of Friends within 50 miles (%)	53.15	13.12	44.65	55.20	63.49	0.96	79.40
Share of Friends within 100 miles (%)	63.71	12.29	56.04	66.61	73.13	5.85	85.58
Share of Friends within 200 miles (%)	73.12	10.70	67.89	76.33	80.75	11.22	90.77
log(SCI)	10.27	1.02	9.61	10.29	10.95	7.30	13.43
Total Population	0.98	3.14	0.11	0.26	0.66	0.00	101.06
Race (%)							
Non-Hispanic White	79.79	19.45	68.95	87.33	95.05	2.09	100.00
Non-Hispanic Black	9.33	14.59	0.77	2.39	10.75	0.01	86.73
Population (%)							
0-14	19.34	2.95	17.54	19.26	20.95	3.30	37.73
15-24	13.22	3.48	11.47	12.56	13.90	1.30	50.29
45-64	26.48	3.29	24.45	26.65	28.52	5.21	46.38
65-84	14.28	3.88	11.68	13.92	16.47	1.63	53.69
85+	2.13	0.87	1.54	1.97	2.54	0.05	8.80
Economic Conditions							
Per Capita Income	33.70	11.62	25.82	31.68	39.12	9.23	230.14
Unemployment Rate (%)	5.93	2.68	4.00	5.30	7.20	0.70	30.30

Notes: The table reports pooled county-level summary statistics for the years 1999 to 2019. All death variables are normalized by county population and expressed per 100,000 residents. Per capita income is expressed in thousands of dollars, while total population is measured in 10^6.

idents consisting of 79.77 percent non-Hispanic Whites and 9.33 percent non-Hispanic Blacks. While friendship networks seem to be geographically concentrated, the average share of friends further away than 50 (100) miles is still 37 (26) percent. These more distant links allow us to distinguish social proximity from mere physical proximity in our later analysis. The average per capita income is 33,700 USD. County unemployment rates vary between 0.7 and 30.3 percent and are on average 5.93 percent. The average total opioid overdose death rate (T40.1-T40.4) is 5.09 per 100,000 residents, but counties differ significantly in their exposure to the opioid epidemic as illustrated by the standard deviation of 8.41 and the maximum value of 139.75 deaths per 100,000 residents. The other death categories like prescription opioid overdoses (T40.2+T40.3), heroin overdoses (T40.1), and combined heroin and fentanyl overdoses (T40.1+T40.4) show a similar pattern.

3 Effects of Social Proximity to the Epidemic on Overdose Deaths

In this section we study how *Social Proximity* to the opioid epidemic correlates with overdose deaths. Firstly, we document descriptive evidence that counties with more social ties to areas severely affected by the opioid epidemic have higher overdose deaths in the following year. Secondly, we explicitly quantify how much of the within-county variation in overdose deaths is explained by *Social Proximity* to the opioid epidemic.

We begin by presenting descriptive evidence showing that the SCI can help to predict the geographic spread of the opioid epidemic. To this end, we construct two different time-varying measures of proximity to the opioid epidemic. *Social Proximity* measures a county's exposure to the opioid epidemic through friendship networks, while *Physical Proximity* allows us to control for county-level exposure through physical proximity. Both measures are necessarily correlated because friendship networks are geographically concentrated (see Table 1). However, Kuchler et al. (2021b) showcase that geographically distant places like, e.g., Westchester and the east coast of Florida can nevertheless have strong social connections. We exploit such variation in social connections.

Our main variable of interest, *Social Proximity* to the opioid epidemic, is defined as

$$Social\ Proximity_{it} = \sum_{j \neq i} Deaths_{jt} \times \frac{SCI_{ij}}{\sum_h SCI_{ih}}, \tag{2}$$

where $Deaths_{jt}$ denotes overdose deaths per 100,000 residents in county j and year t.[4] The sum j runs over all counties except the focal county i. Intuitively, $Social\ Proximity_{it}$ measures the friendship network-weighted overdose death rate in county i and year t. Correspondingly, *Physical Proximity* to

[4] We study four different measures of overdose deaths: (i) total opioid overdoses (T40.1-T40.4), (ii) prescription opioid overdoses (T40.2+T40.3), (iii) heroin overdoses (T40.1), and (iv) combined heroin and fentanyl overdoses (T40.1+T40.4). Therefore, when constructing $Social\ Proximity_{it}$ and $Physical\ Proximity_{it}$ we also use these different overdose measures to proxy for $Deaths_{j,t}$ in Equations (2) and (3).

the opioid epidemic is constructed as

$$Physical\ Proximity_{it} = \sum_{j \neq i} Deaths_{jt} \times \frac{1}{1 + Distance_{ij}}, \tag{3}$$

where $Distance_{ij}$ is the physical distance between counties i and j. Thus, counties that are located nearby the focal county i are assigned a higher weight in the construction of *Physical Proximity*. Both proximity measures vary over time due to the dynamic nature of the county-level death rates.

We first study how social and physical proximity dynamically affect overdose death rates. Therefore, we estimate the following regression model

$$\begin{aligned} y_{it} = &\beta_1 \times Social\ Proximity_{i,t-1} + \beta_2 \times Physical\ Proximity_{i,t-1} \\ &+ \mathbf{X}_{it} \times \boldsymbol{\delta} + \boldsymbol{\Phi}_{it} + \varepsilon_{it}, \end{aligned} \tag{4}$$

where y_{it} are different overdose death rates, $Social\ Proximity_{i,t-1}$ and $Physical\ Proximity_{i,t-1}$ are lagged measures of social and physical proxmity to the opioid epidemic, respectively, $\mathbf{X}_{it}$ is a matrix of control variables, and $\boldsymbol{\Phi}_{it}$ contains different sets of location and time fixed effects. Our main results are population-weighted using county population in 1999, but the results are not sensitive to this choice. Standard errors are clustered at the state level.

Table 2 documents our first set of results. The dependent variable measures total opioid overdose deaths per 100,000 residents (T40.1-T40.4). We standardize *Social Proximity* and *Physical Proximity* before running the regressions. Thus, the coefficients can be interpreted as the effect of a one standard deviation change in the respective proximity measure. Specification (1) shows that when we only include $Social\ Proximity_{i,t-1}$ and $Physical\ Proximity_{i,t-1}$ as regressors, without controlling for any fixed effect, the estimated coefficient on $Social\ Proximity_{i,t-1}$ is 4.48. This implies that a one standard deviation increase in *Social Proximity* to the opioid epidemic in year $t-1$ is associated with 4.48 more opioid overdoses per 100,000 residents in the following year. Given that the sample mean of total opioid overdoses is 7.22, the effect size is clearly economically meaningful. Moreover, the estimated coefficient is also statistically significant at the one percent level. Notably, the coefficient on *Physical Proximity* is smaller in absolute terms and only marginally significant.[5]

In specification (2) we add general measures of social connectedness like the share of friends within 100 miles and the log of the average SCI per county. The coefficients on *Social Proximity* and *Physical Proximity* do not change notably. General network concentration seems to correlate positively with opioid

[5] For total opioid overdose deaths the correlation between *Social Proximity* and *Physical Proximity* is 0.76.

TABLE 2—SPATIAL SPREAD OF OPIOID EPIDEMIC

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proximity								
Social Proximity$_{t-1}$	4.481***	5.675***	5.758***	5.799***	4.950***	4.686***	5.317***	4.305***
	(0.730)	(0.720)	(0.603)	(0.554)	(0.425)	(0.452)	(0.451)	(0.473)
Physical Proximity$_{t-1}$	1.458*	0.067	-0.082	0.508	2.536***	0.838	2.245***	1.210
	(0.777)	(0.733)	(0.606)	(0.580)	(0.708)	(1.352)	(0.737)	(1.906)
General Connectedness								
Friends within 100mi		0.055**	0.032	-0.003	0.037	0.038		
		(0.024)	(0.022)	(0.022)	(0.023)	(0.025)		
log(SCI)		1.165***	0.425	-0.025	-0.472	-0.642		
		(0.230)	(0.322)	(0.317)	(0.377)	(0.399)		
Race								
Non-Hispanic White			0.066***	0.062***	0.083**	0.092***	0.393***	0.328**
			(0.021)	(0.021)	(0.033)	(0.032)	(0.094)	(0.148)
Non-Hispanic Black			0.057**	0.052**	0.108**	0.106**	0.365**	0.278*
			(0.025)	(0.024)	(0.041)	(0.040)	(0.149)	(0.160)
Age Group								
15-24			-0.071	-0.114*	-0.040	-0.044	-0.038	-0.239
			(0.059)	(0.065)	(0.066)	(0.063)	(0.261)	(0.221)
45-64			-0.214**	-0.103	-0.000	0.003	-0.022	0.000
			(0.102)	(0.089)	(0.107)	(0.110)	(0.239)	(0.229)
65-84			0.206**	0.034	0.009	-0.033	-0.337*	-0.408*
			(0.083)	(0.087)	(0.077)	(0.087)	(0.190)	(0.233)
85+			-0.353	0.459	0.570	0.601	-0.482	-0.375
			(0.397)	(0.444)	(0.407)	(0.429)	(0.792)	(0.645)
Economic Conditions								
log(Per Capita Income)				-3.513***	-2.685***	-2.310***	-3.962	-3.497
				(0.707)	(0.847)	(0.831)	(2.493)	(2.161)
Unemployment Rate				-0.060	0.210**	0.361***	-0.091	-0.095
				(0.056)	(0.090)	(0.126)	(0.089)	(0.139)
Dependent Mean	7.22	7.22	7.22	7.24	7.24	7.23	7.24	7.23
State FE	No	No	No	No	Yes	No	No	No
Year FE	No	No	No	No	Yes	No	Yes	No
State × Year FE	No	No	No	No	No	Yes	No	Yes
County FE	No	No	No	No	No	No	Yes	Yes
N	62020	62020	61771	60617	60617	60597	60617	60597
R^2	0.508	0.522	0.537	0.551	0.593	0.634	0.730	0.770

Notes: The table shows results from panel regressions. The dependent variable is the total opioid overdose death rate per 100,000 residents (T40.1-T40.4). Social Proximity measures a county's network exposure to the opioid epidemic. Physical Proximity captures a county's distance-weighted exposure to the opioid epidemic. Friends within 100mi is the share of friends within 100 miles. Log(SCI) is the log of a county's average SCI. Non-Hispanic White and Non-Hispanic Black denote the percentage of people in the respective race group. The different age groups measure the percentage of people in the respective age bracket. log(Per Capita Income) is the log of the average per capita income in a county. Unemployment Rate is measured in percent and captures employment conditions. All regressions are population-weighted and include fixed effects as indicated. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

overdose deaths as a ten percentage point increase in the share of friends within 100 miles is associated with an increase in opioid overdose deaths by 0.55 per 100,000 residents. Furthermore, a ten percent increase in the average level of connectedness is related to an increase in opioid overdose deaths by 0.12 per 100,000 residents. In columns (3) and (4) we further add demographic as well as economic control variables. The effect of *Social Proximity* remains statistically significant at the one percent level and becomes even larger in magnitude, while *Physical Proximity* does not seem to exert any significant impact on total opioid overdose deaths. In column (4), a ten percent increase in per capita income results in a statistically significant decrease in opioid overdose deaths by 0.35 per 100,000 residents.

In columns (5) to (8) we saturate the model with various fixed effects to control for unobserved (time-varying) state, county, and year-specific effects. The estimated coefficients on *Social Proximity* remain statistically significant at the one percent level but slightly decrease in magnitude when imposing more conservative model specifications. For example, including state-by-year as well as county fixed effects in specification (8) decreases the coefficient estimate on *Social Proximity* to 4.31.[6] Nevertheless, we argue that this effect size is still economically large as it represents some 60 percent of the mean opioid overdose death rate. *Physical Proximity* now has a positive impact on opioid overdose deaths, but the coefficient is imprecisely estimated. The effect size of *Social Proximity* remains of much larger magnitude than the effect size of *Physical Proximity*. Furthermore, economic conditions seem to have a moderate impact once we control for year-specific state and county characteristics: both per capita income and unemployment rates display an insignificant relation with total opioid overdose deaths. Moreover, across all specifications the coefficients of the variables capturing demographic and economic conditions frequently switch signs and are rather small in terms of economic magnitude. Overall, these results show that *Social Proximity* is strongly associated with total opioid overdose death rates as the estimated coefficients are statistically significant and substantial in magnitude whether or not we control for demographic factors, economic conditions, or unobserved heterogeneity at the state, state-by-year, or county-level.

To further explore the link between *Social Proximity* and the spatial spread of the opioid epidemic, we estimate specification (8) of Table 2 using different measures of overdose deaths. Table 3 documents the results. For the sake of clarity, we only report coefficient estimates on *Social Proximity* and *Physical Proximity*. All specifications include state-by-year and county fixed effects, effectively making this a comparison of within-county variation in the same state and year. Panel A reports results based on all friendship links. Irrespective of the overdose death measure used in the regressions, *Social Proximity* is

[6] State-by-year and county fixed effects imply that the coefficients are identified from within-county variation in the same state and year, thereby controlling for a plethora of confounders like time-constant county characteristics or differential state laws.

TABLE 3—SPATIAL SPREAD OF OPIOID EPIDEMIC

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
Social Proximity$_{t-1}$	4.305***	1.262***	1.551***	3.896***
	(0.473)	(0.184)	(0.165)	(0.418)
Physical Proximity$_{t-1}$	1.210	1.253**	0.095	0.311
	(1.906)	(0.558)	(0.725)	(1.837)
Dependent Mean	7.23	3.85	2.02	4.04
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	60597	60597	60597	60597
R^2	0.094	0.029	0.158	0.158
Panel B: Distance > 50 miles				
Social Proximity$_{t-1}$	1.440***	0.119	0.555***	1.297***
	(0.464)	(0.150)	(0.154)	(0.366)
Physical Proximity$_{t-1}$	4.568**	1.952***	1.865	4.396**
	(1.752)	(0.494)	(1.203)	(2.116)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	60597	60597	60597	60597
R^2	0.031	0.007	0.052	0.055
Panel C: Distance > 100 miles				
Social Proximity$_{t-1}$	1.184**	-0.134	0.463***	1.050***
	(0.447)	(0.166)	(0.150)	(0.325)
Physical Proximity$_{t-1}$	3.397	1.154*	1.130	2.539
	(2.128)	(0.589)	(1.445)	(2.665)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	60597	60597	60597	60597
R^2	0.023	0.003	0.039	0.040

Notes: The table shows results from fixed effects panel regressions. Social Proximity measures a county's network exposure to the opioid epidemic. Physical Proximity captures a county's distance-weighted exposure to the opioid epidemic. Panels A, B, and C implement different restrictions on the physical proximity of county pairs when computing the social proximity measure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. All regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

always statistically significant at the one percent level. Moreover, the coefficients are also economically meaningful. For example, column (4) shows that if *Social Proximity* to the opioid epidemic increases by a standard deviation in year $t-1$, combined heroin and fentanyl overdoses increase by 3.9 per 100,000 residents in year t. The effect size is quantitatively large when compared to the unconditional sample mean of 4.04 combined heroin and fentanyl overdoses per 100,000 residents. A similar picture is documented in column (3) for heroin overdose deaths. Regarding *Physical Proximity* the picture is more diverse across the four specifications. When considering prescription opioid overdoses, *Physical Proximity* seems to exert a significant positive impact. However, the coefficient estimates are insignificant once we focus on total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses, respectively. To show that our results are not driven by outliers in specific years, Figure A.1 in the Online Appendix A documents year-specific effects of *Social Proximity* to the opioid epidemic. Overall, we find that the coefficient estimates are fairly stable over time and statistically significant throughout the sampling period.

Even though we directly control for *Physical Proximity* in our regressions, one might argue that our coefficient estimates on *Social Proximity* partially take up effects of local socio-economic factors that are not included in our analysis but correlated with *Social Proximity*. Hence, we would falsely attribute the effect of such variables to our *Social Proximity* measure, overstating the network effect. In order to mitigate such considerations, Panels B and C of Table 3 report results based on a modified version of the *Social Proximity* measure. In particular, instead of summing over all counties, the summation index j in Equation (2) only considers counties having a physical distance of more than 50 miles (Panel B) or 100 miles (Panel C) to the focal county. The estimated coefficients on *Social Proximity* become smaller in magnitude but remain statistically significant at least at the five percent level throughout all specifications except column (3) and distance restrictions. For example, computing *Social Proximity* based on all county-pairs with distance greater than 100 miles results in a coefficient estimate of 1.05 when considering combined heroin and fentanyl overdose deaths. Hence, if *Social Proximity* to the opioid epidemic, as measured by the network exposure based on geographically distant counties, increases by one standard deviation in year $t-1$, combined heroin and fentanyl overdose deaths are higher by 1.05 per 100,000 residents in year t. The effect is economically meaningful as it represents 26% of the sample average. This interpretation qualitatively transcends to total opioid overdoses as well as heroin overdoses, while the effect vanishes for prescription opioids. Overall, we conclude that *Social Proximity* is strongly associated with various overdose measures in the cross-section and even when controlling for unobserved heterogeneity as well as implementing county-pair distance restrictions. We provide additional information on the predictive power of *Social Proximity* in Table B.1 of the Online Appendix.

4 Quasi-Experimental Variation in Network Exposure to the Epidemic

While our results hitherto are suggestive for a causal impact of social proximity, the baseline regressions are plagued by potential endogeneity concerns. For example, a county's friendship network structure is inherently endogenous as people decide for themselves who they want to be friends with. In line with this notion, a large literature documents that individuals are more likely to be friends with individuals that share similar personal characteristics along dimensions such as race, ethnicity, age, religion, education, occupation, or gender (e.g., Lazarsfeld et al., 1954, McPherson et al., 2001). Hence, counties are likely to be more closely connected with other counties that are on average similar across various socio-economic dimensions. Therefore, our results could be driven by an omitted third factor that determines both the structure of county friendship networks and overdose deaths. Kuchler and Stroebel (2021) highlight two ways to achieve identification of peer group effects: (i) random peer group assignment and (ii) random shocks to an endogenously formed peer group. We follow the latter strategy in our empirical analysis.

In particular, we exploit two policy-induced shocks to illicit drug consumption that are orthogonal to friendship network formation. Firstly, we leverage the differential network exposure of counties to the 2010 OxyContin reformulation. Secondly, we analyze how county-level overdose deaths change when out-of-state must-access PDMPs are implemented. Both the OxyContin reformulation and must-access PDMPs have been shown to effectively reduce prescription opioid misuse, while at the same time causing substantial substitution to illicit opioids such as heroin and fentanyl (e.g., Alpert et al., 2018, Evans et al., 2019, Powell and Pacula, 2021, Kim, 2021). These findings are also supported by the medical literature showing that most illicit drug users started to abuse prescription opioids before switching to even more dangerous alternatives due to their pharmacological similarity (e.g., Cerdá et al., 2015, Compton et al., 2016). We leverage these shocks to illicit opioid consumption in our empirical design to study how opioids in general and heroin and fentanyl in particular spread through friendship networks.

4.1 The 2010 OxyContin Reformulation

OxyContin was introduced by Purdue Pharma in 1996 and is based on the active ingredient oxycodone. In line with clinical recommendations, opioids were mostly used to treat chronic or acute pain of, e.g., terminally ill cancer patients by that time (Max et al., 1995). The main innovation of OxyContin was its extended release formulation which provided 12 hours of continuous pain relief. To achieve effective pain management over such a prolonged time interval, OxyContin usually featured a high dosage of the active ingredient. The extended release formulation made OxyContin especially attractive and easy to use compared to previous drugs. However, the gradual release mechanism can be circumvented by chewing, snorting, or injecting the dissolved pill. Such abusive behavior causes the full dose of oxycodone to be released immediately. For this reason, OxyContin was particularly attractive for abusers. Spurred by an overwhelming marketing campaign in the late 1990s through the early 2000s promoting OxyContin use for non-cancer chronic pain (Alpert et al., 2019), OxyContin became one the best-selling prescription drugs with more than $3 billion dollar sales in 2010 (Bartholow, 2011). This blockbuster success also contributed to a widespread diversion to non-medical use rendering it one of the most frequently abused prescription drugs (Cicero et al., 2005). Unsurprisingly, OxyContin is frequently considered as a key cause of the opioid epidemic (Kolodny et al., 2015).

In August 2010, Purdue Pharma stopped selling the original formulation of OxyContin to points of distribution. Instead, an abuse-deterrent version of OxyContin was introduced which contained physicochemical barriers to make the pill hard to crush or dissolve, thereby effectively preventing the most harmful abuse methods, while still allowing legitimate treatment of severe pain (Alpert et al., 2018). Prior studies report that OxyContin misuse and oxycodone shipments immediately decreased after the reformulation (Alpert et al., 2018, Evans et al., 2019, Powell and Pacula, 2021). These studies also provide evidence that the reformulation-induced supply interruption caused a quantitatively large substitution to heroin, thereby fueling the second wave of the opioid epidemic.

We start by confirming these results using county-level data because all previous studies on the OxyContin reformulation are conducted at the state level. Alpert et al. (2018) note that the direct effect of the reformulation is dependent on pre-2010 OxyContin abuse as the reformulation has more bite in high abuse states compared to low abuse states. We use county-level oxycodone shipments per capita (ARCOS DEA) between 2006 and 2009 as a proxy for misuse to measure the extent to which counties are affected by the reformulation.[7]

To identify substitution patterns at the county-level, we implement the following difference-in-differences design

$$y_{it} = \beta \times Pre2010OxyRate_i \times Post_t + \mathbf{X}_{it} \times \boldsymbol{\delta} + \boldsymbol{\Phi}_{it} + \varepsilon_{it}, \tag{5}$$

where $Pre2010OxyRate_i$ are total county-level oxycodone shipments per capita between 2006 and 2009.

[7] Alpert et al. (2018) conduct their analysis at the state level and can thus employ self-reported OxyContin misuse rates provided through the National Survey on Drug Use and Health (NSDUH) to measure misuse. Unfortunately, this direct measure of OxyContin abuse is not available at the county-level. However, in the Online Appendix C we show that our oxycodone shipment-based measure is a good proxy for misuse.

y_{it} can be opioid prescriptions or different measures of opioid overdose death rates. $Post_t$ is a dummy variable which equals one in the post reformulation period. Moreover, $\mathbf{X}_{it}$ is a matrix of socio-economic control variables. To control for time-constant county characteristics and time-varying unobserved effects at the state level, $\mathbf{\Phi}_{it}$ contains county as well as state-by-year fixed effects. Our main results are population-weighted using county population in 1999, but the results are not sensitive to this choice. Table 4 shows the direct effect of the OxyContin reformulation. $Pre2010OxyRate_i$ is standardized

TABLE 4—OXYCONTIN REFORMULATION – DIRECT EFFECT

	Opioid Presc.	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Presc. Opioids Only	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)	(5)	(6)
Pre2010OxyRate × Post	-0.030***	0.759*	0.031	-0.157	0.409**	0.916**
	(0.007)	(0.442)	(0.178)	(0.094)	(0.187)	(0.389)
Dependent Mean	0.74	8.65	4.44	3.61	2.47	5.04
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	35900	44065	44065	44065	44065	44065
R^2	0.955	0.762	0.680	0.627	0.756	0.780

Notes: The table shows results from difference-in-differences panel regressions. Pre2010OxyRate is the county-level Oxycodone prescription rate between 2006 and 2009. Post is an indicator variable for the post-reformulation period starting in 2010. Opioid prescriptions per capita is the dependent variable in specification (1), specification (2) incorporates total opioid overdose deaths (T40.1-T40.4), specification (3) considers prescription opioid overdose deaths (T40.2+T40.3), specification (4) uses prescription opioid overdose deaths without cases where heroin or fentanyl are involved (T40.2+T40.3 but not T40.1+T40.4), specification (5) focuses on heroin overdose deaths (T40.1), and specification (6) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The overdose measures are expressed per 100,000 residents. All regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

such that the coefficients can be interpreted as the effect of a one standard deviation increase in the total county-level oxycodone shipments between 2006 and 2009. To explicitly carve out the substitution patterns after the 2010 OxyContin reformulation we study six different outcome variables.

Column (1) shows that counties in which the OxyContin reformulation has more bite experienced larger decreases in opioid prescriptions.[8] In particular, a one standard deviation increase in pre-2010 oxycodone shipments leads to 0.03 fewer opioid prescriptions per capita on an annual basis in the post reformulation period. This effect corresponds to a decrease of 4% relative to the sample average of opioid prescriptions, i.e., the regulation was effective in the sense of leading to fewer prescriptions in counties with indications of higher abuse rates. Columns (2) to (6) analyze the effect of the reformulation on various opioid overdose measures to isolate substitution patterns. Total opioid overdoses, heroin over-

[8]We measure opioid prescriptions using publicly available prescription data from the CDC. One caveat of this data source is that it does not capture dosage strength since it simply reports the per capita opioid prescription in a given county and year.

doses as well as combined heroin and fentanyl overdoses increase significantly after the reformulation. As indicated by column (3), prescription opioid overdoses, however, only display insignificant effects. Our death categories only indicate any presence of specific drugs but do not allow an exact attribution of a death to a single drug when multiple substances are mentioned on the death certificate. Hence, the absence of a reduction in prescription opioid overdose deaths might be explained by taking into account that between 2011 and 2013 more than 45% of heroin users were also addicted to opioid pain relievers (Jones et al., 2015). To support this view, column (4) presents results where we use deaths attributed to prescription opioids but not heroin or fentanyl as the outcome variable. This adjusted measure of prescription opioid overdoses displays a negative effect that is marginally insignificant, but the effect size of 4% of the sample average is consistent with the reduction in opioid prescriptions.

Column (5) shows that the effect size for heroin overdoses is 0.41 implying that a one standard deviation increase in pre-2010 oxycodone shipments translates into 0.41 more annual heroin overdoses in the post reformulation period. The economic magnitude of this effect is large as it corresponds to roughly 16.6% of the sample average of heroin overdoses. For combined heroin and fentanyl overdoses the coefficient estimate is 0.92, further indicating a strong substitution of prescription opioids with heroin and fentanyl. Our results are consistent with previous studies analyzing the OxyContin reformulation at the state level (e.g., Alpert et al., 2018, Evans et al., 2019, Powell and Pacula, 2021).

Having established significant substitution effects caused by the reformulation, we now leverage this supply policy induced shock to illicit drugs to study whether drug usage spreads through friendship networks. We measure friendship network exposure to the OxyContin reformulation as

$$OxyNetExposure_i = \sum_{j \neq i} \mathbb{1}(Pre2010OxyRate_j > Med) \times \frac{SCI_{ij}}{\sum_h SCI_{ih}}, \tag{6}$$

where $\mathbb{1}(Pre2010OxyRate_j > Med)$ captures whether county j has above-median oxycodone shipments between 2006 and 2009. The sum j runs over all counties except the focal county i. Intuitively, $OxyNetExposure_i$ measures the share of a county's friendship network that is exposed to above-median oxycodone counties. Hence, it also captures the degree to which counties are confronted with a shock to illicit drug consumption through their friendship networks. Using the network exposure to the OxyContin reformulation, we test if the shock to illicit drug consumption is transmitted through friendship networks. Therefore, we implement the following difference-in-difference specification

$$y_{it} = \beta_1 \times OxyNetExposure_i \times Post_t + \beta_2 \times Pre2010OxyRate_i \times Post_t + \mathbf{X}_{it} \times \boldsymbol{\delta} + \boldsymbol{\Phi}_{it} + \varepsilon_{it}, \tag{7}$$

where y_{it} are various overdose measures, $Pre2010OxyRate_i \times Post_t$ controls for the direct effect of the reformulation, and $\mathbf{X}_{it}$ is a matrix of control variables. Our main variable of interest is the interaction term $OxyNetExposure_i \times Post_t$ which allows separating the network effect from the direct effect of the OxyContin reformulation. Consequently, β_1 captures how counties having many connections to regions where the OxyContin reformulation has more bite differ in terms of overdose measures from regions with fewer connections to such regions after the reformulation.

Our empirical model contains county fixed effects to control for time-invariant county characteristics. State-by-year fixed effects control for time-varying heterogeneity, e.g., changes in legislation or law enforcement at the state level, effectively making this a comparison between counties in the same state and year. As such the identification comes from within-state differences in county network exposure to the OxyContin reformulation, holding factors like legislation, law enforcement, socio-economic characteristics, unobserved time-constant county heterogeneity, and the direct effect of the reformulation constant.

The identification assumption is that net of all control variables and fixed effects, counties with more friendship links to regions where the OxyContin reformulation has more bite would have behaved like other counties in the same state that had fewer friendship links to such regions if the OxyContin reformulation had not taken place. As always the identification assumption cannot be directly verified, but we provide evidence that pre-trends did not differ significantly between counties with high and low OxyContin friendship network exposure. To provide further information, Table D.1 in the Online Appendix D presents detailed summary statistics conditional on below and above median network exposure to the OxyContin reformulation.

In Table 5, we report the results of estimating Equation (7) to test if network exposure to the OxyContin reformulation contributes to changes in overdose deaths over and above the direct effect of the reformulation. The different columns in the table refer to different opioid overdose measures. Panel A presents the results when computing the network exposure to the OxyContin reformulation based on all friendship links. A one standard deviation increase in the network exposure to the OxyContin reformulation is associated with an increase in total opioid overdose deaths by 1.02 deaths per 100,000 residents, holding the direct effect of the reformulation constant and controlling for unobserved county as well as state-by-year heterogeneity. The effect is statistically significant at the 1% level and also meaningful in economic terms as it corresponds to some 12% of the sample average. In column (3) and (4) we present our findings for heroin overdose deaths and combined heroin and fentanyl overdose deaths. The estimates show that a one standard deviation increase in friendship network exposure is related, c.p., to an

TABLE 5—OXYCONTIN REFORMULATION – INDIRECT FRIENDSHIP NETWORK EFFECT

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	1.020***	-0.012	0.803***	1.311***
	(0.294)	(0.117)	(0.160)	(0.269)
Pre2010OxyRate × Post	0.627	0.031	0.301	0.743*
	(0.453)	(0.189)	(0.181)	(0.385)
Dependent Mean	8.65	4.44	2.47	5.04
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.759	0.782
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.568**	-0.103	0.596***	0.823***
	(0.268)	(0.093)	(0.138)	(0.227)
Pre2010OxyRate × Post	0.714	0.036	0.361*	0.850**
	(0.472)	(0.181)	(0.203)	(0.415)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.760	0.782
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.516*	-0.110	0.543***	0.755***
	(0.268)	(0.082)	(0.138)	(0.231)
Pre2010OxyRate × Post	0.718	0.036	0.365*	0.855**
	(0.475)	(0.179)	(0.206)	(0.420)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.760	0.782

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panels A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

increase in heroin overdose deaths (combined heroin and fentanyl overdose deaths) by 0.80 (1.31) per 100,000 residents or as much as 32% (26%) of the sample average. This effect is statistically significant at the one percent level and also economically substantial.

Nevertheless, one might argue that the social network effect picks up at least some part of the direct effect and will become insignificant when focusing on geographically distant friends. We rule out such an alternative explanation by showing that the coefficient on $OxyNetExposure_i \times Post_t$ remains significant when we impose distance restrictions on the county-pairs used to construct $OxyNetExposure_i$. Panels B and C require that county pairs are at least 50 and 100 miles away from each other for being considered when computing $OxyNetExposure_i$. In our preferred specification in Panel C, the coefficient on $OxyNetExposure_i \times Post_t$ remains statistically significant at the one percent level for heroin overdoses and combined heroin and fentanyl overdoses. As expected, the effect size becomes smaller but remains economically substantial as the coefficient estimate for heroin (combined heroin and fentanyl) overdoses implies that a one standard deviation increase in the OxyContin network exposure incorporating geographically distant friends only results in 0.54 (0.76) more annual heroin (combined heroin and fentanyl) overdoses per 100,000 residents after the reformulation. The effect size is still large as it corresponds to 22% (15%) of the sample average. For total opioid overdoses, the coefficient becomes smaller but remains statistically significant at the 10% level, consistent with the direct effect of the reformulation. Panel C also shows that a distance restriction of 100 miles causes the coefficient on $Pre2010OxyRate_i \times Post_t$ to converge almost fully back to the estimates presented in Table 4, suggesting that with our empirical model we can distinguish between the direct effect and the indirect network effect of the reformulation. Finally, the results in Column (3) show insignificant friendship network effects for prescription opioid overdoses, consistent with the direct effect of the reformulation.

Overall, our findings are fully consistent with basic economic reasoning. We show in Table 4 that the 2010 OxyContin reformulation caused a major transition from prescription opioids to illicit drugs like heroin or fentanyl. Therefore, it is reasonable to expect to see friendship network effects for total opioid overdoses, heroin overdoses and combined heroin and fentanyl overdoses since these death categories exactly cover illicit opioids that are used to substitute for OxyContin. Hence, our results show that the indirect network effect operates in the same direction as the direct effect, consistent with behavior passing through friendship networks.

To formally test for differential pre-trends between high and low network exposure counties, we implement the same specification as in Equation (7) but interact $OxyNetExposure_i$ with a full set of year dummies

$$y_{it} = \sum_{\substack{t=2005 \\ t\neq 2010}}^{2019} \beta_{1,t} \times OxyNetExposure_i \times \mathbb{1}(Year = t) \\ + \beta_2 \times Pre2010OxyRate_i \times Post_t + \mathbf{X}_{it} \times \boldsymbol{\delta} + \boldsymbol{\Phi}_{it} + \varepsilon_{it}. \tag{8}$$

The specification still includes $Pre2010OxyRate_i \times Post_t$ to control for the direct effect of the reformulation. Moreover, $\boldsymbol{\Phi}_{it}$ contains county fixed effects as well state-by-year fixed effect to make this a comparison of within-county variation in the same state and year. Standard errors are clustered at the state level. We normalize the coefficient for 2009—the year prior to the reformulation—to zero such that all coefficients are to be interpreted with respect to this baseline. Figure 3 graphically illustrates the estimates of $\beta_{1,t}$ along with 95% confidence bands. As before, we study four different outcome variables: Panels A, B, C, and D show the results for total opioid overdoses, prescription opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses, respectively. Overall, the results clearly show that differential pre-trends between counties with high and low friendship network exposure are of no concern here as the coefficients are insignificant and practically zero before 2010 in all cases. Panel A shows that a one standard deviation increase in the friendship network exposure to the OxyContin reformulation leads to higher opioid overdose rates in the order of up to three deaths per 100,000 residents after the reformulation. Prescription opioid overdoses do not seem to exhibit a network effect as the coefficients in Panel C are insignificant. This result is consistent with the zero aggregate effect found in column (2) of Table 5. Finally, panels C and D confirm that the overall effect is driven by heroin and fentanyl.

4.1.1 Robustness of Results: OxyContin Reformulation

In the following, we provide evidence that our main results for the friendship network effect of the OxyContin reformulation are consistent across various specifications. In particular, we report the results of placebo experiments in Figure F.3 of the Online Appendix. In each placebo experiment, we randomly assign social connections between county pairs. We then reconstruct the OxyContin network exposure variable defined in expression (6), estimate Equation (7), and store the resulting t-statistic of β_1. The solid lines in the figures correspond to the t-statistics using the true social connections as given by the SCI. For total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses, the t-statistic using the true social connections is significantly to the right of the distribution of the placebo

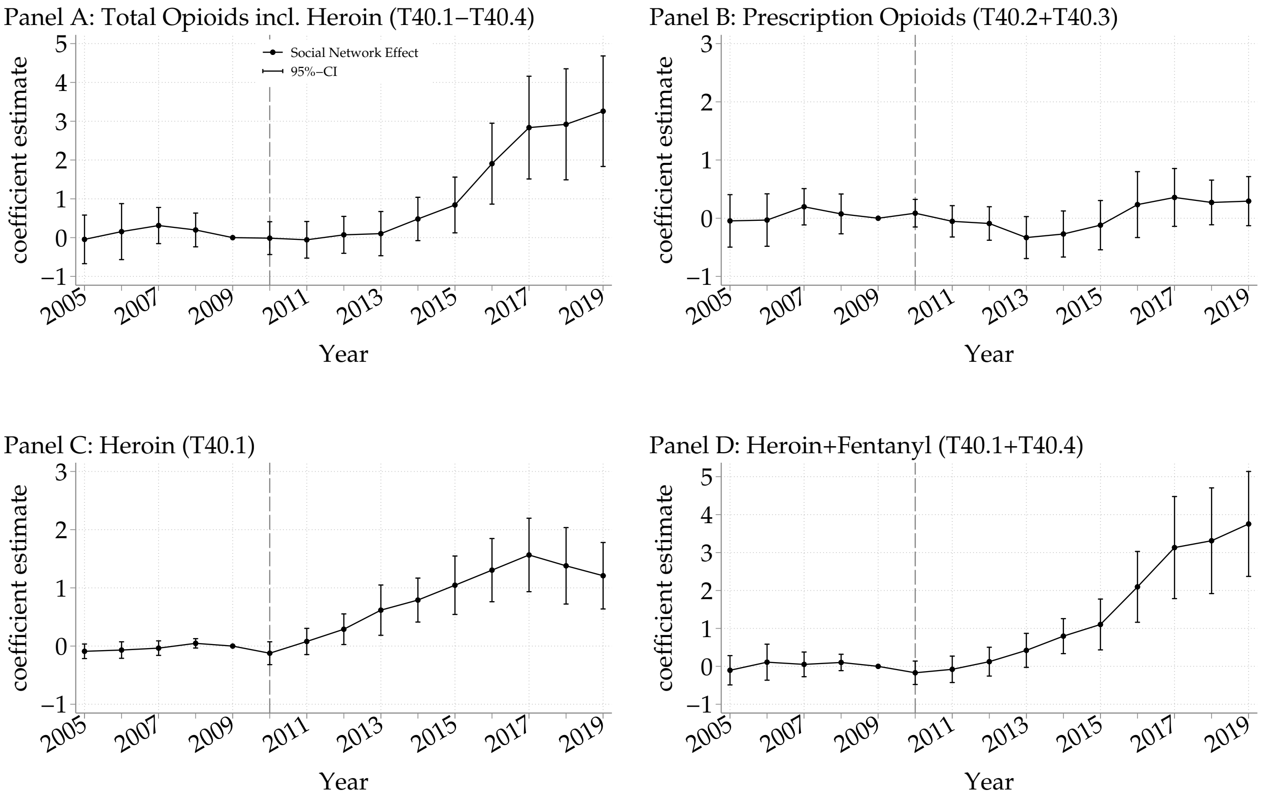

FIGURE 3—EVENT STUDY SPECIFICATION: FRIENDSHIP NETWORK EXPOSURE TO OXYCONTIN REFORMULATION

Notes: The figure illustrates time-specific difference-in-differences coefficients along with 95% confidence bands using different outcome variables. The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid overdose deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. The regressions feature state-by-year and county fixed effects. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted. Standard errors are clustered at the state level.

t-statistic, while for prescription opioid overdoses it lies within the distribution of placebo t-statistics. Overall, these findings are consistent with our baseline results presented in Table 5.

Moreover, we re-estimate the results of Table 5 using different alterations of the research design and report the results in the Online Appendix. Trimming the top 5% of the sample based on the respective dependent variable does not change the interpretation of our results. In fact, Table F.1 provides further evidence in favor of a strong friendship network effect as the direct effect of the reformulation is most of the time insignificant, while the friendship network effect retains its economic and statistical significance. Moreover, Table F.2 shows that clustering standard errors at the county level affects the statistical significance of our estimates only slightly. As described above, our main regressions are population-weighted. When giving equal weight to all observations, all results still go through. Table F.3 documents that the point estimates become even larger when estimating unweighted panel regressions. It might be that our friendship network measure is just a proxy for distance and we are erroneously capturing drug consumption behavior of counties in the same local media or labor market. Our estimates might be biased if the OxyContin reformulation affected local media or labor markets. To counteract such concerns, we control for Designated Market Area (DMA)-by-year fixed effects. Table F.4 shows that our main results remain mostly unaffected. If anything, the friendship network effect becomes even larger. To control for time-varying effects of differences in economic conditions, we estimate a model that includes year-specific fixed effects for percentiles of average county-level household income. Table F.5 documents that our main results are not driven by time-varying effects of income differences.

Another concern might be that our friendship network effect is driven by migration from high oxycodone areas. For example, it could be that a certain county experiences substantial migration from high oxycodone counties after the 2010 OxyContin reformulation. If individuals choose their migration destination based on existing friendship links it might be that our friendship network effect loads on migration patterns without there necessarily being a true friendship network effect. To test for this possibility, we collect data on county-to-county migration flows from the IRS SOI Tax Stats and compute the exposure of counties to the 2010 OxyContin reformulation through migration as

$$OxyMigExposure_{it} = \sum_{j} Pre2010OxyRate_{j} \times Migration_{t}^{ij}, \tag{9}$$

where $Migration_{t}^{ij}$ is the migration rate from county j to the focal county i in year t. The sum j runs over all counties except focal county i. Intuitively, if $OxyMigExposure_{it}$ is high, comparatively many people from high Oxycodone counties migrate to the focal county i in year t. Thus, this measure captures the exposure of county i to the OxyContin reformulation through migration. To control for migration effects we estimate our baseline specification defined in Equation (5) and include an additional term labeled $OxyMigExposure_{it} \times Post_{t}$, where $Post_{t}$ is a dummy variable for the post-reformulation period starting in 2010. The results are presented in Table F.6. Migration exposure to high oxycodone counties seems to have a negligible impact on overdose deaths. Compared with the baseline results in Table 5, the friendship network effect neither looses statistical nor economic significance. To sum up, we conclude that our friendship network effect is not driven by migration patterns.

4.2 Must-Access Prescription Drug Monitoring Programs

In the previous section we control for the direct effect of the OxyContin reformulation and simultaneously study how differential friendship network exposure affects overdose deaths in the post reformulation period. Even though we carefully control for the direct effect and impose distance restrictions, one might argue that our results could be partially driven by the direct effect of the reformulation which is at work at the same time. To rule out such considerations we provide further evidence using a setting where there is arguably no direct effect but—if existent—only an indirect friendship network effect. Our identification approach exploits the introduction of must-access PDMPs, i.e., state level databases that keep track of a patient's prescription history and provide useful information for physicians (Davis et al., 2014). PDMPs are either with or without a must-access provision. In case of a must-access provision, physicians are obliged to consider a patient's prescription record before prescribing controlled substances like opioids. Without a must-access provision, it is in the physician's judgment to consult the PDMP database before prescribing controlled substances. However, unless access to PDMPs is mandated, participation is low with take-up rates around 35% between 2010 and 2012 (Kreiner et al., 2014, Haffajee et al., 2015). Consistent with this observation, previous studies only find weak effects of voluntary PDMPs (e.g., Buchmueller and Carey, 2018). On the contrary, must-access PDMPs are found to effectively reduce opioid prescriptions and prescription opioid misuse, though, at the cost of substantial substitution to illicit drugs like heroin (Buchmueller and Carey, 2018, Mallatt, 2018, Kim, 2021). We concentrate on 16 must-access PDMPs implemented between 2007 and 2015. Figure 4 shows the timeline of must-access PDMP introductions. The introduction dates are obtained from Kim (2021). We proceed

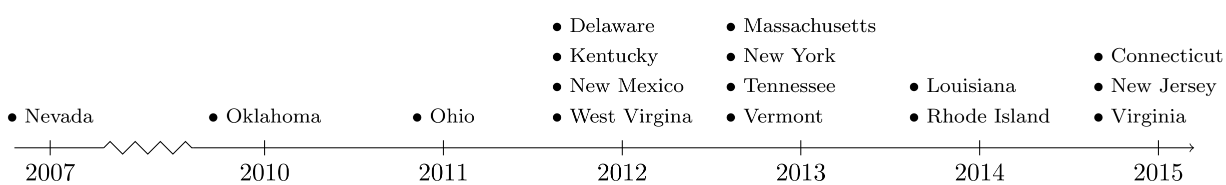

FIGURE 4—TIMELINE OF MUST-ACCESS PDMP INTRODUCTIONS

Notes: The figure illustrates the must-access PDMP introduction dates between 2007 and 2015. The introduction dates are obtained from Kim (2021).

in two steps. First, we show that must-access PDMPs reduce county-level opioid prescriptions, while total opioid overdose deaths increase significantly. This increase is driven by a substantial substitution to illegal substances like heroin and fentanyl. Hence, must-access PDMPs induce a positive shock to illicit drug consumption—particularly for heroin and fentanyl—in counties of implementing states. Second, and more importantly, we exploit this shift towards illicit drug consumption using a difference-in-differences design to study how the opioid epidemic propagates through friendship networks. Specifically, we analyze how counties with many connections to states implementing a must-access PDMP, i.e., to states that experience a positive shock to illicit opioid consumption, differ from counties with fewer such connections. Must-access PDMPs provide an ideal setting to analyze friendship network effects as they are introduced at the state level. Therefore, they only impact counties located in the implementing state, while out-of-state counties are not directly affected.[9] The absence of any direct effect on counties in non-implementing states allows us to cleanly estimate the indirect friendship network effect.

To first investigate the direct impact of must-access PDMPs in counties of implementing states we estimate the following staggered difference-in-differences model

$$y_{it} = \theta \times PDMP_{it} + \mathbf{X}_{it} \times \boldsymbol{\delta} + \phi_i + \gamma_t + \varepsilon_{it}, \tag{10}$$

where $PDMP_{it}$ is a dummy variable that is one when a county has a must-access PDMP provision in year t. We estimate this model using the method proposed by Sun and Abraham (2020). As above, y_{it} are different measures of opioid overdoses. Moreover, $\mathbf{X}_{it}$ is a matrix of socio-economic control variables. Kim (2021) notes that it is crucial to control for the OxyContin reformulation when estimating the impact of must-access PDMPs. Thus, we include $Pre2010OxyRate_i \times Post_t$ in $\mathbf{X}_{it}$ to absorb any confounding effect of the OxyContin reformulation. To control for time-constant county characteristics and time-specific shocks, we include county (ϕ_i) as well as year (γ_t) fixed effects. Note that we cannot control for state-by-year fixed effects because our main variable of interest, $PDMP_{it}$, only varies at the state-year level. Our main results are population-weighted using county population in 1999, but the results are not sensitive to this choice. The identifying assumption in Equation (10) is that overdose measures in counties in must-access PDMP implementing states would have evolved similarly to overdose measures in counties in non-implementing states if the implementing states had not adopted a must-access PDMP.

Table 6 shows the direct effects of must-access PDMP introductions. Column (1) shows that must-access PDMPs indeed reduce opioid prescriptions by 0.038 per capita, or as much as 5% of the average annual prescription rate. At the same time, however, columns (2) to (6) show that overdose deaths do not decline but increase significantly. When evaluated at the average death rate per 100,000 residents, the increase is sharpest for heroin and combined heroin and fentanyl overdoses suggesting that opioid users switch to illicit drugs like heroin once restrictions on legal prescriptions are in place. Despite the decrease

[9]At least theoretically, individuals could travel across state borders to circumvent must-access PDMP legislation in their home state. The analysis presented in Table F.10 of the Online Appendix F excludes counties that border implementing states and shows that our main results are unaffected.

TABLE 6—MUST-ACCESS PDMPS – DIRECT EFFECT

	Opioid Presc.	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Presc. Opioids Only	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)	(5)	(6)
PDMP	-0.038**	5.336***	0.974***	0.163	2.372***	5.172***
	(0.015)	(1.228)	(0.284)	(0.179)	(0.421)	(1.216)
Dependent Mean	0.74	7.80	4.34	3.61	2.18	4.40
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	35913	44080	44080	44080	44080	44080
R^2	0.939	0.620	0.594	0.580	0.623	0.611

Notes: The table shows the direct effect of must-access PDMPs. PDMP is a dummy variable which is equal to one if a state has enacted a must-access PDMP law. Opioid prescriptions per capita is the dependent variable in specification (1), specification (2) incorporates total opioid overdose deaths (T40.1-T40.4), specification (3) considers prescription opioid overdose deaths (T40.2+T40.3), specification (4) uses prescription opioid overdose deaths without cases where heroin or fentanyl are involved (T40.2+T40.3 but not T40.1+T40.4), specification (5) focuses on heroin overdose deaths (T40.1), and specification (6) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The overdose measures are expressed per 100,000 residents. All regressions are population-weighted and include year as well as county fixed effects. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

in opioid prescriptions, column (3) shows that overdoses from prescription opioids increase after a must-access PDMP is introduced. This finding might be explained by the fact that our overdose measures indicate any presence of specific drugs but are not exclusive in the sense that we can attribute the death to a single drug when multiple substances are mentioned on the death certificate. Hence, even though individuals substitute away from prescription opioids as indicated by the decline in opioid prescriptions, we might observe prescription opioid overdoses to increase.[10] Consistent with this reasoning, once we only consider deaths attributable to prescription opioids but not heroin or fentanyl as done in column (4), the positive effect of PDMP on prescription opioid overdoses vanishes almost fully. Nevertheless, our findings overall indicate that must-access PDMPs cause a substantial shift from prescription opioids to illicit drugs.

After showing that must-access PDMPs represent a shock to illicit drug consumption in counties of implementing states, we study whether this shock travels to non-implementing regions through friendship networks. We hypothesize that counties with more social connections to states with must-access PDMPs experience larger increases in opioid overdose deaths than counties with fewer social connec-

[10]For example, it is likely that opioid addicts still continue to get prescriptions from their physicians, but at a lower rate, and substitute the rest by heroin or other drugs obtained illegally on street markets or via the Internet. Moreover, evidence by Jones et al. (2015) indicates that around half of all heroin users are also addicted to prescription opioid pain relievers.

tions to these implementing states because the shock to illicit drug consumption due to must-access PDMPs propagates through friendship networks. To measure changes in must-access PDMP network exposure we define

$$PDMP\ NetExposure_{it} = \sum_{j \neq s}^{S} \mathbb{1}(\text{PDMP in state j})_t \times \frac{SCI_{ij}}{\sum_g^S SCI_{ig}}, \tag{11}$$

where $\mathbb{1}(\text{PDMP in state j})_t$ captures whether state j has a must-access PDMP in year t. The sum j runs over all states except focal county i's own state. Intuitively, $PDMP\ NetExposure_{it}$ measures how strongly a county's friendship network is exposed to states which have recently implemented a must-access PDMP. Hence, it also captures the degree to which counties are confronted with a shock to illicit drug consumption through their friendship networks. Importantly, $PDMP\ NetExposure_{it}$ varies across counties and time as counties are more or less exposed to implementing states through their friendship networks and an increasing number of states implements must-access PDMPs during our sampling period. Using the network exposure to must-access PDMPs, we test whether the shock to illicit drug consumption induced by must-access PDMPs is transmitted through friendship networks. Therefore, we estimate the following regression model

$$y_{it} = \alpha \times PDMP\ NetExposure_{it} + \mathbf{X}_{it} \times \boldsymbol{\delta} + \phi_i + \gamma_{st} + \varepsilon_{it}. \tag{12}$$

Equation (12) contains county (ϕ_i) and state-by-year fixed effects (γ_{st}) to control for any time invariant county characteristic and time-varying unobservables at the state level. Thus, any variation at the state-year level is absorbed, including if a county's own state is implementing a must-access PDMP. As in Equation (10) we include $Pre2010OxyRate_i \times Post_t$ in $\mathbf{X}_{it}$ to control for any confounding effect of the OxyContin reformulation. Our main results are population-weighted using county population in 1999, but the results are not sensitive to this choice. Standard errors are clustered at the state level.

State-by-year fixed effects ensure a comparison between counties in the same state and year. Thus, any effect we document must be driven by within state differences in county network exposure to out-of-state must-access PDMPs, holding factors like legislation, law enforcement, socio-economic characteristics, unobserved time-constant county heterogeneity, and the direct effect of the OxyContin reformulation constant. The identification assumption is that counties with more friendship links to must-access PDMP implementing states would have behaved like other counties in the same state that had fewer friendship links to such states if the out-of-state PDMP had not been introduced. The identification assumption cannot be verified directly, but we later provide evidence that pre-trends did not differ significantly between counties with high and low must-access PDMP network exposure. To provide further information, Table E.1 in the Online Appendix E presents summary statistics for (i) all counties, (ii) counties with an above median change in must-access PDMP network exposure, and (iii) counties with a below median change in must-access network exposure.

Table 7 shows estimation results from model (12) to test whether illicit drug consumption is transmitted through friendship networks. We find that a one standard deviation increase in friendship network

TABLE 7—MUST-ACCESS PDMPS – INDIRECT FRIENDSHIP NETWORK EFFECT

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.525***	0.188*	0.857***	1.592***
	(0.391)	(0.112)	(0.213)	(0.388)
Dependent Mean	8.65	4.44	2.47	5.04
State $\times$ Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.767	0.681	0.765	0.788

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

exposure to out-of-state must-access PDMPs increases total opioid overdoses by 1.53 per 100,000 residents which corresponds to 17.6% of the sample average. The effect is also significant at the one percent level. Prescription opioid overdoses do not seem to exhibit economically significant network effects as the coefficient estimate is only marginally significant but negligible in economic terms. This result reassures trust in our research design since our identification exploits a shock to illicit drug consumption in out-of-state counties that is potentially propagated through friendship networks. Hence, it would be counterintuitive if we found economically significant network effects for prescription opioid overdoses despite our identification aiming at illicit opioids. Finally, column (3) and (4) show statistically and economically significant network effects for heroin overdoses as well as combined heroin and fentanyl overdoses. A one standard deviation increase in network exposures to out-of-state must-access PDMPs is related to an increase in heroin (combined heroin and fentanyl) overdoses by 0.86 (1.59) deaths per 100,000 residents. The effect size corresponds to some 34.8% (31.5%) of the sample average. Moreover, comparing the magnitudes of the coefficients, we conclude that the total effect documented in column (1) is mainly driven by heroin and fentanyl overdoses. In summary, these results show that illicit drug consumption spreads through friendship networks.

The identification assumption in model (12) effectively requires that counties with many connections to states that implement a must-access PDMP would have behaved like other counties in the same state that have fewer such connections had there been no expansion of out-of-state must-access PDMPs. To ensure trust in the research design one could test whether these counties followed similar trends before out-of-state must-access PDMPs were introduced. As states adopt must-access PDMPs at different points in time, pre-trends are more difficult to test than in the canonical difference-in-differences setting. As an alternative, we follow the methodology suggested in Wilson (2020) and treat each out-of-state must-access PDMP introduction as a separate event. Then, we stack the data for each event and estimate the average effect across all out-of-state must-access PDMP introductions as

$$\begin{aligned} y_{ijt} = &\sum_{\substack{t=-5 \\ t\neq 0}}^{8} \alpha_t \times \frac{SCI_{ij}}{\sum_g^S SCI_{ig}} \times \mathbb{1}(t \text{ years from introduction}) \\ &+ \mathbf{X}_{ijt} \times \boldsymbol{\delta} + \phi_{ij} + \gamma_{jst} + \varepsilon_{ijt}. \end{aligned} \tag{13}$$

In this setting, SCI_{ij} is the SCI for county i and the implementing state j. Hence, the fraction $SCI_{ij}/\sum_g^S SCI_{ig}$ captures the importance of state j for the friendship network of county i. The vector $\mathbf{X}_{ijt}$ contains the same controls as in model (12). However, the county fixed effects from (12) are now replaced by event-by-county fixed effects (ϕ_{ij}) and the state-by-year fixed effects by event-by-state-by-year fixed effects (δ_{jst}) to ensure a within-event comparison. Therefore, one can interpret Equation (13) as comparing different overdose measures between counties in the same state that have strong versus weak social connections to the must-access PDMP implementing state j and then taking an average over all events.

We restrict the sampling period to five years before and eight years after each event. The coefficients of interest, α_t, are plotted along with 95% confidence bands in Figure 5 for our four different overdose measures. Panel A (B, C, D) shows total opioid overdoses (prescription opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses). Generally, the coefficients illustrated in Figure 5 are in line with the numerical baseline results provided in Table 7. Across all panels, we do not observe evidence of differential pre-trends as the coefficient estimates are close to zero and statistically insignif-

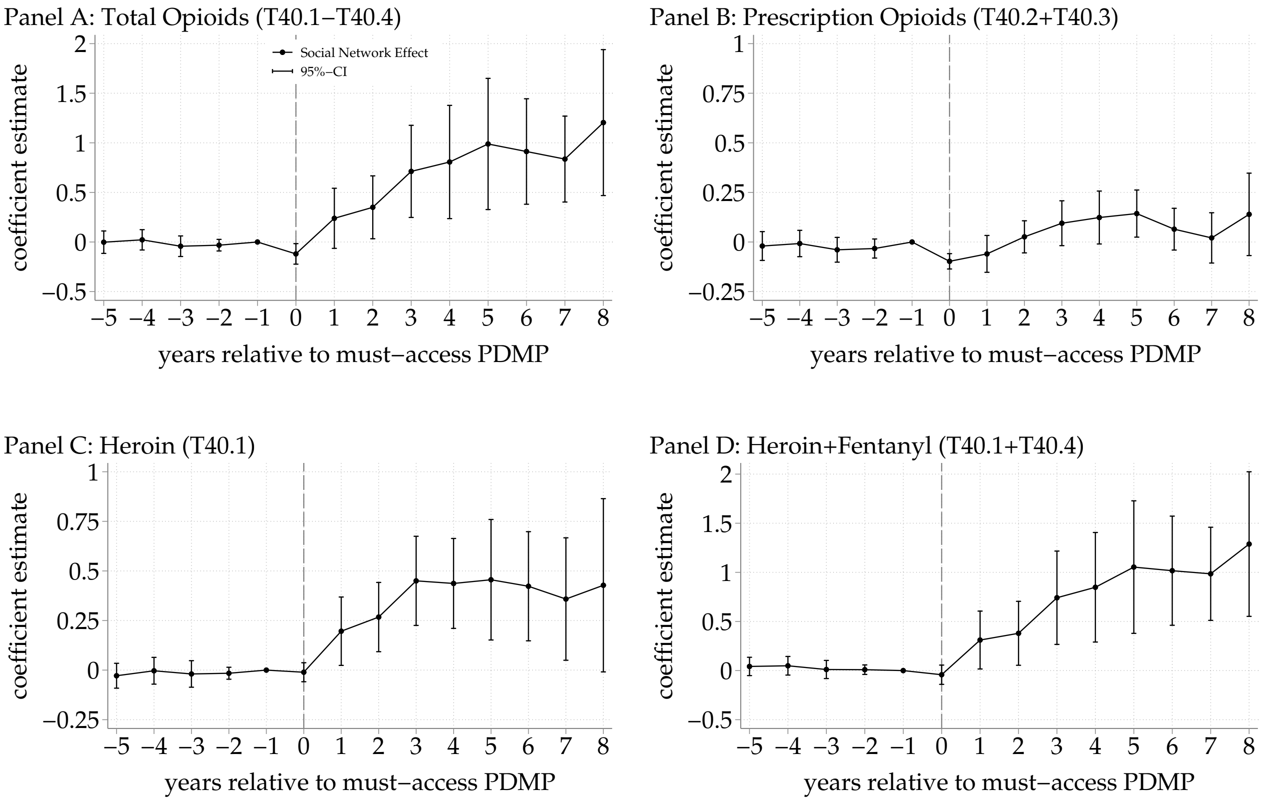

FIGURE 5—EVENT STUDY SPECIFICATION: FRIENDSHIP NETWORK EXPOSURE TO OUT-OF-STATE PDMPS

Notes: The figure illustrates time-specific difference-in-differences coefficients along with 95% confidence bands using different outcome variables. We consider the friendship network exposure to out-of-state must-access PDMP introductions. In our sample we examine 16 staggered adoptions of must-access PDMPs. The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. The regressions include event-by-state-by-year and event-by-county fixed effects. We flexibly control for demographic and local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted. Standard errors are clustered at the state level.

icant. Therefore, we conclude that network exposure to states that introduce must-access PDMPs does not affect overdose measures before the policy is implemented. This observation would reject any view that counties with more connections to must-access PDMP implementing states are systematically different in key characteristics that affect overdose measures over time. When out-of-state must-access PDMPs are introduced, total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses diverge suggesting that shocks to illicit drug consumption as caused by must-access PDMPs are propagated through friendship networks. Hence, this implies that overdose measures in counties with many and few social connection to implementing states were on the same trend and only diverge after out-of-state must-access PDMPs are implemented.

4.2.1 Robustness of Results: Out-of-State PDMPs

In the Online Appendix we present evidence that our main results for the friendship network effects of out-of-state PDMPs are consistent across a wide range of specifications.

In particular, similar to the OxyContin setting, we report the results of placebo experiments in Figure F.4 of the Online Appendix. In each of the 500 placebo experiments, we randomly assign social connections between county pairs. We then reconstruct the out-of-state PDMP network exposure variable defined in expression (11), estimate Equation (12), and store the resulting t-statistic of β_1. The solid lines in the figures correspond to the t-statistics using the true social connections as given by the SCI. For total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses, the t-statistic using the true social connections is significantly to the right of the distribution of placebo t-statistics. In contrast, the t-statistic for prescription opioid overdoses using true social connections is in the middle of the distribution of placebo t-statistics. These findings are consistent with our baseline results in Table 7 and provide confidence in our empirical set-up.

Furthermore, we re-estimate our baseline specification presented in Table 7. Results in Table F.7 show that trimming the top 5% of the sample based on the respective dependent variable does not change the statistical significance of our results. Hence, our results are not solely driven by counties in the upper five percentile for overdose deaths. Moreover, Table F.8 shows that clustering standard errors at the county level does not downgrade the statistical significance of our estimates. As described above, our main regressions are population-weighted. Table F.9 shows that the results become even stronger when giving equal weight to all observations.

We include state-by-year fixed effects in our identification to absorb potential direct effects of policy changes at the state-by-year level, e.g., PDMP introductions. A remaining concern could be that counties located in non-implementing states but adjacent to implementing states see a rise in the quantity of opioids sold because individuals from implementing states cross the border to obtain easy access. This could, at least theoretically, drive our friendship network effect. To exclude this view we discard all counties adjacent to PDMP-implementing states and find our results to hold (see Table F.10). One might also worry that our results are biased because individuals and their friends are exposed to the same set of information through local media. If media consumption influences individuals' decisions, we might wrongly attribute its impact to friendship networks. To alleviate any such concern, we control for DMA-by-year fixed effects. Table F.11 shows that our main results become, if anything, even larger. Table F.12 shows that our friendship network effect of out-of-state PDMPs is not driven by time-varying effects of income differences.

As for the OxyContin reformulation, another concern might be that our friendship network effect is driven by migration patterns as some counties might experience high migration from PDMP implementing states. Correlation between the SCI and migration rates could bias our estimates. To test for this possibility, we compute the exposure of counties to out-of-state PDMPs through migration as

$$PDMP\ MigExposure_{it} = \sum_{j \neq s}^{S} \mathbb{1}(\text{PDMP in state j})_t \times Migration_t^{ij}, \tag{14}$$

where $Migration_t^{ij}$ is the migration rate from state j to the focal county i in year t. The sum j runs over all states except county i's own state. $PDMP\ MigExposure_{it}$ is high, if comparatively many people from PDMP implementing states migrate to the focal county i in year t. To capture migration effects we estimate our baseline specification defined in Equation (12) and include $PDMP\ MigExposure_{it}$ as an additional term. The results are presented in Table F.13. Migration from PDMP-implementing states does not seem to have a strong impact on overdose deaths as the coefficients on the migration term are rather small in magnitude and insignificant in two out of four specifications. The friendship network effect of out-of-state PDMPs becomes even larger in magnitude and fully retains statistical significance. Overall, our friendship network effect does not seem to be driven by migration patterns.

5 Conclusion

Over the last 20 years, the U.S. has experienced a pronounced opioid epidemic but the overall dynamics of this crisis are not well-understood. In this paper, we analyze the role of social connections in shaping the spatial dynamics of the opioid epidemic. We first show in simple panel regressions that counties with more social ties to areas severely affected by the opioid epidemic have higher overdose deaths in the following year. This network effect is robust to controlling for physical proximity, socio-economic conditions, and unobserved heterogeneity.

Moreover, to establish causality we identify spatial shocks to illegal drug consumption and then study how these shocks propragate through friendship networks. Firstly, having closer ties to locations more affected by the 2010 OxyContin reformulation adversely impacts overdose death rates in the post-2010 period. Secondly, having more friends face an out-of-state PDMPs results in higher overdose death rates. Both sets of results are consistent with the direct effects of these interventions, suggesting that information and social behavior spread through social networks.

The importance of friendship networks for the spread of infectious diseases has long been recognized in epidemiology (Keeling and Eames, 2005, Newman, 2002, Mossong et al., 2008, Danon et al., 2011). Very recently, e.g., Kuchler et al. (2021b) show that the spatial spread of Covid-19 correlates with the strength of social ties as proxied by Facebook links. We provide some first important evidence regarding the causal effect of friendship networks on the spread of *non-infectious* diseases.

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Online Appendix to:

"The Social Transmission of Non-Infectious Diseases: Evidence from the Opioid Epidemic"

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A Year-Specific Effect of Social Proximity

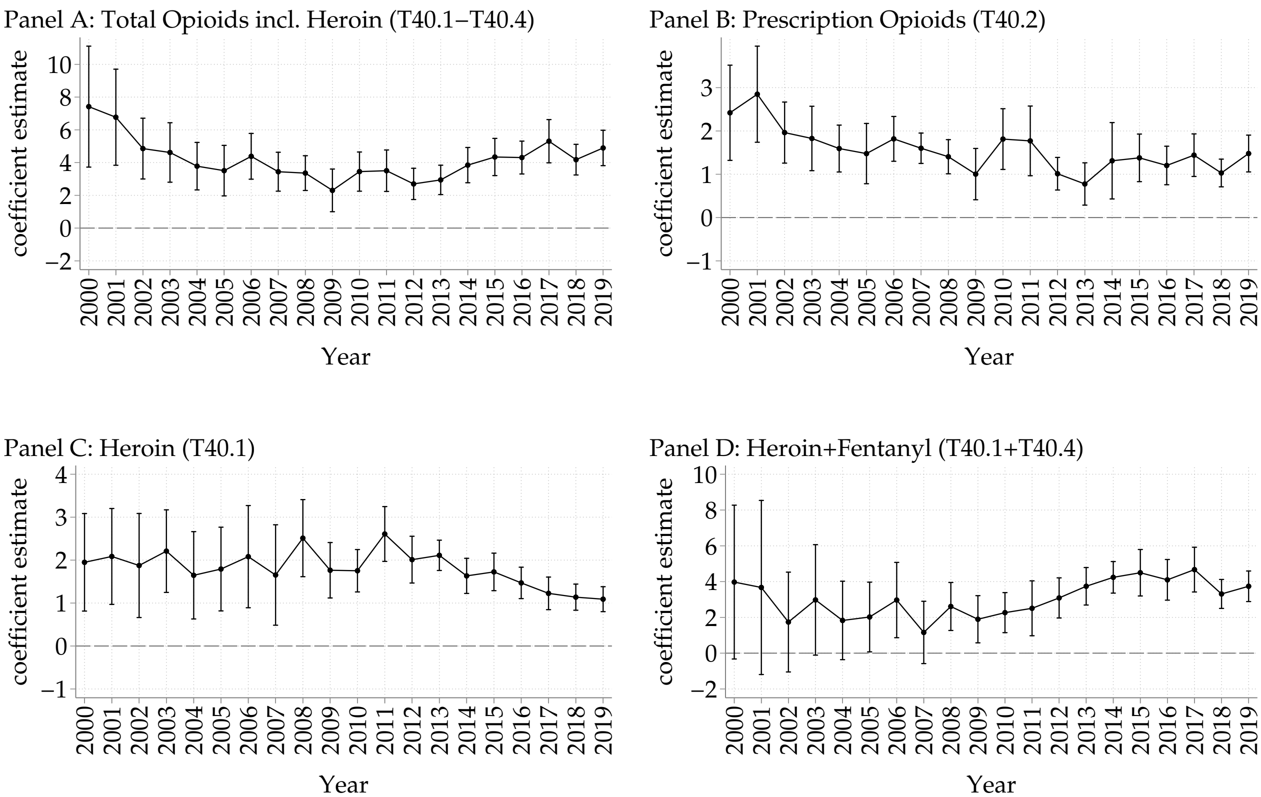

FIGURE A.1—SPATIAL SPREAD OF OPIOID EPIDEMIC - TIME-VARYING HETEROGENEITY

Notes: The figure illustrates coefficient estimates along with 95% confidence bands from interacting social proximity with a full set of year dummies. The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. The regressions include state-by-year and county fixed effects as well as socio-economic control variables. Standard errors are clustered at the state level.

B Incremental within-R^2

In Table B.1 we document the incremental within-R^2 of the regressors employed in our baseline regressions of Table 3. As in the main text, Panels A, B, and C implement different restrictions on the physical proximity of county pairs when computing the social proximity measure. The incremental within-R^2 is reported in percent and defined as the difference in the within-R^2 using the full set of variables and the within-R^2 using the full set of variables except the variable in the respective row. Hence, it measures how much of the within-county variation in overdose deaths is explained by the variable under consideration. Overall, we find that demographic and economic variables do not explain a substantial fraction of the time variation in county-level overdose deaths as the reported incremental within-R^2s are almost always well-below 1%. The same is also true for *Physical Proximity*. However, our results strongly indicate that *Social Proximity* is important for explaining time variation in county-level overdose deaths. The additional within-county variation in overdose deaths explained by *Social Proximity* amounts to up to roughly 9.5%. For example, when computing *Social Proximity* based on the full friendship network, we can explain an additional 9.5% of the time variation in combined heroin and fentanyl overdose deaths when adding *Social Proximity* to a model which already contains measures of *Physical Proximity*, demographic factors, and economic conditions. Imposing distance restrictions when computing the *Social Proximity* measures lowers, as expected, the predictive information contained in *Social Proximity*, but the within-R^2 almost always stays well-above the corresponding measure of the other variables contained in the regression model. Thus, even geographically distant friendship links help explaining time variation in county overdose deaths. Overall, the relationship between *Social Proximity* and overdose deaths is strongest for combined heroin and fentanyl overdoses, while being weaker for prescription opioid overdoses.

TABLE B.1—INCREMENTAL WITHIN-R^2

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
Panel A: All friends				
Social Proximity$_{t-1}$	5.502	1.291	8.838	9.505
Physical Proximity$_{t-1}$	0.048	0.174	0.003	0.005
Non-Hispanic White	0.512	0.245	0.114	0.428
Non-Hispanic Black	0.146	0.075	0.014	0.134
Age 15-24	0.068	0.001	0.006	0.142
Age 45-64	0.000	0.000	0.066	0.048
Age 65-84	0.263	0.063	0.224	0.288
Age 85+	0.008	0.000	0.010	0.021
log(Per Capita Income)	0.298	0.054	0.771	0.472
Unemployment Rate	0.052	0.041	0.055	0.116
Panel B: Distance > 50 miles				
Social Proximity$_{t-1}$	0.847	0.015	2.538	2.622
Physical Proximity$_{t-1}$	2.278	0.421	0.678	0.850
Non-Hispanic White	0.439	0.147	0.028	0.332
Non-Hispanic Black	0.211	0.051	0.017	0.212
Age 15-24	0.058	0.000	0.005	0.138
Age 45-64	0.000	0.000	0.004	0.006
Age 65-84	0.345	0.099	0.388	0.471
Age 85+	0.000	0.005	0.001	0.001
log(Per Capita Income)	0.286	0.027	0.641	0.423
Unemployment Rate	0.034	0.024	0.039	0.092
Panel C: Distance > 100 miles				
Social Proximity$_{t-1}$	0.999	0.022	2.182	2.172
Physical Proximity$_{t-1}$	0.270	0.114	0.213	0.232
Non-Hispanic White	0.353	0.091	0.004	0.261
Non-Hispanic Black	0.178	0.030	0.002	0.189
Age 15-24	0.065	0.000	0.002	0.132
Age 45-64	0.001	0.000	0.005	0.006
Age 65-84	0.351	0.111	0.334	0.468
Age 85+	0.000	0.007	0.002	0.002
log(Per Capita Income)	0.125	0.038	0.473	0.281
Unemployment Rate	0.044	0.016	0.070	0.118

Notes: The table shows the incremental within-R^2 for the variables employed in the regressions of Table 3. Panels A, B, and C implement different restrictions on the distance of county pairs when computing the social proximity measure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All regressions are population-weighted and include state-by-year as well as county fixed effects. The incremental within-R^2 is reported in percent and defined as the difference in the within-R^2 using the full set of variables and the within-R^2 using the full set of variables except the variable in the respective row.

C Direct Exposure to OxyContin Reformulation

Figure C.1 shows that total county-level oxycodone shipments are a good proxy for actual OxyContin misuse. The figure shows binned scatterplots using the state level OxyContin misuse rate between 2004 and 2009 to form 30 equally-sized bins. Dosages per capita refers to the total county-level oxycodone shipments between 2006 and 2009. We compute the mean oxycodone dosage per capita within each bin and illustrate the 30 data points in a scatterplot. Panel A depicts the correlation between oxycodone shipments and state level OxyContin misuse rates in percent. Panel B shows the association between oxycodone shipments and general pain reliever misuse rate in percent. The grey line represents the fit of a linear regression. Overall, the figure confirms a positive association between state level misuse rates and county-level oxycodone shipments, i.e., counties with high oxycodone shipments are located in states with high misuse rates. Hence, we conclude that our oxycodone shipment-based measure is a good proxy for misuse.

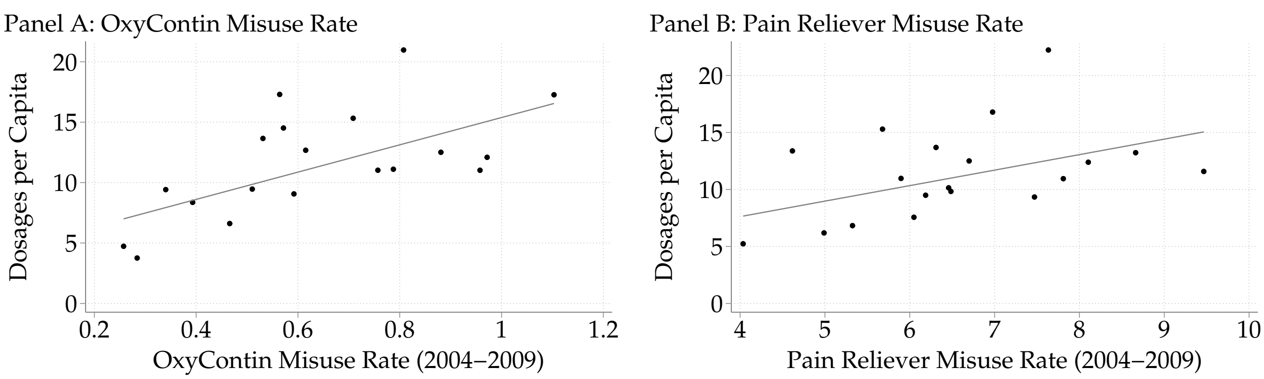

FIGURE C.1—OXYCODONE SHIPMENTS AND OXYCONTIN MISUSE RATES

Notes: The figure shows binned scatterplots using the state level OxyContin misuse rate between 2004 and 2009 to form 30 equally-sized bins. Dosages per Capita refers to the total county-level Oxycodone shipments between 2006 and 2009. We compute the mean of the x-axis and y-axis variable within each bin and illustrate the 30 data points in a scatterplot. Panel A depicts the correlation between Oxycodone shipments and state level OxyContin misuse rates in percent. Panel B shows the association between Oxycodone shipments and general pain reliever misuse rate in percent. The grey line represents the fit of a linear regression.

D Summary Statistics Conditional on OxyContin Network Exposure

TABLE D.1—SUMMARY STATISTICS CONDITIONAL ON OXYCONTIN NETWORK EXPOSURE

	Mean in 2005			Δ from 2005 to 2019		
	All Counties	Exposure > Median	Exposure < Median	All Counties	Exposure > Median	Exposure < Median
Overdose Deaths						
Total Opioids (T40.1-T40.4)	3.56	5.23	1.90	6.05	9.62	2.50
Prescription Opioids (T40.2+T40.3)	2.69	4.10	1.28	0.39	0.56	0.22
Heroin (T40.1)	0.20	0.32	0.08	1.48	2.58	0.38
Heroin+Fentanyl (T40.1+T40.4)	0.72	1.12	0.32	6.07	10.23	1.92
Total Population	9.77	16.67	2.89	1.09	1.93	0.26
Population (%)						
0-14	19.71	19.62	19.80	-1.59	-1.98	-1.20
15-24	13.83	13.94	13.72	-1.46	-1.37	-1.55
25-44	25.14	26.20	24.08	-1.50	-2.01	-0.99
45-64	26.31	26.33	26.29	-0.12	0.17	-0.42
65-84	13.04	12.24	13.85	4.24	4.70	3.78
85+	1.96	1.67	2.26	0.43	0.49	0.37
Race (%)						
Non-Hispanic White	80.93	82.34	79.51	-3.72	-3.74	-3.69
Non-Hispanic Black	9.25	8.78	9.72	0.55	0.61	0.50
Hispanic	7.26	5.97	8.55	2.45	2.32	2.58
Other	2.57	2.90	2.24	0.72	0.81	0.62
Per Capita Income	28.76	30.04	27.52	16.87	16.38	17.34
Unemployment Rate (%)	5.44	5.48	5.40	-1.47	-1.49	-1.45

Notes: The table reports county-level summary statistics. The first three columns display means in the year 2005 for different groups of counties. The last three columns report changes from 2005 to 2019 for different groups of counties. Exposure > Median (Exposure < Median) refers to set of counties for which the network exposure to the 2010 OxyContin reformulation is greater (smaller) than the sample median. All death variables are normalized by county population and expressed per 100,000 residents. Per capita income is expressed in thousands of dollars, while total population is measured in 10^6.

Table D.1 presents summary statistics for (i) all counties, (ii) counties with above median OxyContin network exposure, and (iii) counties with below median OxyContin network exposure. Our sample starts in 2005 and runs through 2019 to observe sufficiently many periods before and after the reformulation. The first three columns indicate that there are some level differences between counties having higher network exposure to the reformulation and counties having a lower network exposure. Counties with more links to regions where the OxyContin reformulation had a larger bite had more opioid overdose deaths in 2005, are larger in terms of population size, and earn somewhat more than counties with fewer connections to regions most affected by the reformulation. As our identification exploits changes over time, these level differences are not too worrisome. We observe only small level difference in population age group shares, racial shares, and labor markets. Moreover, columns (4) to (6) present changes in means from 2005 to 2019. Generally, the changes for above and below median network exposure counties have the same sign. However, regarding the overdose measures the changes are larger in absolute and relative terms for above median network exposure counties suggesting potential friendship network effects.

E Summary Statistics Conditional on PDMP Network Exposure

TABLE E.1—SUMMARY STATISTICS CONDITIONAL ON PDMP NETWORK EXPOSURE

	Mean in 1999			Δ from 1999 to 2019		
	All Counties	ΔExposure > Median	ΔExposure < Median	All Counties	ΔExposure > Median	ΔExposure < Median
Overdose Deaths						
Total Opioids (T40.1-T40.4)	1.01	1.38	0.64	8.61	13.06	4.18
Prescription Opioids (T40.2+T40.3)	0.62	0.85	0.39	2.46	3.66	1.26
Heroin (T40.1)	0.17	0.32	0.03	1.50	2.65	0.36
Heroin+Fentanyl (T40.1+T40.4)	0.37	0.58	0.15	6.43	10.67	2.20
Total Population	9.21	16.32	2.12	1.62	3.13	0.11
Population (%)						
0-14	20.93	20.90	20.95	-2.80	-3.04	-2.57
15-24	13.55	13.75	13.36	-1.19	-1.03	-1.34
25-44	27.81	29.00	26.63	-4.18	-4.64	-3.72
45-64	22.92	22.87	22.97	3.27	3.41	3.13
65-84	12.89	11.92	13.86	4.40	4.73	4.07
85+	1.90	1.56	2.24	0.50	0.57	0.43
Race (%)						
Non-Hispanic White	83.11	82.84	83.39	-5.89	-6.58	-5.21
Non-Hispanic Black	9.67	8.62	10.85	0.72	0.88	0.56
Hispanic	5.71	6.47	4.95	4.01	4.49	3.54
Other	2.14	2.15	2.12	1.12	1.21	1.03
Per Capita Income	22.70	24.37	21.06	22.93	23.20	22.67
Unemployment Rate (%)	4.93	4.73	5.12	-0.96	-0.76	-1.16

Notes: The table reports county-level summary statistics. The first three columns display means in the year 1999 for different groups of counties. The last three columns report changes from 1999 to 2019 for different groups of counties. Δ Exposure > Median (Δ Exposure < Median) refers to set of counties for which the change in the network exposure to must-access PDMPs between 1999 and 2019 is greater (smaller) than the sample median. All death variables are normalized by county population and expressed per 100,000 residents. Per capita income is expressed in thousands of dollars, while total population is measured in 10^6.

Table E.1 presents summary statistics for (i) all counties, (ii) counties with above median change in must-access PDMP network exposure, and (iii) counties with below median change in must-access network exposure. The sample ranges from 1999 to 2019. Thus, we observe sufficiently many years before the first and last must-access PDMP introduction in 2007 and 2015, respectively. The first three columns indicate that there are some level differences between counties that saw larger increases in must-access PDMP network exposure and counties that saw smaller increases. Counties that experienced larger increases had more opioid overdose deaths, were larger in terms of population size, and had higher per capita incomes in 1999. As our identification exploits changes over time, these level differences are not too worrisome. We observe only small level differences in population age group shares, racial shares, and labor markets. To further illustrate the data, columns (4) to (6) present changes in means from 1999 to 2019. Regarding the overdose measures the changes are larger in absolute and relative terms for above median change in must-access PDMP network exposure counties. Nevertheless, the changes for counties with above and below median change in must-access PDMP network exposure have the same sign in general.

F Robustness of Results

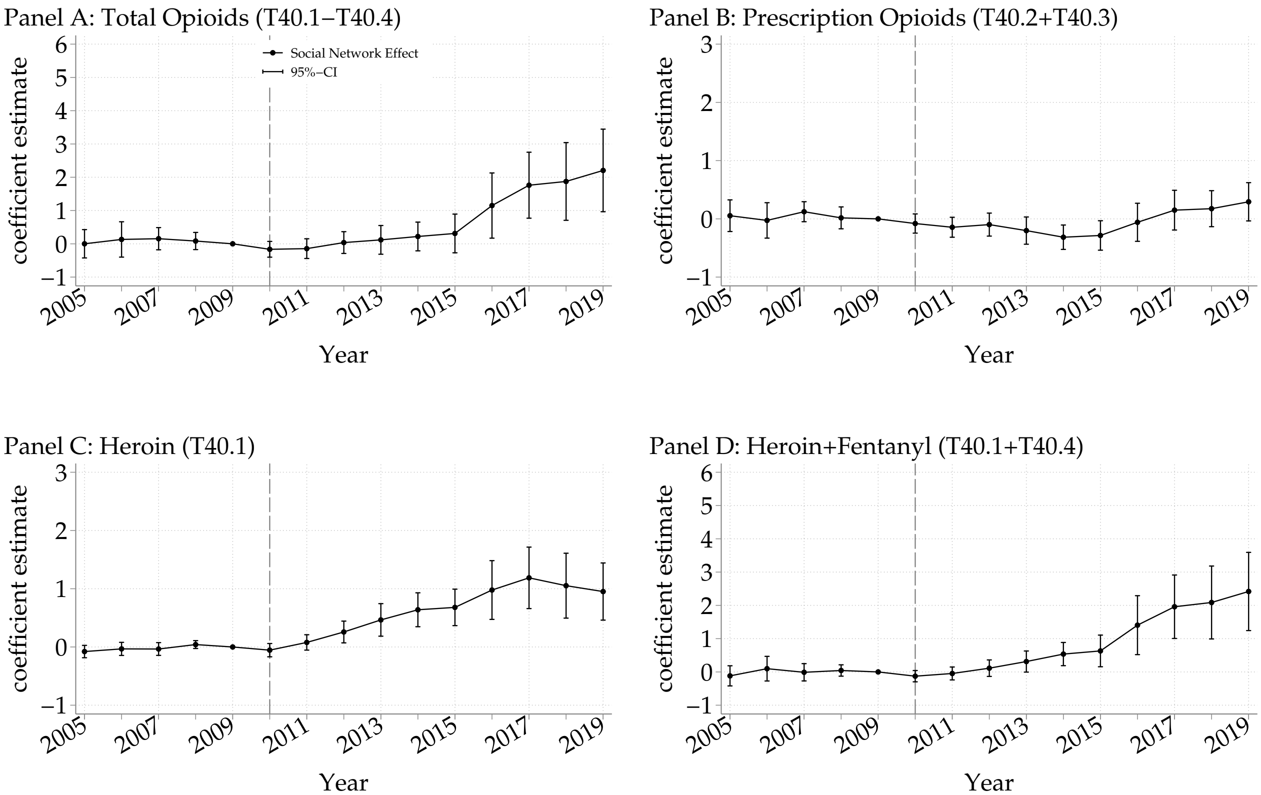

FIGURE F.1—EVENT STUDY SPECIFICATION OXYCONTIN – ROBUSTNESS: DISTANCE > 50 MILES

Notes: The figure illustrates time-specific difference-in-differences coefficients along with 95% confidence bands using different outcome variables. The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. The specifications used to estimate the coefficients include state-by-year and county fixed effects. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted. Standard errors are clustered at the state level.

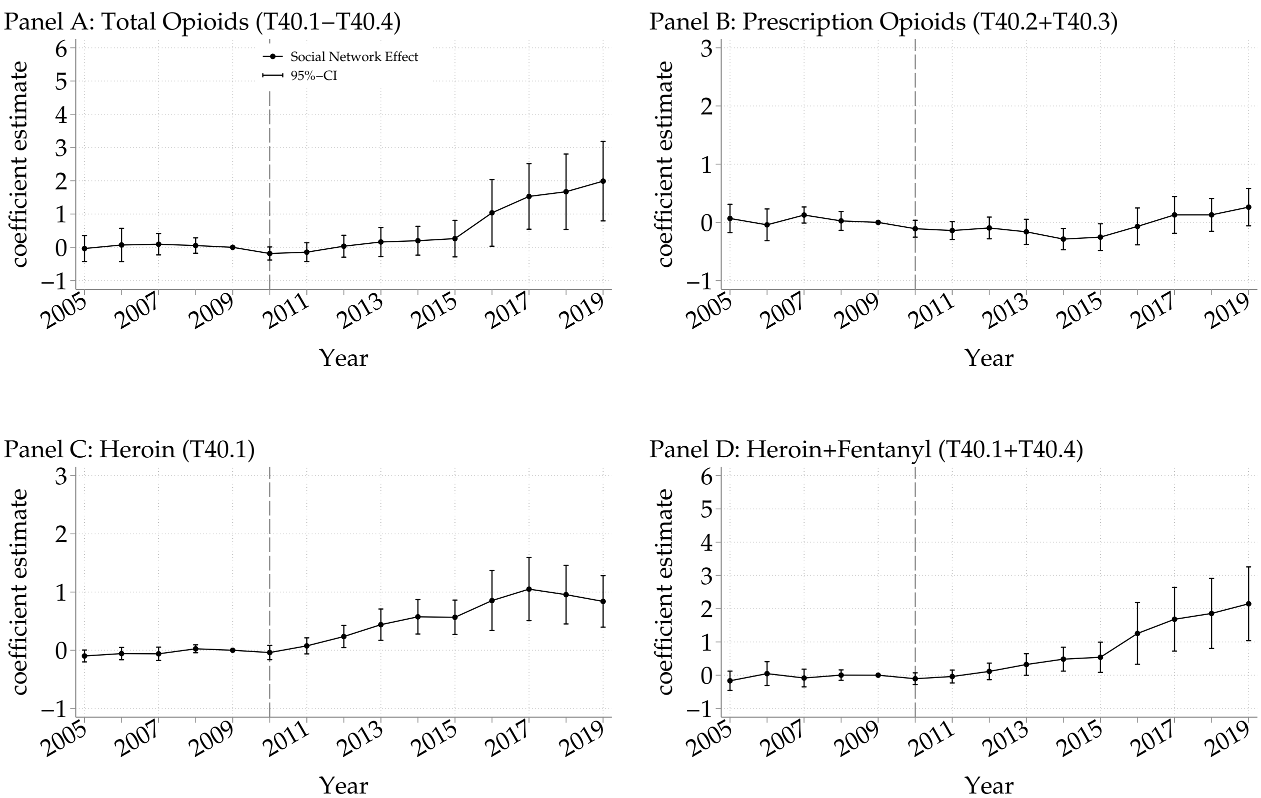

FIGURE F.2—EVENT STUDY SPECIFICATION OXYCONTIN – ROBUSTNESS: DISTANCE > 100 MILES

Notes: The figure illustrates time-specific difference-in-differences coefficients along with 95% confidence bands using different outcome variables. The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. The specifications used to estimate the coefficients include state-by-year and county fixed effects. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted. Standard errors are clustered at the state level.

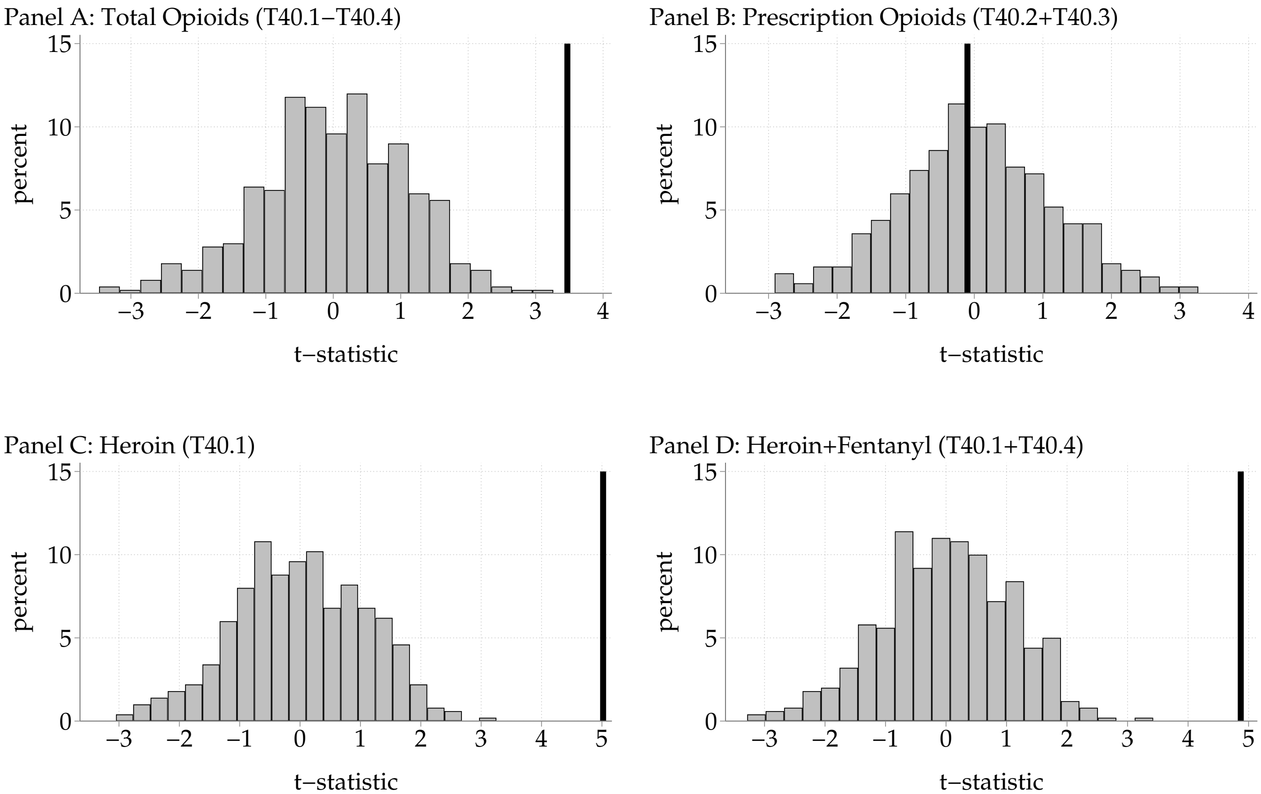

Figure F.3—Effect of Friendship Network Exposure to OxyContin Reformulation across Placebo Experiments

Notes: The figure illustrates the distribution of t-statistics across 500 placebo experiments. In each placebo experiment, we randomly assign social connections between county pairs. We then reconstruct the OxyContin network exposure variable and estimate Equation (7). The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The solid line corresponds to the t-statistic using the true social connections.

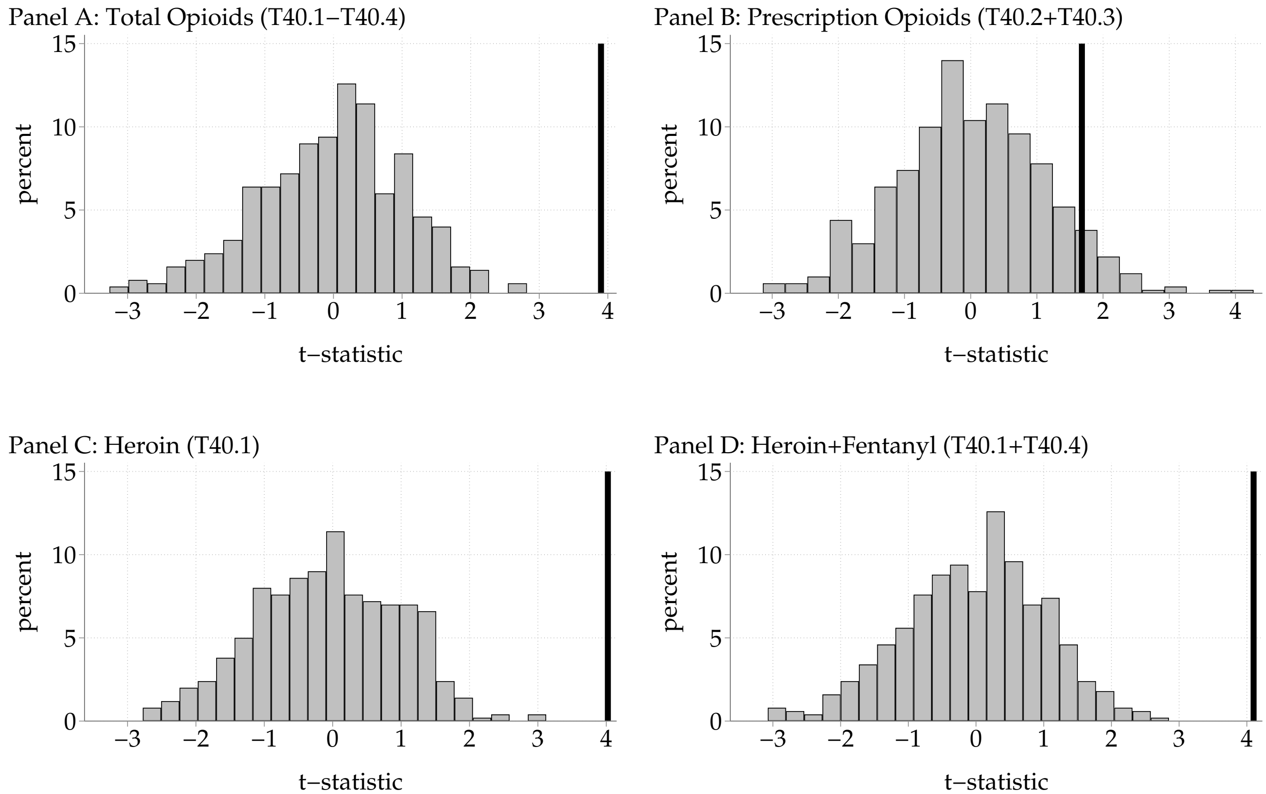

FIGURE F.4—EFFECT OF FRIENDSHIP NETWORK EXPOSURE TO OUT-OF-STATE PDMPS ACROSS PLACEBO EXPERIMENTS

Notes: The figure illustrates the distribution of t-statistics across 500 placebo experiments. In each placebo experiment, we randomly assign social connections between county pairs. We then reconstruct the out-of-state PDMP network exposure variable and estimate Equation (12). The dependent variable in Panel A incorporates total opioid overdose deaths (T40.1-T40.4), Panel B considers prescription opioid deaths (T40.2+T40.3), Panel C focuses on heroin overdose deaths (T40.1), and Panel D concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The solid line corresponds to the estimated effect using the true social connections.

TABLE F.1—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: TRIM TOP 5%

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	0.700***	0.029	0.531***	0.749***
	(0.170)	(0.098)	(0.054)	(0.105)
Pre2010OxyRate × Post	-0.065	-0.088	0.001	0.082
	(0.190)	(0.136)	(0.049)	(0.099)
Dependent Mean	7.02	4.03	1.39	2.99
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	41869	41851	41897	41875
R^2	0.747	0.681	0.737	0.750
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.428**	-0.059	0.410***	0.525***
	(0.171)	(0.086)	(0.047)	(0.088)
Pre2010OxyRate × Post	-0.008	-0.080	0.039	0.140
	(0.203)	(0.131)	(0.064)	(0.118)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	41869	41851	41897	41875
R^2	0.747	0.681	0.739	0.750
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.366**	-0.066	0.357***	0.463***
	(0.164)	(0.076)	(0.047)	(0.084)
Pre2010OxyRate × Post	-0.003	-0.080	0.043	0.145
	(0.205)	(0.130)	(0.067)	(0.122)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	41869	41851	41897	41875
R^2	0.747	0.681	0.738	0.750

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Before running the regression, the top 5% observations based on the respective dependent variable are discarded. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.2—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: COUNTY CLUSTERING

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	1.020***	-0.012	0.803***	1.311***
	(0.232)	(0.099)	(0.138)	(0.211)
Pre2010OxyRate × Post	0.627**	0.031	0.301***	0.743***
	(0.259)	(0.121)	(0.097)	(0.216)
Dependent Mean	8.65	4.44	2.47	5.04
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.759	0.782
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.568***	-0.103	0.596***	0.823***
	(0.181)	(0.070)	(0.099)	(0.163)
Pre2010OxyRate × Post	0.714***	0.036	0.361***	0.850***
	(0.262)	(0.117)	(0.100)	(0.223)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.760	0.782
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.516***	-0.110*	0.543***	0.755***
	(0.174)	(0.067)	(0.092)	(0.156)
Pre2010OxyRate × Post	0.718***	0.036	0.365***	0.855***
	(0.263)	(0.117)	(0.101)	(0.225)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.763	0.680	0.760	0.782

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the county level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.3—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: NO POPULATION WEIGHTING

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	0.966***	0.047	0.748***	1.245***
	(0.222)	(0.083)	(0.123)	(0.240)
Pre2010OxyRate × Post	0.703***	0.227**	0.273**	0.698***
	(0.239)	(0.111)	(0.115)	(0.235)
Dependent Mean	6.47	3.79	0.95	2.74
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.646	0.606	0.630	0.664
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.926***	0.102	0.744***	1.136***
	(0.167)	(0.079)	(0.092)	(0.146)
Pre2010OxyRate × Post	0.770***	0.222*	0.321**	0.792***
	(0.264)	(0.115)	(0.131)	(0.258)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.646	0.606	0.632	0.665
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.897***	0.093	0.713***	1.093***
	(0.179)	(0.076)	(0.104)	(0.156)
Pre2010OxyRate × Post	0.782***	0.224*	0.332**	0.809***
	(0.268)	(0.116)	(0.134)	(0.261)
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.646	0.606	0.632	0.664

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.4—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: DMA × YEAR FE

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	0.914***	-0.014	0.764***	1.220***
	(0.266)	(0.133)	(0.148)	(0.261)
Pre2010OxyRate × Post	0.756**	0.050	0.395**	0.907**
	(0.374)	(0.134)	(0.165)	(0.346)
Dependent Mean	8.68	4.44	2.48	5.06
DMA × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	43930	43930	43930	43930
R^2	0.827	0.734	0.835	0.855
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.473**	-0.072	0.518***	0.707***
	(0.228)	(0.116)	(0.122)	(0.196)
Pre2010OxyRate × Post	0.803**	0.050	0.431**	0.968**
	(0.388)	(0.131)	(0.180)	(0.367)
DMA × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	43930	43930	43930	43930
R^2	0.827	0.734	0.835	0.855
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.412*	-0.084	0.463***	0.650***
	(0.238)	(0.118)	(0.127)	(0.205)
Pre2010OxyRate × Post	0.800**	0.052	0.428**	0.963**
	(0.389)	(0.131)	(0.180)	(0.369)
DMA × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	43930	43930	43930	43930
R^2	0.827	0.734	0.835	0.855

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year, DMA-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.5—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: INCOME × YEAR FE

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	1.051***	0.062	0.623***	1.277***
	(0.385)	(0.154)	(0.172)	(0.341)
Pre2010OxyRate × Post	0.547	0.007	0.278**	0.692**
	(0.371)	(0.171)	(0.135)	(0.298)
Dependent Mean	8.65	4.44	2.47	5.04
Income × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.795	0.702	0.795	0.820
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.546*	-0.080	0.489***	0.770***
	(0.291)	(0.119)	(0.118)	(0.244)
Pre2010OxyRate × Post	0.643	0.020	0.325**	0.803**
	(0.387)	(0.167)	(0.151)	(0.324)
Income × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.795	0.702	0.795	0.820
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.490	-0.086	0.442***	0.707***
	(0.303)	(0.108)	(0.129)	(0.262)
Pre2010OxyRate × Post	0.646	0.020	0.327**	0.806**
	(0.390)	(0.166)	(0.153)	(0.328)
Income × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.795	0.702	0.795	0.820

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year, and county fixed effects as well as year-specific fixed effects for percentiles of average household income. Standard errors are clustered at the state level. The sample consists of 44,146 observations. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.6—NETWORK EFFECT OXYCONTIN REFORMULATION – ROBUSTNESS: MIGRATION

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
Panel A: All friends				
OxyNetExposure × Post	1.354***	0.048	1.013***	1.627***
	(0.315)	(0.141)	(0.176)	(0.287)
Pre2010OxyRate × Post	0.556	-0.103	0.338*	0.806*
	(0.467)	(0.186)	(0.194)	(0.407)
OxyMigExposure × Post	-0.261	-0.024	-0.194**	-0.272
	(0.197)	(0.095)	(0.091)	(0.189)
Dependent Mean	8.65	4.44	2.47	5.04
N	44065	44065	44065	44065
R^2	0.763	0.680	0.761	0.784
Panel B: Distance > 50 miles				
OxyNetExposure × Post	0.765***	-0.075	0.736***	1.009***
	(0.279)	(0.097)	(0.149)	(0.243)
Pre2010OxyRate × Post	0.662	-0.096	0.413*	0.932**
	(0.493)	(0.178)	(0.222)	(0.446)
OxyMigExposure × Post	-0.185	0.010	-0.188**	-0.209
	(0.191)	(0.093)	(0.089)	(0.186)
N	44065	44065	44065	44065
R^2	0.763	0.681	0.762	0.783
Panel C: Distance > 100 miles				
OxyNetExposure × Post	0.690**	-0.075	0.655***	0.906***
	(0.288)	(0.084)	(0.154)	(0.259)
Pre2010OxyRate × Post	0.664	-0.096	0.415*	0.934**
	(0.497)	(0.177)	(0.226)	(0.451)
OxyMigExposure × Post	-0.160	0.010	-0.161*	-0.174
	(0.200)	(0.090)	(0.089)	(0.192)
N	44065	44065	44065	44065
R^2	0.763	0.681	0.762	0.783

Notes: We study the network effects of the OxyContin reformulation on various public health outcomes. OxyNetExposure measures the degree to which a county's social network is exposed to the 2010 OxyContin reformulation. Moreover, Migration Exposure × Post controls for the impact of migration from high Oxycodone counties. Panel A, B, and C implement different restrictions on the physical proximity of county pairs when computing the network exposure. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). The dependent variables are expressed per 100,000 residents. We flexibly control for local economic conditions as well as the direct effect of the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.7—MUST-ACCESS PDMPS – ROBUSTNESS: TRIM TOP 5%

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	0.529***	-0.169*	0.303***	0.661***
	(0.163)	(0.098)	(0.042)	(0.111)
Dependent Mean	3.80	2.18	0.28	1.10
State $\times$ Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	58395	58380	58429	58397
R^2	0.650	0.615	0.688	0.646

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. Before running the regressions, the top 5% observations based on the respective dependent variable are discarded. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.8—MUST-ACCESS PDMPS – ROBUSTNESS: COUNTY CLUSTERING

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.149***	0.203	0.522***	1.097***
	(0.226)	(0.149)	(0.118)	(0.205)
Dependent Mean	6.98	3.73	1.96	3.90
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	61463	61463	61463	61463
R^2	0.837	0.711	0.844	0.869

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the county level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.9—MUST-ACCESS PDMPS – ROBUSTNESS: NO POPULATION WEIGHTING

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.249***	0.319	0.647***	1.199***
	(0.293)	(0.241)	(0.128)	(0.204)
Dependent Mean	5.23	3.15	0.73	2.11
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	61463	61463	61463	61463
R^2	0.695	0.610	0.740	0.768

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.10—MUST-ACCESS PDMPS – ROBUSTNESS: DISCARD PDMP-BORDER COUNTIES

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.442***	0.234	0.761***	1.355***
	(0.430)	(0.325)	(0.277)	(0.382)
Dependent Mean	6.10	3.47	1.65	3.16
State $\times$ Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	40961	40961	40961	40961
R^2	0.848	0.719	0.869	0.882

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. Before running the regressions, counties bordering to PDMP states are discarded. The regressions are population-weighted and include state as well as county fixed effects. Standard errors are clustered at the state-by-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.11—MUST-ACCESS PDMPS – ROBUSTNESS: DMA × YEAR FE

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.428***	0.515***	0.473***	1.169***
	(0.335)	(0.191)	(0.155)	(0.278)
Dependent Mean	7.00	3.73	1.96	3.91
DMA × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	61274	61274	61274	61274
R^2	0.874	0.759	0.887	0.908

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws.The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year, DMA-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.12—MUST-ACCESS PDMPS – ROBUSTNESS: INCOME × YEAR FE

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	0.949**	-0.003	0.523***	1.066***
	(0.363)	(0.133)	(0.171)	(0.351)
Dependent Mean	6.98	3.73	1.96	3.90
Income × Year FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	61463	61463	61463	61463
R^2	0.856	0.731	0.863	0.889

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions include state-by-year, and county fixed effects as well as year-specific fixed effects for percentiles of average household income. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

TABLE F.13—MUST-ACCESS PDMPS – ROBUSTNESS: MIGRATION

	Total Opioids (T40.1-T40.4)	Presc. Opioids (T40.2+T40.3)	Heroin (T40.1)	Heroin+Fentanyl (T40.1+T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	1.080***	0.139	0.499**	1.065***
	(0.379)	(0.169)	(0.216)	(0.362)
PDMP MigExposure	-0.222**	-0.049	-0.074	-0.209**
	(0.095)	(0.055)	(0.052)	(0.098)
Dependent Mean	8.65	4.44	2.47	5.04
State $\times$ Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	44065	44065	44065	44065
R^2	0.850	0.733	0.858	0.887

Notes: The table shows the indirect friendship network effect of must-access PDMPs. PDMP NetExposure measures the degree to which a county's out-of-state social network is exposed to must-access PDMP laws. Migration Exposure controls for the impact of migration from PDMP implementing states. The dependent variable in specification (1) incorporates total opioid overdose deaths (T40.1-T40.4), specification (2) considers prescription opioid overdose deaths (T40.2+T40.3), specification (3) focuses on heroin overdose deaths (T40.1), and specification (4) concerns combined heroin and fentanyl overdose deaths (T40.1+T40.4). All death counts are expressed per 100,000 residents. We flexibly control for demographic and local economic conditions as well as the 2010 OxyContin reformulation by interacting the county-level Oxycodone prescription rate between 2006 and 2009 with a post-2010 dummy. The regressions are population-weighted and include state-by-year as well as county fixed effects. Standard errors are clustered at the state level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.