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Friends with drugs: The role of social networks in the opioid epidemic☆

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HIGHLIGHTS

- Friendship networks help explain the spatial spread of the U.S. opioid epidemic.
- We measure social proximity to the epidemic via the Social Connectedness Index.
- Network exposure to illicit drugs raises heroin and fentanyl overdose deaths.
- Spillovers also appear in earlier stages of addiction, not only overdose deaths.
- Cohesive local friendship networks and social capital attenuate spillovers.

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ABSTRACT

The current opioid epidemic is the worst drug epidemic the U.S. has ever faced. We examine the role of social connections in its spatial spread. Having many friendship links to counties with high opioid exposure positively correlates with overdose death rates. To address identification concerns, we exploit two quasi-natural experiments that led users to substitute illicit opioids for prescription drugs. Having more friends exposed to counties affected by these shocks leads to more overdose deaths. We also provide suggestive evidence that spillovers affect different stages of addiction, are influenced by both supply- and demand-side conditions, and are attenuated in counties with more cohesive local friendship networks. Our results show how social connections contribute to the spread of the opioid epidemic.

1. Introduction

The widespread prescribing of opioid-based painkillers and illicit use of related substances has led to a severe public health crisis in the U.S. that has claimed approximately one million lives over the past two decades. Reports from the Centers for Disease Control and Prevention (CDC) show that more than 107,000 individuals died from drug overdoses in 2022, suggesting that the COVID-19 pandemic has *accelerated* the opioid epidemic. Recent data indicate that the opioid epidemic may be showing signs of decline.[1] Opioid consumption is influenced by a wide range of socioeconomic factors such as personal characteristics and network effects induced by the social environment. In this paper, we focus on the role of friendship networks in the opioid epidemic. Our main contribution is to document that friendship networks help to explain the spread of the opioid epidemic. At a general level, our results thus

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[1] See Provisional Drug Overdose Death Counts, accessed on May 10, 2026.

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provide evidence of the social transmission of a health condition that spreads without person-to-person infection.

To study the role of friendship networks in 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 constructed from the universe of friendship links on Facebook, but serves as a proxy for online and offline friendship networks, both of which could affect drug usage patterns. For example, friends meet in person and exchange information or substances. Consistent with this idea, survey evidence documents that among individuals with substance misuse, approximately 41% obtained their first pills for free from close friends or relatives (Han et al., 2017). Acquaintances might also communicate remotely and share information about drug use or supply sources. A plethora of anecdotal evidence also supports this view: for example, *The New York Times* reports that young buyers almost exclusively obtain illegal substances through social media, where dealers and potential buyers first spot each other and then communicate directly.[2] Offline and online social connections can affect the spread of drugs via both supply-side and demand-side channels.

For each county in our sample, we calculate *Social Proximity* to the opioid epidemic as the SCI-weighted sum of overdose death rates in all other counties. First, we use panel regressions to correlate *Social Proximity* with different overdose death measures. Our analyses are conducted at the county level to enable us to analyze the evolution of the opioid epidemic at a granular level. Second, to approach causality, we leverage the staggered introduction of must-access prescription drug monitoring programs (PDMPs) across different states.

We find that *Social Proximity* to the opioid epidemic significantly correlates with opioid overdose death rates. This result holds after controlling for lagged opioid death rates, demographic factors, economic conditions, and unobserved heterogeneity at the county and state-by-year level. A one-standard-deviation increase in *Social Proximity* to the opioid epidemic is associated with an increase in opioid overdose death rates by more than 25%. 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 in all our analyses. Additionally, we redo our analyses based only from data on county pairs that are geographically distant and find that our results generally still hold.

While we believe that 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, 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 endogeneity concerns. Network formation is generally endogenous, 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 who 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 friendship networks and overdose deaths, which would lead us to inconsistently estimate friendship network effects. Kuchler and Stroebel (2021) highlight two ways to achieve identification of peer group effects: (1) random peer group assignment and (2) random shocks to an endogenously formed peer group. We follow the latter strategy in our analysis. In particular, we leverage policy-induced shocks to illicit drug consumption as quasi-experiments: the staggered introduction of must-access PDMPs in 29 states between 2007 and 2019. PDMPs are state-level programs that collect data on patients' prescription history and make this information available to physicians. Must-access PDMPs require physicians to review patients' prescription records before prescribing strong pain medication, potentially decreasing the likelihood that individuals with a suspicious record of opioid prescriptions continue to be served legal opioid prescriptions.

The idea of our identification strategy is as follows. Must-access PDMPs have been shown to make it harder for drug addicts to feed their addiction using legal opioids, thereby driving individuals to consume illegal alternatives such as heroin and fentanyl (e.g., Kim, 2021). Hence, the introduction of must-access PDMPs constitutes an unintentional shock to illegal drug consumption that is plausibly orthogonal to friendship network formation.

It is important to note that the introduction of must-access PDMPs might well be endogenous. However, as long as the structure of a county's friendship network is unrelated to the direct effect of the interventions in other counties, we can still identify friendship network effects. Moreover, must-access PDMPs are particularly well-suited to identify friendship network effects because they are state-level programs, i.e., only applicable in the implementing state. Thus, there should be no direct effect on counties in non-implementing states but only—if at all—an indirect friendship network effect.

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 linked to increases in total opioid overdose deaths. The effect is driven by 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, which is consistent with state-level evidence from Kim (2021). As the key part of our empirical analysis, we then study how these shocks propagate through friendship networks to socially proximate counties in non-implementing states. Crucially, must-access PDMP legislation 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 isolate the indirect friendship network effect. Our results are consistent with the spread of illicit drug consumption 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 total opioid overdoses by 0.92 deaths per 100,000 residents. The effect size is as large as 12% of the sample average. The effects are largest for heroin and fentanyl overdoses. We find that the spillover channel is roughly one-third to one-half the size of the direct channel depending on the death variable. Moving beyond deaths as the ultimate outcome, we document that friendship networks also affect earlier stages of addiction: exposure to out-of-state PDMPs increases opioid-related Google search activity, illicit drug use, alcohol use disorder, cocaine consumption, marijuana consumption, and depressive episodes, but not legal opioid prescriptions.

In the Online Appendix, as an alternative identification strategy, we explore the spillover effects of another intervention that also led to a substitution toward illegal opioids: the 2010 OxyContin reformulation. Similar to our PDMP analysis, we first provide evidence that the OxyContin reformulation is not only associated with decreases in opioid prescriptions but also with substantial substitution to illicit alternatives such as heroin and fentanyl, confirming prior state-level evidence from Alpert et al. (2018). We then analyze whether the OxyContin reformulation shock spreads through friendship networks. We find that counties having closer ties to other counties more affected by the 2010 OxyContin reformulation experience an adverse impact on total opioid overdose measures in the post-2010 period. The economic magnitude of the effects is consistent with our PDMP results.

We also provide some suggestive evidence on the mechanisms underlying our baseline results, on various stages of addiction as well as related substance abuse patterns being affected by spillovers, and on the role of protective peer effects. On the mechanism side, our results suggest that spillovers are stronger in counties with a high baseline level of opioid prescriptions, with many interstate connections, and in states with "thick markets" for illicit opioids, pointing to an interaction of demand- and supply-side conditions with social

[2] See NYT article, accessed on October 20, 2022.

networks. Finally, we show that local social capital can mitigate negative spillovers from friendship networks: counties with more cohesive and inclusive within-county friendship networks—particularly those that bridge socioeconomic divides—experience attenuated increases in overdose deaths.

We conduct a battery of robustness tests ensuring that our main results are stable. We explicitly control for labor market effects, time-varying effects of income differences, Designated Market Area (DMA)-by-year fixed effects, and migration between counties with different exposure to the epidemic. Our friendship network effect is stable across all specifications. Furthermore, we implement placebo tests where we randomize the social connections between any county pair and reestimate our specifications. We find that the point estimates using the true social connections are located in the extreme tail of the placebo distribution, further validating our empirical results. To mitigate the concern that our friendship network effect might be partly driven by the direct effect of must-access PDMPs, we also perform several robustness checks that impose restrictions on the physical proximity of focal counties to PDMP-implementing states. Finally, we also conduct a matched-sample analysis. Our results are robust across these tests.

Related Literature. Our paper contributes to three different strands of literature. First, we relate to the growing literature that explores the 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; Meinhofer, 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.

Evans et al. (2019) provides similar evidence suggesting that the reformulation of OxyContin did not bring about a reduction in combined heroin and opioid mortality, but that each prevented prescription opioid death was offset by an additional heroin death, while Powell and Pacula (2021) documents sustained negative long-term consequences. Liu and Bharadwaj (2020) analyzes the impact of online drug availability using the phased introduction of Craigslist. They find that both drug abuse and overdose deaths increase significantly after Craigslist's entry. We provide complementary evidence by showing that friendship networks are an important driver of the opioid epidemic.

Two recent papers also examine social-network effects in the opioid epidemic, both of which we view as complementary to our work. Cutler and Donahoe (2024) develop a calibrated structural model in which thick-market externalities and dynamic spillovers in addiction stocks propagate the epidemic across counties and over time. Adamopoulou et al. (2024) use the Add Health survey and an instrumental variables strategy to estimate peer effects in opioid misuse among adults aged 25–34 and their high school best friends. Our paper differs from both in using a quasi-experimental research design based on policy-induced shocks, nationwide county-level data on overdose deaths, and the SCI as a broad proxy for friendship ties beyond narrowly defined peer relationships.

Other authors examine the influence of economic conditions and institutional details (Currie et al., 2018; Pierce and Schott, 2020; Currie and Schwandt, 2021), the effects of location-specific factors (Finkelstein et al., 2022), the role of retail pharmacy ownership (Janssen and Zhang, 2023), and the impact of physicians (Bobroske et al., 2022; Eichmeyer and Zhang, 2022; Schnell, 2022) on the opioid epidemic. Alpert et al. (2022) discuss the introduction of OxyContin in 1996 and the associated marketing activities as a potential cause of the opioid epidemic. They show that state-based triplicate prescription programs[3] 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. We confirm the findings concerning must-access PDMPs as well as the OxyContin reformulation using county-level data and, more importantly, we identify friendship networks as an important driver of the opioid epidemic that has not been formally analyzed before.

Second, we also contribute to the growing literature on friendship networks and social ties. Several studies have identified peer effects in different economic settings such as consumption choices (e.g., Kuhn et al., 2011; Gilchrist and Sands, 2016; De Giorgi et al., 2020), households' financial decisions (e.g., Duflo and Saez, 2003; Hong et al., 2004; Bursztyn et al., 2014; Beshears et al., 2015; Ouimet and Tate, 2020), information aggregation (e.g., Alatas et al., 2016), job opportunities (e.g., Munshi, 2003; Bandiera et al., 2009), microfinance (e.g., Banerjee et al., 2013), and health behavior (e.g., Kremer and Miguel, 2007; Fletcher and Marksteiner, 2017; Fadlon and Niel sen, 2019). Historically, empirical research on friendship networks using large-scale data from online social networks has been scarce due to data availability issues. Only recently, Bailey et al. (2018b) introduced the publicly available SCI based on de-identified Facebook data, triggering more work on friendship networks. Using the SCI as a proxy for social connections in the real world, various authors document friendship network effects in access to capital (Rehbein and Rother, 2020; Kuchler et al., 2022a), tax filing behavior (Wilson, 2022), housing markets (Bailey et al., 2018a), insurance decisions (Hu, 2020), international trade (Bailey et al., 2021), product adoption choices (Bailey et al., 2022), social distancing during COVID-19 (Bailey et al., 2020b), and upward income mobility (Chetty et al., 2022a).

Third, we relate to the literature that studies the role of friendship networks in the spread of epidemics. On a general level, the potential role of friendship networks in 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 renewed interest in this line of research (Buckee et al., 2021). Using the SCI, Kuchler et al. (2022b) 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 opioid overdose deaths and the causal role of friendship networks. To the best of our knowledge, there are no other studies that analyze the spread of the opioid epidemic through friendship networks.

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 socioeconomic control variables. These datasets provide information at the county level, which allows us to study spatial dynamics at a relatively fine geographic granularity.[4] We describe our data in the next subsections.

[3] Triplicate prescription programs require physicians to follow 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 records, the second copy is stored by the pharmacy, and the third copy is sent to the state drug monitoring institution.

[4] Using counties as the unit of observation, which can be relatively small geographical units, could lead to a situation where geographical regions that are in some cases extremely connected and whose opioid use is also highly correlated end up being split. However, we control for physical distance in all our regressions and show that our results hold if we completely exclude geographically close counties from the analysis. Thus, using counties as the unit of observation should not lead to any bias in our results.

2.1. Social connectedness

We proxy for real-world 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 of county-pair Facebook users

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

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, 2021, 2020b), Kuchler et al. (2022b), 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. Only a small fraction of Facebook users use opioids, so one assumption of our analysis is that the SCI is also representative of real-world networks of potential drug users.

We provide illustrative statistics on the SCI in the Online Appendix A. Figure A.1 documents the distance distribution of friendship ties in more detail. Panel A shows that while friendships concentrate locally, with around 60% falling within 100 miles, a non-trivial share extends well beyond commuting distance. Panel B shows meaningful cross-county heterogeneity in the share of long-distance ties. Moreover, the vast majority of counties have more than 25% of their friendship links with places that are more than 100 miles away. In an illustrative example, Figure A.2 depicts the network structure for two counties that rank among the most heavily affected by the opioid epidemic, Cabell County, WV, and Carbon County, UT, demonstrating that strong friendship ties extend well beyond each focal county's immediate region.

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 may be preferable, other authors find that the SCI is extremely stable over time (Kuchler et al., 2022b). Hence, concerns related to using time-constant friendship data should be of minor importance here. To further mitigate potential concerns related to time-varying friendship networks, we access the 2016 version of the SCI and find that our results are virtually identical. Additionally, we find that changes in opioid mortality between 2016 and 2020 are uncorrelated with changes in the average connectedness of a county. Finally, to ensure that migration flows are not driving our results, we explicitly control for migration patterns in a robustness check and find that our results do not change.

2.2. Mortality data

Mortality data for the years 1999 to 2019 come from the National Vital Statistics System (NVSS) Multiple Cause of Death database, 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 causes of death. We access the public-use version of the data and create a dataset at the county-year level. For confidentiality reasons, data points are suppressed in the public-use dataset if a county has fewer than ten deaths per year. However, we can recover almost all data points by applying a backout procedure. For each death category, we first query the total number of deaths at the county level between 1999 and 2019, which is rarely suppressed due to the long time period. We then run the same query but explicitly exclude the year under consideration. In the last step, we construct the number of deaths by taking the difference between the first and second query. We are able to recover almost all deaths with this procedure.[5]

We follow the 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 and fentanyl overdoses (T40.1 + T40.4).[6] It is important to note that these death categories indicate any presence of specific drugs in the system of the deceased person but are not exclusive: we cannot attribute a 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.

Fig. 1 shows national trends in overdose death rates from 1999 through 2019. Total opioid overdoses have increased from 2 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 onward. Third, more recently, synthetic opioid overdoses (caused by, e.g., 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 remains at historically high levels. To illustrate the geographic distribution of the opioid epidemic, Fig. 2 shows a heatmap of total opioid overdose deaths (T40.1-T40.4) between 1999 and 2019. Darker 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 Northeast. In those regions, up to 1000 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. To further illustrate the evolution of the opioid epidemic, we present heatmaps of opioid overdoses at selected points in time in Figure B.1. Our goal is to better understand the spatial spread of the opioid epidemic over time.

[5] In untabulated results, we find that the backed-out death counts exactly match the non-suppressed data points of the public-use dataset. Moreover, we perform several robustness checks and find that our results become even stronger when we conservatively set suppressed death counts to zero, showing that our use of the public version of the data does not lead to any bias favoring us to find any results. In addition, our main regressions are population-weighted, meaning that larger counties with a low probability of data suppression are assigned larger weights, and, as a robustness check, we also document that our results are robust when all observations are weighted equally.

[6] 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.

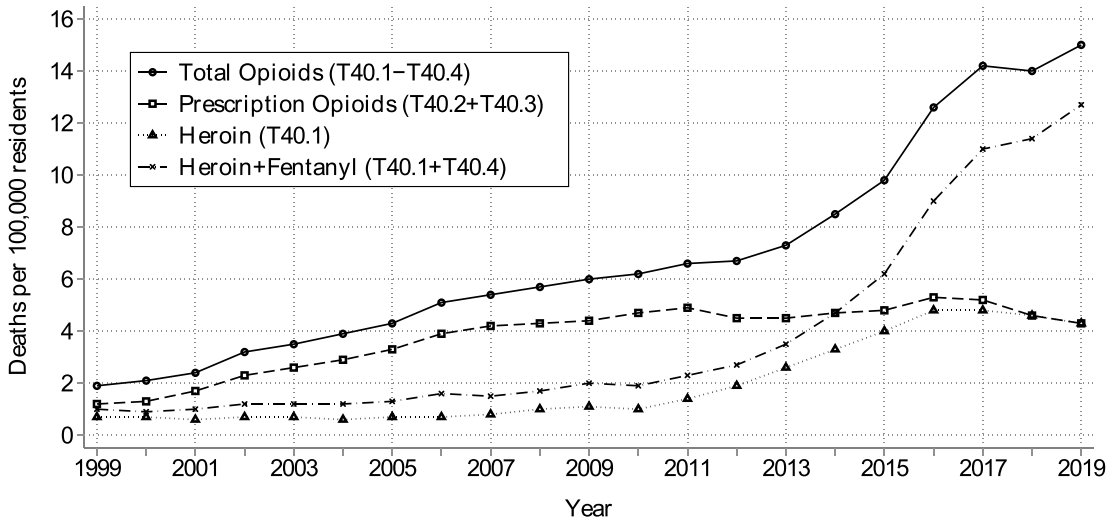

Fig. 1. National trends in death rates. *Notes:* The figure plots national trends in death rates from 1999 through 2019. Drug overdose death rates are identified using ICD-10 underlying cause of death codes X40-X44, X60-X64, X85, and Y10-Y14. We use the following multiple cause of death codes to measure 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).

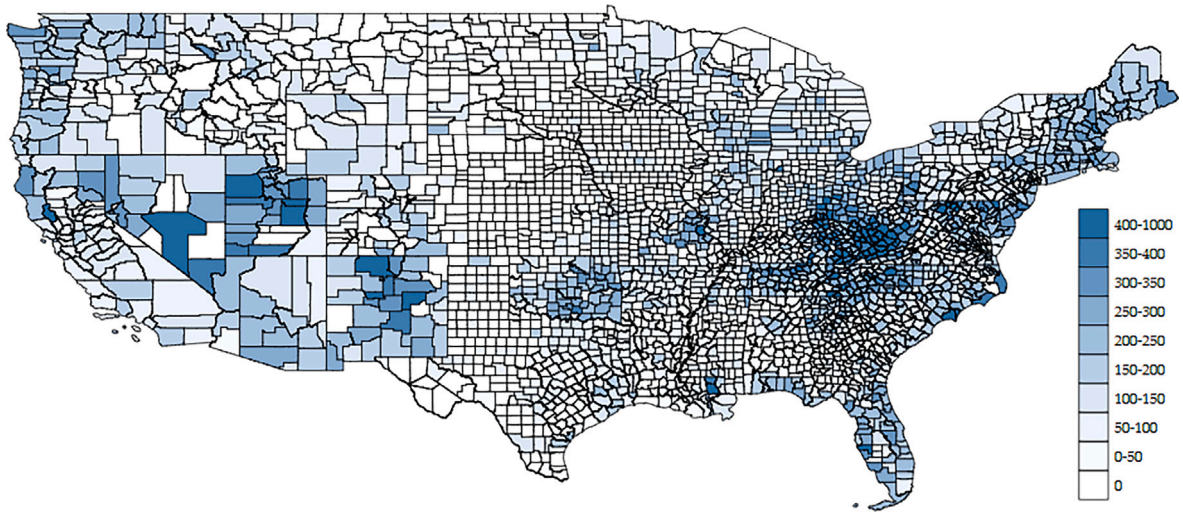

Fig. 2. Geographic distribution of opioid overdose deaths by county. *Notes:* The figure illustrates the geographic distribution of the total number of opioid overdose deaths per 100,000 residents between 1999 and 2019. The data are obtained from the National Vital Statistics System (NVSS). Total opioid 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.

2.3. Further data sources and summary statistics

We gather additional data from multiple other sources. Total population counts and population counts by age group and racial group 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). Data on the physical distance between counties 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. For some of our analyses, we use per-capita opioid prescriptions (CDC) and pharmacy-level opioid shipments sourced from ARCOS DEA.

Merging all different data sources results in our final dataset, which contains 65,247 county-year observations from 1999 to 2019. Since we drop territories and counties in outlying states, we cover 3107 out of 3243 U.S. counties. Table 1 reports pooled summary statistics for our final dataset. The average county in our sample has roughly 100,000 residents consisting of 80% non-Hispanic Whites and 9% non-Hispanic Blacks. While friendship networks seem to be geographically concentrated, the average share of friends further away than 50 (100) miles is still 47% (36%). 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% and are on average 5.93%. The average annual 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 (Cabell County, West Virginia, 2017). 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.

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 millions.

Generally, our summary statistics are in line with other papers that use opioid mortality data at the county level (e.g., Cornaggia et al., 2022).

3. Social proximity to the opioid epidemic and overdose deaths

In this section, we study how *Social Proximity* to the opioid epidemic correlates with overdose deaths. 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 and explicitly quantify how much of the within-county variation in overdose deaths is explained by *Social Proximity* to the opioid epidemic. We emphasize that the results in this section only represent correlations which are nevertheless useful for predicting the future spread of the epidemic. In Section 4, we document results using an identification strategy.

We begin by presenting descriptive evidence showing that the SCI can help predict the geographic spread of the opioid epidemic. To this end, we construct two different time-varying measures of proximity to the opioid epidemic. Our main variable of interest is *Social Proximity* to the opioid epidemic, which 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.[7] The sum j runs over all counties except the focal county i. $Social\ Proximity_{it}$ measures the friendship network-weighted overdose death rate in county i and year t. Correspondingly, *Physical Proximity* to 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 near the focal county i are assigned a higher weight in the construction of *Physical Proximity*. The construction of both proximity measures follows the same approach as in Kuchler et al. (2022b).[8]

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). In line with the evidence in Online Appendix A, Figure C.1 illustrates that there is substantial geographic variation in *Social Proximity* that cannot be explained by *Physical Proximity*. We exploit such variation in our analysis.

We standardize *Social Proximity* and *Physical Proximity* before running our regressions. Thus, our following coefficient estimates can be interpreted as the effect of a one-standard-deviation change in the respective proximity measure. We first study how social and physical proximity dynamically affect overdose death rates. Therefore, we estimate the following regression model:

$$Deaths_{it} = \beta_1 \times Social\ Proximity_{i,t-1} + \beta_2 \times Physical\ Proximity_{i,t-1} + \delta \mathbf{X}_{it} + \mathbf{\Phi}_{it} + \varepsilon_{it}, \tag{4}$$

where $Deaths_{it}$ denotes overdose death rates, $Social\ Proximity_{i,t-1}$ ($Physical\ Proximi\text{-}ty_{i,t-1}$) is a measure of lagged social (physical) proximity to the opioid epidemic, $\mathbf{X}_{it}$ is a matrix of control variables, and $\mathbf{\Phi}_{it}$ contains different sets of location and time fixed effects. We also include the lag of the dependent variable as a control variable in all specifications to account for any persistent effect on opioid death rates. 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.

[7] We study four different measures of overdose deaths (as in Kim (2021)): (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 $Deaths_{jt}$ in Eqs. (2) and (3).

[8] In a robustness test, we construct an alternative version of *Physical Proximity* that is normalized analogously to *Social Proximity*, dividing by $\sum_h 1/(1 + \text{Distance}_{ih})$. Our estimates are robust to this alternative construction (see Table E.1).

Table 2
Spatial spread of opioid epidemic.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Proximity							
Social Proximity$_{t-1}$	1.100***	1.478***	1.514***	1.339***	1.415***	2.176***	1.983***
	(0.270)	(0.219)	(0.211)	(0.201)	(0.203)	(0.279)	(0.286)
Physical Proximity$_{t-1}$	0.534**	0.119	0.295*	0.820***	0.044	1.481***	0.665
	(0.210)	(0.165)	(0.164)	(0.232)	(0.461)	(0.311)	(0.964)
Dependent Variable$_{t-1}$	0.783***	0.766***	0.760***	0.731***	0.724***	0.548***	0.518***
	(0.038)	(0.037)	(0.039)	(0.041)	(0.042)	(0.051)	(0.054)
General Connectedness							
Friends within 100mi		0.013*	0.003	0.012	0.016*		
		(0.007)	(0.007)	(0.009)	(0.009)		
log(SCI)		−0.055	−0.193**	−0.341**	−0.330**		
		(0.095)	(0.093)	(0.150)	(0.158)		
Race							
Non-Hispanic White		0.023***	0.020***	0.030**	0.036***	0.138***	0.134**
		(0.006)	(0.006)	(0.012)	(0.012)	(0.026)	(0.059)
Non-Hispanic Black		0.028***	0.026***	0.041***	0.044***	0.125	0.119
		(0.008)	(0.007)	(0.015)	(0.014)	(0.084)	(0.087)
Age Group							
15–24		−0.014	−0.022	0.045*	0.034	0.204	0.023
		(0.021)	(0.023)	(0.024)	(0.022)	(0.171)	(0.156)
25–44				0.104**	0.119**	0.422*	0.361
				(0.045)	(0.046)	(0.245)	(0.242)
45–64		−0.043	0.007	0.097**	0.059*	0.357	0.252
		(0.034)	(0.033)	(0.039)	(0.033)	(0.249)	(0.234)
65–84		0.040*	−0.013	0.002	0.039	−0.006	−0.051
		(0.024)	(0.025)	(0.029)	(0.037)	(0.172)	(0.174)
85 +		0.084	0.338**	0.509***	0.325**	0.630	0.142
		(0.125)	(0.144)	(0.148)	(0.123)	(0.415)	(0.341)
Economic Conditions							
log(Per Capita Income)			−1.136***	−1.405***	−1.252***	−2.319*	−1.886
			(0.257)	(0.380)	(0.352)	(1.249)	(1.211)
Unemployment Rate			−0.058***	0.081**	0.119***	−0.063	−0.102
			(0.019)	(0.034)	(0.037)	(0.052)	(0.081)
Dependent Mean	7.22	7.22	7.24	7.24	7.23	7.24	7.23
State FE	No	No	No	Yes	No	No	No
Year FE	No	No	No	Yes	No	Yes	No
State × Year FE	No	No	No	No	Yes	No	Yes
County FE	No	No	No	No	No	Yes	Yes
N	62,020	61,771	60,617	60,617	60,597	60,617	60,597
R^2	0.755	0.758	0.761	0.772	0.797	0.795	0.822

Notes: The dependent variable is the total opioid overdose (T40.1-T40.4) death rate per 100,000 residents. Social (Physical) Proximity measures a county's network (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 (Black) denotes the percentage of people in the respective race group. The different age groups measure the percentage of people in the respective age bracket. Unemployment Rate is measured in percent. All regressions are population-weighted. Standard errors are clustered at the state level and in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 2 documents our first set of results. The dependent variable measures total opioid overdose deaths (T40.1-T40.4) per 100,000 residents. Column (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 lagged *Social Proximity* is 1.1. This implies that a one-standard-deviation increase in *Social Proximity* to the opioid epidemic in year $t-1$ is associated with 1.1 more opioid overdoses per 100,000 residents in the following year. Given that the sample mean of total opioid overdoses is 7.23, 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.

In columns (2) and (3) we add measures of general connectedness as well as demographic and 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 a consistently significant impact on total opioid overdose deaths after the inclusion of control variables. In column (3), a ten percent increase in per-capita income results in a statistically significant decrease in opioid overdose deaths by 0.11 per 100,000 residents.

We saturate the model with various fixed effects to control for unobserved (time-varying) state, county, and year-specific effects in columns (4) to (7). The estimated coefficients on *Social Proximity* remain statistically significant at the one percent level and only slightly decrease in magnitude when imposing more conservative model specifications. In our most conservative specification in column (7), we add state-by-year as well as county fixed effects and our results are thus driven by 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. Now, the coefficient estimate on *Social Proximity* becomes 1.98. This effect size is economically large as it represents some 27% of the mean opioid overdose death rate. *Physical Proximity* shows a positive but insignificant estimate. Correspondingly, the effect size of *Social Proximity* remains much larger in 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 now display an insignificant relationship 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. The only exception

Table 3
Overdose deaths and social proximity.

	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)
Social Proximity$_{t-1}$	1.983***	0.819***	0.562***	1.185***
	(0.286)	(0.125)	(0.092)	(0.322)
Physical Proximity$_{t-1}$	0.665	0.822**	0.395	0.317
	(0.964)	(0.372)	(0.309)	(0.653)
Dependent Variable$_{t-1}$	0.518***	0.291***	0.569***	0.689***
	(0.054)	(0.031)	(0.029)	(0.065)
Dependent Mean	7.23	3.85	2.02	4.04
N	60,597	60,597	60,597	60,597
R^2	0.300	0.109	0.398	0.462

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. The dependent variable in specification (1) is 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. Control variables are included as in Table 2, column (7). 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 in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

is the influence of the race variables, which indicates that counties with a higher non-Hispanic (Black or White) population share experience significantly higher death rates. Additionally, the coefficient on the lagged dependent variable is statistically significant across all specifications and ranges between 0.51 and 0.78.

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 the most conservative specification of Table 2 using different measures of overdose deaths by drug category instead of aggregate death rates. Table 3 documents the results. For the sake of clarity, we only report coefficient estimates on *Social Proximity*, *Physical Proximity*, and the lagged dependent variable. 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. In column (1) we repeat the results for aggregate death rates for comparison. Irrespective of the overdose death measure used in the regressions, *Social Proximity* is 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 one standard deviation in year $t-1$, combined heroin and fentanyl overdoses increase by 1.19 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.[9] Regarding *Physical Proximity* the picture is less clear. While we obtain the expected positive coefficients in most cases, they are often not significant, suggesting that it plays a less important role than *Social Proximity*.

Even though we directly control for *Physical Proximity* in our regressions, it is possible that our coefficient estimates on *Social Proximity* partially take up effects of local socioeconomic factors that are not included in our analysis but are 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, Table E.2 of the Online Appendix reports results based on a modified version of the *Social Proximity* measure. In particular, instead of summing over all counties, we now only consider counties having a physical distance of more than 50 miles (Panel A) or 100 miles (Panel B) to the focal county, while still normalizing with respect to the full network. We continue to find strong evidence for an important role of *Social Proximity* in predicting the spread of the epidemic—especially for illegal opioids such as heroin and fentanyl.

We provide additional information on the predictive power of *Social Proximity* in Table F.1 of the Online Appendix. More specifically, we compute the incremental within-R^2 of the regressors used in our baseline regressions in Table 3. 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 *Physical Proximity* and socioeconomic variables do not explain a substantial fraction of the time variation in county-level overdose deaths as the reported incremental within-R^2s show. However, our results strongly indicate that *Social Proximity* is the most important variable for explaining time variation in county-level overdose deaths. For example, the additional within-county variation in total opioid overdose deaths explained by *Social Proximity* amounts to roughly 1.07%, while all other variables considered—except for the lagged dependent variable—individually explain clearly less than 0.4% of the variation.

4. Quasi-experimental variation in network exposure to the epidemic

While our results hitherto are suggestive of a causal impact of social proximity, the baseline regressions are plagued by potential endogeneity concerns due to the inherently endogenous nature of friendship networks. Therefore, our results could be driven by an omitted third factor that determines both the structure of county friendship networks and overdose deaths. To address this concern, we use a shock to drug consumption that affects a locally restricted part of the network.

4.1. Direct effect of PDMPs

In particular, we exploit policy-induced shocks to illicit drug consumption that are orthogonal to friendship network formation. We analyze how county-level overdose deaths change when out-of-state must-access PDMPs are implemented. 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 which shows 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.

We emphasize that our identification strategy does not require PDMP introductions to be exogenous. It is actually not unlikely that the introduction of must-access PDMPs is endogenous. However, as long as the

[9] To show that our results are not driven by outliers in specific years, Figure D.1 in the Online Appendix D documents year-specific effects of *Social Proximity* to the opioid epidemic. Generally, we find that the coefficient estimates are fairly stable over time. In line with the observation that fentanyl played a minor role in the early years of the epidemic, the coefficients in Panel D of Figure D.1 (combined heroin and fentanyl overdose deaths) start to become consistently significant only from 2008 on.

structure of a county's friendship network is unrelated to the direct effect of the interventions in *other* counties—an assumption that seems plausible—we can still identify friendship network effects.

We focus on the introduction of must-access PDMPs, i.e., state-level databases that keep track of a patient's prescription history and provide useful information to physicians (Davis et al., 2014). PDMPs are either with or without a must-access provision. In the 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 at the physician's discretion whether 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 29 must-access PDMPs implemented between 2007 and 2019. Table G.1 in the Online Appendix shows the introduction dates of must-access PDMPs obtained from Sacks et al. (2021).

We proceed 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. These findings confirm the state-level results of Kim (2021) in our slightly expanded sample and show that they also hold at the county level. Second, and more importantly, we exploit this shift toward illicit drug consumption using a difference-in-differences design to study how the opioid epidemic propagates through friendship networks to counties in other states. Specifically, we analyze how counties with many connections to counties in states that implement a must-access PDMP, i.e., to regions that experience a positive shock to illicit opioid consumption, differ from counties with fewer such connections in terms of subsequent opioid-related death rates. Must-access PDMPs provide an ideal setting to analyze friendship network effects since they are introduced at the state level in a staggered way over time. Thus, they only affect counties located in the implementing state, while out-of-state counties are not directly affected. The absence of any direct effect on counties in non-implementing states allows us to cleanly estimate the indirect friendship network effect.[10]

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

$$y_{it} = \theta \times PDMP_{it} + \delta \mathbf{X}_{it} + \phi_i + \gamma_t + \varepsilon_{it}, \tag{5}$$

where $PDMP_{it}$ is a dummy variable that is one when a county has a must-access PDMP provision in year t.[11] As above, y_{it} are different measures of opioid overdoses or opioid prescriptions per capita. Moreover, $\mathbf{X}_{it}$ is a matrix of socioeconomic control variables. Following Kim (2021), we control for the OxyContin reformulation when estimating the impact of must-access PDMPs.[12] 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 in this setting because our main variable of interest $PDMP_{it}$ 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. Table 4 shows the direct effects of must-access PDMP introductions. Column (1) indicates that must-access PDMPs indeed reduce opioid prescriptions by 0.04 per capita, or as much as 5.3% of the average annual prescription rate. At the same time, however, columns (2) to (5) show that overdose deaths do not decline but increase significantly, confirming the evidence from Kim (2021). 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 in opioid prescriptions, column (3) shows that overdoses involving prescription opioids increase after a must-access PDMP is introduced. This finding is likely to be driven by the fact that our overdose measures indicate any presence of specific drugs but are not exclusive: we cannot 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 an increase in prescription opioid overdose death rates.[13]

4.2. Spillover effects of out-of-state PDMPs

Baseline analysis. After confirming that must-access PDMPs represent a shock to illicit drug consumption in counties of implementing states, in the main step of this analysis we now study whether this shock spills over to non-implementing regions through friendship networks. We hypothesize that shocks to illicit drug consumption due to must-access PDMPs propagate through friendship networks and eventually lead counties with more social connections to states with must-access PDMPs to experience larger increases in opioid overdose deaths than counties with fewer social connections to these implementing states. To measure changes in must-access PDMP network exposure we define

$$PDMP\ NetExposure_{it} = \sum_{s(i)\neq s(j)}^{S} \mathbb{1}(\text{PDMP in state } s(j))_t \times \frac{SCI_{ij}}{\sum_h SCI_{ih}}, \tag{6}$$

where $\mathbb{1}(\text{PDMP in state } s(j))_t$ captures whether state $s(j)$ has a must-access PDMP in year t. The sum j runs over all states $s(j)$ except focal county i's own state $s(i)$. Intuitively, $PDMP\ NetExposure_{it}$ measures how strongly a county's friendship network is exposed to states which have already implemented a must-access PDMP in year t. Hence, it proxies for the degree to which counties are confronted with a PDMP-induced 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 implement must-access PDMPs during our sample 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

[10] Out-of-state counties could be indirectly affected if individuals travel across state borders to circumvent must-access PDMP legislation in their home state. The analysis presented in Table J.2 in the Online Appendix excludes counties that are geographically close to implementing states and shows that our main results are unaffected.

[11] We also provide estimates of the average treatment effect based on Callaway and Sant'Anna (2021) in the Online Appendix H. We find our baseline OLS results to be confirmed. Additionally, note that the choice of the method to estimate the direct effect of PDMPs does not affect our follow-up analyses.

[12] In particular, to absorb any confounding effects of the OxyContin reformulation, we include $Pre2010OxyRate_i \times Post_t$ in $\mathbf{X}_{it}$, where $Pre2010OxyRate_i$ are total county-level oxycodone shipments between 2006 and 2009.

[13] 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. Consistent with this explanation, Jones et al. (2015) report that around half of all heroin users are also addicted to prescription opioid pain relievers. Therefore, we run additional tests and only consider deaths attributable exclusively to prescription opioids but *not* heroin or fentanyl. As expected, the positive effect of PDMPs on prescription opioid overdoses vanishes, with the point estimate shrinking to 0.26 and being insignificant (p-value = 0.23). Thus, our findings overall indicate that must-access PDMPs cause a substantial shift from prescription opioids to illicit drugs.

Table 4
Must-access PDMPs: Direct effect.

	Opioid Presc.	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)	(5)
PDMP	−0.039**	4.847***	1.010***	1.933***	4.605***
	(0.018)	(1.369)	(0.291)	(0.580)	(1.364)
Dependent Mean	0.74	7.80	4.12	2.18	4.40
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
N	35,913	52,876	52,876	52,876	52,876
R^2	0.942	0.627	0.605	0.619	0.615

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) covers total opioid overdose deaths (T40.1-T40.4), specification (3) considers prescription opioid overdose deaths (T40.2 + T40.3), specification (4) focuses on heroin overdose deaths (T40.1), and specification (5) 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 in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

networks by estimating the following regression model:

$$Deaths_{it} = \alpha \times PDMP\ NetExposure_{it} + \delta \mathbf{X}_{it} + \phi_i + \gamma_{st} + \varepsilon_{it}. \quad (7)$$

Eq. (7) 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 Eq. (5), we include $Pre2010OxyRate_i \times Post_t$ in $\mathbf{X}_{it}$ to control for any confounding effect of the OxyContin reformulation.[14] Moreover, we control for economic and socio-demographic conditions as well as geographical distance to PDMP-implementing states.[15] 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 (e.g., Pill Mill laws[16]), law enforcement, socioeconomic 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 implementing states would have behaved like other counties in the same state that have 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.[17]

Table 5 shows estimation results from model (7) to test whether illicit drug consumption is transmitted through friendship networks. In column (1), we find that a one-standard-deviation increase in friendship network exposure to out-of-state must-access PDMPs increases total opioid overdoses by 0.92 per 100,000 residents, which corresponds to 12% of the sample average. The effect is also statistically significant at the five percent level.

In contrast, results in column (2) show that prescription opioid overdoses do not exhibit significant network effects as the coefficient estimate is statistically and economically insignificant. This result reinforces confidence 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 sizable network effects for prescription opioid overdoses despite our identification aiming at illicit opioids.

Table 5
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	0.920**	0.138	0.560***	0.975***
	(0.393)	(0.107)	(0.169)	(0.352)
Dependent Mean	7.79	4.12	2.18	4.39
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	52,858	52,858	52,858	52,858
R^2	0.759	0.670	0.752	0.779

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) is 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. Moreover, we control for geographical distance to PDMP-implementing states. 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 in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

[14] As above, the results are robust to controlling for $Pre2010OxyRate_i$ interacted with a full set of year dummies (see Table J.6 of the Online Appendix).

[15] We define physical proximity to PDMPs for county i and year t as $\sum_{s(i)\neq s(j)}^{S} \mathbb{1}(\text{PDMP in state } s(j))_t \times \frac{1}{1+Distance_{ij}}$, where i indexes the focal county and j indexes counties in other states.

[16] In untabulated results, we find that our results remain quantitatively unchanged when excluding Florida from the sample.

[17] Additionally, Table I.1 in the Online Appendix I 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 PDMP network exposure.

Finally, columns (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 exposure to out-of-state must-access PDMPs is related to an increase in heroin (combined heroin and fentanyl) overdoses by 0.56 (0.98) deaths per 100,000 residents. The effect size corresponds to 26% (22%) 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.

To put the friendship network effect in perspective, we translate our estimated coefficient on network exposure into an average per-capita spillover effect. Specifically, we multiply the coefficient by each county-year's observed network exposure level and take a population-weighted mean across counties and years. This yields an average spillover effect of 1.61 per 100,000 residents for total opioid overdose deaths, 0.98 for heroin deaths, and 1.71 for combined heroin and fentanyl deaths. By comparison, the direct effects of must-access PDMP adoption are estimated at 4.85, 1.93, and 4.61 deaths per 100,000 residents for the same three outcomes. The spillover channel is therefore roughly one-third to one-half the size of the direct channel depending on the outcome.

The identification assumption in model (7) 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. However, 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. We implement a stacked-event-study approach (Cengiz et al., 2019) and treat each out-of-state must-access PDMP introduction as a separate event. Specifically, we stack the data for each event and estimate the average effect across all out-of-state must-access PDMP introductions as

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

In this setting, SCI_{ij} is the SCI for county i and the implementing state j. The fraction $SCI_{ij}/\sum_g^S SCI_{ig}$ thus 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 (7). The county fixed effects from (7) 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 (γ_{jst}) to ensure a within-event comparison. Therefore, one can interpret Eq. (8) 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.

The coefficients of interest α_t are plotted along with 95% confidence bands in Fig. 3 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 Fig. 3 are in line with the aggregate baseline results provided in Table 5.[18] Across all panels, we do not observe evidence of differential pre-trends as the coefficient estimates are close to zero and statistically insignificant. 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 alleviates potential concerns that counties with more connections to must-access PDMP-implementing states are systematically different in key characteristics that affect overdose measures over time compared to counties with fewer such connections.

When out-of-state must-access PDMPs are introduced, we observe a small and typically insignificant change in prescription opioid overdoses, while total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses significantly diverge, suggesting that shocks to illicit drug consumption caused by must-access PDMPs are propagated through friendship networks. Hence, this implies that overdose measures in counties with many and few social connections to implementing states were on the same trend and only diverged after out-of-state must-access PDMPs are implemented, lending credibility to our identification assumption.[19]

Stages of addiction. Our main analysis focuses on death variables, because data on these outcomes are available at the county-year level. However, death represents only the ultimate consequence of opioid addiction. To better understand earlier stages of addiction, we move to the state-year level, where additional data are available to us. We also aggregate network exposure to PDMPs at the state level and consider two sets of outcomes.[20] First, we examine Google search volumes for the terms "opioids", "fentanyl", and "heroin", which capture early information-seeking behavior. Second, we use data from the National Survey on Drug Use and Health (NSDUH) on illicit drug use, alcohol use disorder, cocaine consumption, marijuana consumption, and depressive episodes, which reflect different stages of substance use and associated mental health conditions. The NSDUH variables are expressed per 100,000 residents.

The results in Table 6 suggest that friendship networks matter well before addiction causes deaths: we find positive and significant spillovers on opioid-related search behavior, illicit drug use, alcohol use disorder, cocaine, and marijuana consumption. These positive spillovers across substances are consistent with widespread polysubstance use—cocaine and alcohol are frequently co-used with opioids, and cannabis tends to co-move with broader substance use. While one might expect a negative spillover on marijuana if opioids and cannabis were substitutes, the evidence for substitution is mixed at best: Bachhuber et al. (2014) find substitution whereas Shover et al. (2019) document they are complements in a sample period more similar to ours. We therefore read these estimates as evidence that friendship networks transmit substance use broadly rather than reallocating it across substances. We also find an increase in depressive episodes, highlighting that illicit drug use may exacerbate underlying mental health vulnerabilities. A limitation of these results is the state-level resolution of the NSDUH data.

4.3. Robustness of results

Sample restrictions. We only consider out-of-state friendship networks and include state-by-year fixed effects in our identification to eliminate potential direct effects of PDMP introductions. Even though we control for state-by-year fixed effects, one concern might be that our friendship network effect at least partly loads on the direct effect of must-access PDMPs. To mitigate this concern we perform two different tests. First, we estimate the friendship network effect for counties that never implement a must-access PDMP during our sample period.

[18] Panel B shows that there is a temporary but economically small increase in prescription opioid deaths. As discussed above, this effect can be explained by the fact that the overdose measures indicate the presence of specific drugs, but are not exclusive to a specific drug when multiple drugs are listed on the death certificate.

[19] The relatively quick emergence of spillovers is striking, but two factors help reconcile it. First, the event-study coefficients diverge two to three years after the out-of-state PDMP introduction, consistent with the time substitution takes to propagate into fatal overdoses. Second, these shocks arrive atop an already-evolving epidemic, so network exposure should be interpreted as accelerating pre-existing addiction trajectories rather than initiating new ones.

[20] We compute the average state-level exposure by summing up all county-to-county friendship links.

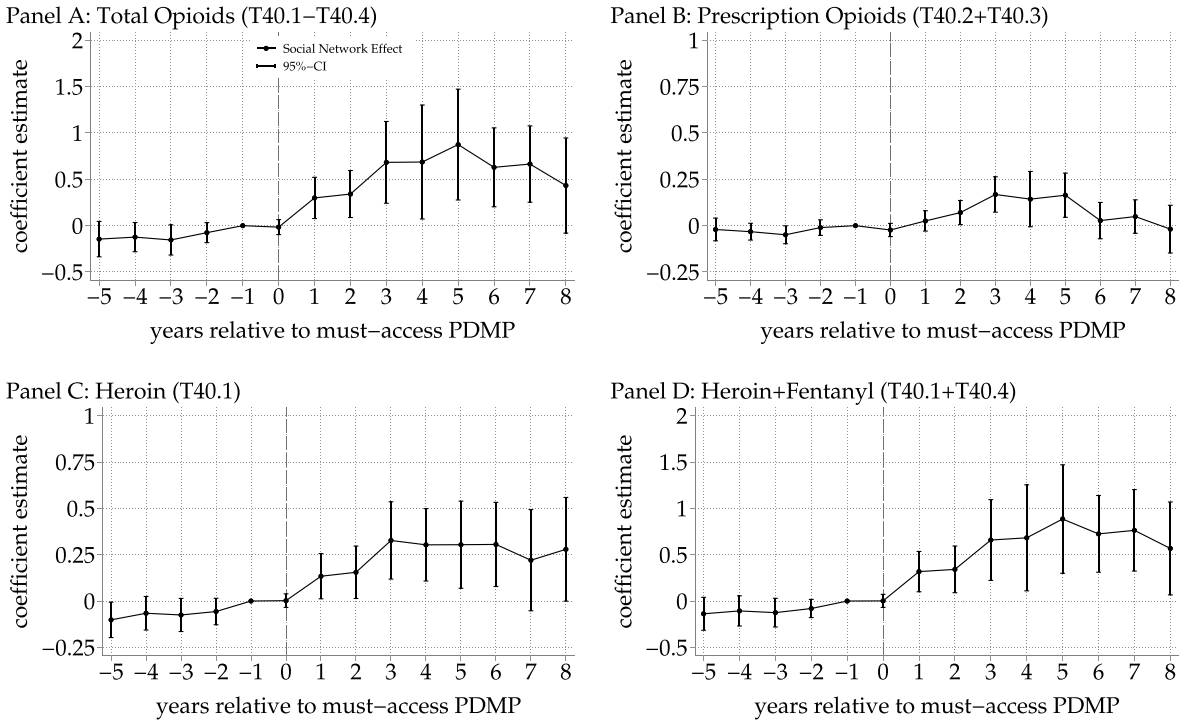

Fig. 3. Event study specification: Friendship network exposure to out-of-state PDMPs. *Notes:* The figure illustrates dynamic difference-in-differences coefficients along with 95% confidence bands using different outcome variables. We consider the friendship network exposure to out-of-state must-access PDMP introductions. In our sample, we examine 29 staggered adoptions of must-access PDMPs. The dependent variable in Panel a is 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. Moreover, we control for geographical distance to PDMP-implementing states. The regressions are population-weighted. Standard errors are clustered at the state level and are shown in parentheses.

Table 6
Friendship network effect: Stages of addiction.

	Google Search Opioids	Illicit Drug Use	Alcohol Use Disorder	Cocaine Consumption	Marijuana Consumption	Depressive Episodes
	(1)	(2)	(3)	(4)	(5)	(6)
PDMP NetExposure	0.076**	0.098**	0.093***	0.046**	0.501***	0.043*
	(0.035)	(0.043)	(0.035)	(0.020)	(0.074)	(0.024)
Dependent Mean	-0.00	1.95	5.42	1.55	5.79	5.30
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	784	539	784	784	784	686
R^2	0.697	0.734	0.851	0.824	0.897	0.730

Notes: The table shows the indirect friendship network effect of must-access PDMPs for selected stages of addiction. PDMP NetExposure measures the degree to which a state's social network is exposed to other states' must-access PDMP laws. The dependent variable in specification (1) is the Google search volume of the terms "fentanyl", "heroin", and "opioids". In all other specifications, we consider variables from the NSDUH survey. We control for state-level economic conditions, own-PDMP introductions, physical distance to out-of-state PDMPs, and the 2010 OxyContin reformulation. The regressions are run at the state level and include state and year fixed effects. Standard errors are clustered at the state level and in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Second, we estimate the friendship network effect by excluding counties from our sample once they enact a must-access PDMP. Our results remain highly significant and of similar magnitude (see Table J.1 in the Online Appendix).

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, bias our friendship network effect. To address this potential concern, we discard all counties adjacent to or within 100 miles of any state implementing an out-of-state PDMP. We again find our results to hold (see Table J.2). In an even more conservative robustness test, we combine the two sets of robustness analyses from before and exclude (i) all counties that are less than 100 miles away from a PDMP-implementing state and (ii) all counties in PDMP-implementing states after the PDMP is enacted. The baseline effects are robust (see Table J.3).

Migration. Another concern might be that our friendship network effect is driven by migration patterns as some counties might experience high

migration from PDMP states. Correlation between the SCI and migration rates could bias our estimates. 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 out-of-state PDMPs through migration as $PDMP\ MigExposure_{it} = \sum_{j \neq s}^{S} \mathbb{1}(\text{PDMP in state j})_t \times Migration_t^{ij}$, 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. Exposure to PDMPs through migration is high if 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 Eq. (7) and include $PDMP\ MigExposure_{it}$ as an additional term. The results are presented in Table J.4. 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 three 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.

Matched sample analysis. As a robustness check, we implement a matched sample analysis to ensure that our results are not driven by systematic differences between high and low exposure counties. Specifically, for each out-of-state PDMP introduction, we define treated counties as those with above-average social proximity to the event. We then match each treated county to a similar control county based on pre-treatment characteristics, including opioid death rates, unemployment, income, and prescription levels. We stack all events and reestimate our regressions on the matched sample. The matching approach is successful in reducing the pre-event differences between high and low exposure counties with all differences being statistically insignificant (see Table J.5). Figure J.1 shows that the matched-sample event study plots are quantitatively very similar to our baseline findings.

Additional sensitivity checks. We present further evidence in the Online Appendix that our main results for the friendship network effects of out-of-state PDMPs are consistent across a wide range of specifications. We check the robustness of our results by implementing the following additional analyses: placebo tests (Figure J.2), controlling for the dynamic effect of the OxyContin reformulation (Table J.6), trimming the top 5sample based on the respective dependent variable (Table J.7), clustering standard errors at the county level (Table J.8), giving equal weight to all counties in our regressions (Table J.9), controlling for DMA-by-year fixed effects (Table J.10) as well as time-varying effects of income differences (Table J.11), excluding potentially endogenous control variables (Table J.12), using alternative PDMP dates from Horwitz et al. (2021) (Table J.13), and adopting the absolute number of friendship links rather than the relative friendship intensity used in the baseline PDMP-exposure analysis (Table J.14). We find that our results are robust across these tests.

Additional identification strategy. Finally, in Section L of the Online Appendix, we provide evidence from a different identification strategy based on the 2010 OxyContin reformulation. Consistent with the must-access PDMP results discussed in this section, we find that counties having closer ties to other counties more affected by the 2010 OxyContin reformulation experience an adverse impact on total opioid overdose measures in the post-2010 period.

5. Mechanisms and further discussion

Our findings indicate that the opioid epidemic spreads through friendship networks. While the nature of our data limits our ability to pinpoint the exact mechanisms and channels through which the baseline effect operates, this section provides additional evidence on the conditions under which spillovers are stronger or weaker. Specifically, we examine whether spillovers arise from legal or illegal drug use, whether physical contacts play a role, and which factors make counties particularly vulnerable or resilient.

Potential mechanisms. Our results suggest that opioid use can spread via social networks. In the early waves of the opioid epidemic, many users started by obtaining opioid-based painkiller prescriptions from physicians. In the next step of our analysis we thus want to investigate the role opioid-prescribing physicians play. Therefore, in our main regression, we replace opioid-related death rates with legal opioid prescriptions. The results are shown in column (1) of Table 7. We find no evidence that legal opioid prescriptions increase if a county has a high network exposure to out-of-state PDMPs. This suggests that peer influence does not primarily operate through individuals seeking legal opioid prescriptions from their physicians.

We also test whether spillovers are stronger in counties where many residents already have experience with legal opioids, making exposure to out-of-state illegal drug use more likely to fall on fertile ground. Thus, we introduce an interaction between the dummy variable *High Prescriptions 2006*—which equals one if a county's per-capita legal opioid prescribing in 2006 (the year preceding the first PDMP introduction in our sample) is above the median—and $PDMP\ NetExposure$. We then reestimate our baseline specification from column (1) of Table 5. Column (2) of Table 7 shows that spillovers are indeed much stronger in counties with higher initial rates of opioid prescriptions. This pattern is intuitive: a higher stock of opioid prescriptions implies a larger pool of individuals with prior exposure to opioids, which can facilitate the transmission of behaviors through social networks. In other words, demand-side conditions matter, as existing users may be more receptive to information or opportunities circulating through their social ties.

Next, we analyze whether information about drugs is exchanged without direct physical contact, or whether in-person meetings, where not only information but also drugs themselves can directly be exchanged, might also play a role. To this end, we define a dummy variable *High Interstate* that takes on the value one if a county has an above-median number of direct interstate connections, as a proxy for infrastructural proximity and ease of travel.[21] We relate the interaction of this variable with our baseline network exposure variable to total opioid death rates. We find a significant estimate for the interaction effect in column (3) of Table 7, while the baseline impact of PDMP network exposure is reduced by about 50% as compared to our main result from Table 5, i.e., spillover effects are stronger when traveling is easier. This evidence is consistent with remote information transmission not being the only channel that drives our main results, but physical contacts across county and state borders also play a role.

Finally, we examine whether supply-side factors facilitate opioid spillovers. Specifically, we test whether counties located in states with "thick markets" for drugs exhibit stronger friendship network effects. We proxy market thickness by the occurrence of heroin, fentanyl, or hydrocodone in law enforcement seizures reported by the National Forensic Laboratory Information System (NFLIS) program. A county is classified as having a thick market if it is located in a state belonging to the top 25% in per-capita seizures of illegal opioids in 2007—the earliest year for which seizure data are available. As shown in the last column of Table 7, the estimated effects are indeed stronger in states with thick markets.[22] This finding highlights the importance of supply-side conditions: when illicit opioids are more readily available, social network connections are more likely to translate into tangible spillovers.

Taken together, these results suggest that both demand- and supply-side factors interact with social networks to shape the observed

[21] We use county-to-county interstate connections provided by Wilson (2022). Alternatively, one could also look at the distance to airports. However, according to the Bureau of Transportation Statistics, the overwhelming majority of all long-distance trips are by ground transportation in personal vehicles (90%) or buses (2%). Moreover, security screenings at airports further reduce the likelihood that drugs are transported via plane.

[22] We do not find significant differences when using a median split, suggesting that only the most supply-intense environments meaningfully amplify peer effects.

Table 7
Friendship network effect: Potential mechanisms.

	Opioid Presc.	Total Opioids (T40.1-T40.4)	Total Opioids (T40.1-T40.4)	Total Opioids (T40.1-T40.4)
	(1)	(2)	(3)	(4)
PDMP NetExposure	−0.003	0.673*	0.461	0.285
	(0.004)	(0.343)	(0.407)	(0.499)
PDMP NetExposure × High Prescriptions 2006		1.075***		
		(0.290)		
PDMP NetExposure × High Interstate			0.424**	
			(0.195)	
PDMP NetExposure × Thick Markets				1.343*
				(0.688)
Dependent Mean	0.74	7.79	7.79	7.79
State × Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	35,900	52,858	52,858	52,858
R^2	0.956	0.764	0.759	0.760

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) is opioid prescriptions per capita, while in all other specifications we examine total opioid overdose deaths (T40.1-T40.4) per 100,000 residents. High Prescriptions 2006 is a dummy that is equal to one if a county has above-median opioid prescriptions in 2006. High Interstate is an indicator that is equal to one if its number of county-to-county interstate connections is above median. Thick Markets is an indicator that is equal to one if a county is located in a state with top 25% seizures of illegal fentanyl, heroin, or hydrocodone in 2007. We retrieve data on law enforcement seizures from the NFLIS program. 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. Moreover, we control for geographical distance to PDMP-implementing states. 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 in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 8
Cross-sectional heterogeneity by local friendship network characteristics.

	High Within-County Connectedness	High Network Density	High Civic Culture	High Economic Connectedness
	(1)	(2)	(3)	(4)
DV: Total Opioid Overdoses (T40.1-T40.4)				
PDMP NetExposure	0.797**	0.795*	0.947**	1.141***
	(0.394)	(0.401)	(0.419)	(0.411)
PDMP NetExposure × Variable	−0.970***	−1.454***	−0.175	−0.941**
	(0.355)	(0.318)	(0.380)	(0.414)
DV: Heroin Overdoses (T40.1)				
PDMP NetExposure	0.491***	0.485***	0.579***	0.611***
	(0.172)	(0.171)	(0.176)	(0.179)
PDMP NetExposure × Variable	−0.245	−0.467***	−0.101	−0.253*
	(0.155)	(0.141)	(0.147)	(0.146)
DV: Heroin + Fentanyl Overdoses (T40.1 + T40.4)				
PDMP NetExposure	0.813**	0.838**	0.997**	1.180***
	(0.353)	(0.358)	(0.378)	(0.371)
PDMP NetExposure × Variable	−1.036***	−1.407***	−0.147	−0.878*
	(0.304)	(0.252)	(0.365)	(0.449)

Notes: The table documents the cross-sectional heterogeneity of the friendship network effect for the out-of-state PDMP introductions. PDMP NetExposure measures the degree to which a county's social network is exposed to out-of-state PDMP introductions. PDMP NetExposure × Variable shows the cross-sectional variation of the baseline effect with respect to the column variables. A detailed description of the interaction variables can be found in Table K.1 of Online Appendix K. 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. Moreover, we control for geographical distance to PDMP-implementing states. 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 in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

spillovers. On the demand side, pre-existing exposure to opioids increases susceptibility to peer influences, while on the supply side, thicker drug markets enable information flows to translate into actual exchanges.

Protective peer effects. In our baseline findings, friendship network exposure to PDMPs is associated with higher opioid overdose deaths. At the same time, however, one might intuitively expect a strong friendship network to make individuals less vulnerable to opioid abuse. It is important to note that the effects we document are driven by the *structure* of geographically distant friendship links and not by the friendship intensities per se. Thus, we now complement our baseline analyses and examine protective peer effects by studying whether characteristics of local within-county friendship links affect our results.

We first hypothesize that the influence of shocks to illicit drug consumption coming from *non-local* friendship networks would be lower in counties where *local* friendship networks are stronger. Apart from the strength of local networks, the characteristics of within-county friendship links might also affect our baseline results. In recent contributions, Chetty et al. (2022a,b) examine various characteristics of local (within-county) friendship networks, including, among others, network density, civic engagement, and economic connectedness as proxies for local social capital. For example, they show that economic connectedness,

defined as the share of high-socioeconomic-status (SES) friends among individuals with low SES, is a strong predictor of upward income mobility. Thus, if many within-county friendship links bridge socioeconomic groups, individuals may be less vulnerable to substance abuse because they have better economic prospects, *irrespective* of their out-of-county friendship network exposure to illicit drug usage. Likewise, if social cohesion or civic engagement is high within a county, friendship relationships within the county might also be more intense, making it less likely that outside friends affect beliefs and behaviors.

Following this logic, we use within-county connections from the SCI dataset and data provided by Chetty et al. (2022a) to study four different dimensions of within-county friendship networks: (1) within-county connectedness, (2) network density, (3) civic culture, and (4) economic connectedness.[23] Specifically, we interact our network exposure variable with dummies indicating above-median values for the respective proxy. We focus on death rates related to those drug categories for which we expected and did find strong spillover effects, i.e., total opioid overdoses, heroin overdoses, and combined heroin and fentanyl overdoses. Table 8 presents the results.

We find that high within-county connectedness mitigates the friendship network effect. With the exception of the high civic culture proxy for local social capital in column (3), we almost always find statistically significant negative interaction terms. This finding is consistent with high social capital (as proxied by characteristics of the local network) protecting counties from the adverse effect of being exposed to positive shocks to out-of-state drug consumption due to the introduction of PDMPs. The economic magnitudes are meaningful: the interaction terms are most of the time similar in magnitude but of opposite sign to the baseline effect of PDMP network exposure, suggesting that counties with above-median values with respect to the respective social capital dimension (with the exception of civic culture) are completely protected from the negative external influences we document above. Overall, we find evidence in favor of the view that counties with high within-county connectedness and more cohesive local friendship networks experience muted increases in overdose deaths compared to other counties. One implication of our findings is that counties at risk could try to promote economic and social cohesion to mitigate, among other things, negative spillovers from friendship network exposure to illegal drug consumption elsewhere.

6. Conclusion

Over the last 20 years, the U.S. has experienced a pronounced opioid epidemic, but the overall dynamics of this crisis are still not well understood. In this paper, we analyze the role of social connections in shaping the spatial dynamics of this epidemic. We first show that counties with more social ties to areas severely affected by the opioid epidemic have higher overdose deaths in subsequent years. To address identification concerns, we employ spatial shocks to illegal drug consumption and then study how these shocks propagate through friendship networks to distant counties. Having more connections to counties in states with must-access PDMPs results in higher overdose death rates, especially from heroin and fentanyl. We also find that friendship networks affect proxies for earlier stages of addiction and mental health outcomes.

Our analysis of potential mechanisms highlights that spillovers are stronger in counties with high baseline opioid prescriptions, dense interstate connections, and “thick markets” for illicit opioids, suggesting that both demand- and supply-side conditions interact with social networks. Finally, we show that local social capital, as measured by the density and structure of within-county social networks, attenuates these spillovers, also suggesting a bright side to social connections in this context.

In summary, we provide novel evidence on the effect of friendship networks on the spread of the opioid epidemic.

[23] Within-county connectedness is the ratio of within-county connections to out-of-county connections. Network density is the average fraction of an individual’s friend pairs who are also friends with each other. Civic culture is measured by the fraction of Facebook users who are members of activist groups on Facebook. Economic connectedness is the share of high-SES friends among individuals with low SES. A technical description of these variables can be found in Table K.1 of Online Appendix K.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data for this article can be found online at doi:10.1016/j.jpubeco.2026.105739.

Data availability

All data used in this study are generally publicly available (subject to data use restrictions of the CDC) and we will be able to provide code that will allow researchers to replicate our analysis.

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