Punishing Pill Pushers: Effects of Physician Discipline on Local Opioid Prescribing and Mortality

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To combat the ongoing opioid epidemic, the United States Drug Enforcement Agency punishes physicians that violate the Controlled Substances Act (CSA) by prescribing opioids without legitimate medical reason. Recent work on the opioid crisis finds that prescription opioid distribution and opioid-related mortality are strongly positively correlated, but research analyzing the direct effects of CSA enforcement is limited. Using court-acquired microdata on the distribution of prescription opioids, I estimate that a federal investigation of prescribers in Mingo County, West Virginia decreased opioid prescribing by at least 30%. Next, I find that this supply shock decreased long-run opioid mortality in West Virginia and did not initiate unintended substitution towards heroin or fentanyl. Leveraging a self-created dataset of opioid-related physician arrests, I also show that more-general types of disciplinary action reduced opioid pills prescribed by approximately 20%. The discipline effect grows as post-arrest time elapses; however, the average effect on prescribing declines to zero for counties that experienced arrests after 2010, suggesting that CSA enforcement effects were stronger during the prescription opioid wave of the epidemic. Altogether, these findings suggest that early enforcement of the CSA was effective in reducing overprescribing and likely led to declines in opioid-related harms such as mortality without resulting in any unintended consequences seen in studies of other supply-side opioid policies.

I. Introduction

In the last two decades, opioid-related misuse has increased to crisis-levels in the United States. Survey estimates from the National Survey on Drug Use and Health indicate that nearly 11.5 million Americans misused prescription opioids in 2016 alone and that 2.1 million Americans were diagnosed with an opioid use disorder (SAMHSA 2017). Similarly, the Center for Disease Control (CDC) reports that opioid-related deaths have increased by approximately 400% since 2000 to nearly 72,000 deaths in 2019. During this time period, policy efforts to curb the crisis have had mixed success as opioid use and opioid mortality continued to climb. Further, predictions and early data suggest that opioid mortality has again increased in 2020 due to increased anxiety, isolation, and financial hardship stemming from the COVID-19 pandemic (Abramson 2021; "AMA Advocacy Resource Center" 2021). Given the unprecedented duration and complexity of the epidemic, it is imperative that we understand the effects of policies that are designed to solve the current crisis. One way in which state governments have tackled the crisis is through disciplining healthcare professionals – frequently referred to as "pill pushers"[1] – who are violating the Controlled Substances Act (CSA) by prescribing opioids without legitimate medical purpose.[2]

This study focuses on the effects of such physician discipline – in the most extreme case of legal action coupled with medical license revocation – on local opioid dispensing and, when possible, opioid mortality. My analysis is partly motivated by the call for researchers to study "the identification and investigation of high-risk providers" by Patrick et al. (2016). Further, my results allow us to better understand

[1]A Lexis Nexis search of the joint terms "pill pusher" and "opioid" presents over 100 results of news articles or court briefings applying the phrase "pill pusher" to describe physicians punished for illegally prescribing opioids. One specific example is a 2008 lawsuit alleging that two doctors unlawfully prescribed OxyContin without legitimate medical reason, thereby violating the CSA ("United States v. Chube"). The briefing states that "the doctors were not using their medical licenses [as a way to] treat patients with true complaints…, but as profiteering pill pushers".

[2]Prescription opioids such as oxycodone and hydrocodone are Schedule II substances under the CSA. A Schedule II substance is defined as a drug currently accepted for medical use that may lead to severe psychological or physical dependence if abused.

both the effects of drug diversion enforcement and the broader causal pathway between state supply-side policies and reduced opioid use. Specifically, I leverage detailed arrest reports from the Drug Enforcement Agency (DEA) to indicate the counties in which opioid-related physician arrests have taken place. By considering these counties as treated in the period following the arrest and exploiting variation in the time and place of these physician discipline cases, I can estimate the effect of enforcement on the distribution of prescription opioids. While past research has examined the effects of opioid policies associated with drug enforcement and nonmedical prescribing, I estimate the direct effects of the DEA's enforcement of the CSA using investigations and arrests as proxies for disciplinary action.

To begin, I analyze the effects of the DEA's investigation of a pill mill in Mingo County, West Virginia.[3] I find that the number of oxycodone and hydrocodone pills distributed per capita fell as soon as the DEA investigation began in early 2009. Further, once a subsequent raid occurred and physicians at the pill mill was no longer able to prescribe opioids, the prescription opioid supply maintained this lower level. Overall, I estimate that the investigation caused monthly prescribing levels of hydrocodone and oxycodone to fall by at least 30-60%. I also show that the count of opioid transactions fell by a lesser 10-30%, suggesting that the primary factor driving the decrease in pills per capita was fewer high-quantity transactions. Based on anecdotes indicating that pills shipped to Mingo County spread throughout West Virginia, I estimate the effect of the DEA investigation supply shock on state-level opioid mortality in West Virginia. Results suggest that the investigation caused opioid mortality to fall three years following the start of the investigation and the reduction in opioid mortality has persisted since then. An established concern with supply-side prescription opioid policies is the potential for dependent users to switch to illicit opioids. I find no evidence of unintended substitution and in fact show that both heroin and fentanyl mortality fell after the DEA investigation. Altogether, my findings are

[3] "Pill mill" is used to describe a medical practice that is prescribing or dispensing controlled substances inappropriately (Rigg et al. 2010).

consistent with research analyzing the effects of opioid policy on the supply of opioids; however, my results differ from recent literature on substitution effects of supply-side opioid policy. Instead, my results support survey research reporting that only 3.6% of dependent prescription opioid users initiate heroin use (Muhuri et al. 2013).

Next, I utilize a web-scraped dataset created from DEA reports to indicate the timing of opioid-related physician arrests across counties in the United States. I address relevant concerns recently raised by econometricians to properly estimate the average effects of physician discipline on local opioid prescribing using this staggered treatment adoption design and consider the potential for time-varying heterogenous effects. I find that physician discipline decreased monthly pills per capita by approximately 20%. Interestingly, event studies show the effect grew as more time elapsed since the initial disciplinary action; however, the effect is heterogenous with respect to calendar time. For arrests that occurred after 2010, the average discipline effect was essentially zero while the average effects for arrests occurring in earlier years were clearly negative and comparatively larger.

My work extends and builds upon the current literature and ultimately provides more evidence that supply-side policies do affect prescribing levels and the local opioid supply. In particular, the results from the Mingo County case study suggest that physician discipline is effective in drastically reducing the quantity of opioids prescribed from high-quantity prescribers and that this decrease in prescription opioid supply may reduce addiction rates and decrease overall opioid mortality. Further, my analysis using DEA reports as treatments also demonstrates that physician discipline by the DEA reduced opioid prescribing in the physician's practicing county. While further research is necessary to tie these DEA disciplinary actions to changes in county mortality rates due to severe data suppression in the public-access mortality data, the results from the Mingo County case study and earlier literature suggest that these instances of physician discipline may have also decreased the local opioid mortality rate (Bohnert et al. 2011; Pacula and Powell 2018).

Opioid overprescribing was an early cause of the opioid crisis and continues to be an issue today. As such, my work provides important insight into the immediate and longer-term effects of government-led disciplinary actions that enforce protections against nonmedical opioid prescribing. I also contribute a new dataset that may be used in future analyses to more closely study the effects of opioid-related physician arrests on other opioid-related harms or black market drug activity.

The remainder of the paper proceeds as follows: Section II provides background on the history of the opioid crisis and reviews research on the effects of similar supply-side policies; Section III describes the data; Section IV outlines the difference-in-differences and event study frameworks for both the Mingo County case study and the broader national-level analysis; Section V details the effects of the DEA investigation in Mingo County; Section VI details results for the national analysis; and lastly, Section VII discusses and concludes.

II. Background

Overview of the United States Opioid Crisis and Related Policies

The ongoing opioid crisis is understood to have occurred in three waves. The first wave began in the mid-1990s as doctors began aggressive treatment of pain with opioids following the introduction of Purdue Pharma's opioid painkiller, OxyContin (Jones et al. 2018). The release of OxyContin was coupled with aggressive marketing from Purdue Pharma, which has been cited as a key cause of the crisis (Alpert et al. 2018; Van Zee 2009). In addition to spurring demand through direct marketing, Purdue Pharma and other opioid manufacturers urged healthcare providers to commit to treating chronic pain as concerns spread regarding the potential under-treatment of pain (Melzack 1990; WHO 1986). In response, the American Pain Society – partly funded by opioid manufacturers (McGreal 2018) – launched the "fifth vital sign" campaign and the non-profit Joint Commission on Accreditation of Healthcare Organizations responded by substantially revising its guidelines on pain management in 2001. After a long period of reluctancy to prescribe addictive opioids for pain management (Macy 2018; Paice et al. 1998), this change effectively encouraged healthcare providers to prioritize pain assessment and treat chronic pain symptoms medically – including through prescription opioids (Phillips et al. 2017). These factors establish the vital role that opioid prescribing played in the early stages of the crisis and support findings that opioid dispensing and opioid mortality rose in parallel through 2013 (Bohnert et al. 2011; Pacula and Powell 2018).

The second wave of the crisis occurred between 2010 and 2013 and was characterized by exponential growth in the use of heroin and subsequent heroin-related opioid mortality. The transition from growth in prescription opioid mortality to growth in heroin mortality has been traced to a reformulation of OxyContin that had the unintended consequence of causing dependent opioid users to search for non-prescription substitutes (Alpert et al. 2018; Evans et al. 2019). Other research has shown similar, but smaller, substitution effects following the passage of state prescription drug monitoring programs (PDMPs) (Grecu et al. 2019; Mallatt 2019). PDMPs allow physicians and pharmacists to enter information into state databases when patients are prescribed controlled substances, such as opioids, and give these providers the ability to view patient's prescription history. Participation is voluntary in some states and research suggests that only state policies with mandatory-access provisions, requiring prescribers to always check the state database prior to prescribing controlled substances, are effective in reducing prescription opioid misuse (Buchmueller and Carey 2018). More recently, Kaestner and Ziedan (2020) estimate that pain management clinic laws (PMCLs) – legislation aimed at preventing or disbanding pill mills where medical professionals knowingly prescribe prescription opioids for direct nonmedical use or diversion on the black market – reduced opioid sales between 15-50%.

The third wave of the crisis is ongoing and associated with a dramatic increase in the use of synthetic opioids such as fentanyl. This is due to an increase in the supply of illicit fentanyl (DEA 2016). As with the transition from the first wave to the second wave, some research suggests that the rise in synthetic opioid mortality is at least partly due to substitution from prescription opioids to fentanyl (Evans et al. 2019). According to the DEA, fentanyl poses a higher risk for overdose; however, not all dependent prescription opioid users will substitute to illicit drugs. So, the effect of these supply-side policies on overall opioid mortality is theoretically ambiguous. The study of state-level laws introduced during the first-half of the opioid crisis have empirically established cases of substitution towards heroin and fentanyl following supply shocks that decrease the availability of prescription opioids for nonmedical use.

In addition to supply factors, Case and Deaton (2015) suggest that demand factors also contributed to “deaths of despair” more generally.[4] Economic conditions worsened for less-educated workers in rural areas and employment opportunities decreased. As such, deaths of despair increased most for non-Hispanic white males aged 25 to 55 without a college education. These findings are supported by research drawing an association between declines in manufacturing employment and increased opioid mortality (Charles et al. 2019) or increased unemployment and increased opioid mortality (Hollingsworth et al. 2017). However, Currie et al. (2018) find no significant relationship between county-level employment and opioid use. To this point, Maclean et al. (2020) suggest that supply-side factors were the main cause of the opioid crisis while demand-side factors alter who was most impacted by the crisis. Given strong evidence of causal effects from supply-side policies and limited evidence from demand-side studies, this interpretation seems most plausible in explaining the complexities of the crisis.

The Role of Prescribing

Medical and legal researchers estimate that approximately 20-25% of prescription opioids in the United States are used nonmedically (Phillips et al. 2017). The DEA reports that 12 to 15 billion opioid pills are distributed annually, which

[4]Case and Deaton (2015) define “deaths of despair” as deaths from drug overdoses (the majority of which involved opioids), suicides, and chronic liver disease. Drug overdoses make up the majority of these deaths of despair.

suggests that 2.4 to 3.75 billion pills are diverted from legitimate prescriptions for nonmedical use when applying the Phillips et al. (2017) estimate.[5] Many of these diverted pills, which are either directly obtained by dependent users or sold to dependent users, come from overprescribers. Overprescribers can be grouped into three categories: those that "inadvertently overprescribe", "qualitatively overprescribe", or "corruptly overprescribe" prescription opioids.[6] All three of these groups pose significant issues given opioids' addictive nature. In fact, ProPublica reported that of Medicare's top 20 OxyContin prescribers for 2010, 12 prescribers faced legal or disciplinary action due to their dangerous overprescribing practices ("Prescriber Checkup" 2010). In summary, some prescribers intentionally or unintentionally increase the supply of opioids available for abuse through overprescribing.

Pacula and Powell (2018) highlight the role which prescribing practices played in the opioid crisis. They show that high disbursements of prescription opioids were associated with increased opioid mortality. In order to curb excess prescribing, states implemented PDMPs and PMCLs. Specifically, PDMPs often required physicians to access patient history in order to remind physicians to only prescribe controlled substances when medically necessary. Similarly, PMCLs increase oversight of pain management clinics that were often go-to spots for dependent opioid users seeking prescription opioids. If prescribers continue to overprescribe opioids, actions are taken by local law enforcement, state medical boards, and the DEA to discipline these

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[5]The DEA itself reports that only 11 million dosage units are lost to diversion (DEA 2016). This figure is a large underestimate of diversion because only losses in the supply chain (e.g., prescriptions lost due to pharmacy robberies or losses in transit) are included. As such, these numbers do not account for opioids obtained from legal prescriptions that are either used by the recipient for nonmedical purposes or sold by the recipient on the black market to dependent opioid users.

[6]The terminology between types of overprescribing is quite vague. For the purposes of this paper, I adopt the terminology offered by Dineen (2018). "Inadvertent overprescribing" is when a prescriber prescribes opioids to a patient who feigns pain with plans to either use the drugs nonmedically or sell the obtained opioids on the black market. "Corrupt overprescribing" is defined as purposeful abandonment of proper prescribing practices for profit. "Qualitative overprescribing" is prescribing opioids without sufficient medical evidence or in situations in which there is indication of nonmedical use or diversion. The CSA addresses the widespread issues of corrupt and qualitative overprescribing.

prescribers. When caught, these prescribers are frequently arrested for violating the CSA and typically lose their medical licenses. In such cases, physician discipline is expected to decrease the opioid supply by directly incapacitating the offending overprescriber and potentially deterring other physicians from overprescribing.

The Case of Mingo County

Perhaps the most well-documented case of excessive opioid prescribing is in Mingo County, West Virginia. West Virginia has consistently had among the highest rates of opioid-related mortality throughout the duration of the opioid crisis. While the general population of West Virginia fits the Case and Deaton description of individuals most at risk of deaths of despair – largely non-Hispanic white, non-college educated, and significant levels of male unemployment due to manufacturing plant closures–, some towns in West Virginia also had a serious problem with physicians overprescribing prescription opioids. In fact, dependent users from across the state travelled to Williamson and Kermit – towns in Mingo – to obtain prescriptions for prescription opioids which they filled at nearby pharmacies.[7] These towns each had a population of no more than 4,000, but were shipped nearly two million opioid pills per year prior to a DEA investigation in 2009 and subsequent raid in March 2010 (Snyder 2018).

The circumstances of the investigation are well-documented because of the multiple legal cases involving the offending prescribers, pharmacies, and opioid distributors. In fact, various media outlets did a series of reports showcasing the unusual levels of prescribing in Mingo County[8]. According a Department of Justice press release, Mountain Medical Care Center in Mingo County was "a notorious pill mill" home to many physicians who fit the description of an overprescriber (DOJ

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[7]Several personal stories are shared in court documents related to the investigation of Mountain Medical Care Center. Federal investigators referred to the medical practice as a "pill mill" and one woman describes that she made a three-hour trip from elsewhere in West Virginia to "simply [provide] cash in exchange for prescriptions" (Johnson 2010).

[8]The Washington Post leverages their court-acquired data to describe prescribing practices across the United States – the reporting explicitly mentions Mingo County as one of the counties with the highest number of opioids per capita (Higham et al. 2019).

2013). Physicians at the practice tacitly worked with Tug Valley Pharmacy, Strosnider Pharmacy, and Hurley Pharmacy who filled the prescribers' prescriptions despite the staggering number of opioid prescriptions requested. According to court documents, hundreds of patients from across the state waited outside of the medical practice and nearby pharmacies to obtain prescription opioids. Opioid prescriptions in the county rose dramatically, from already-high levels compared to the rest of the United States, from 2006 through 2008. Eventually, the DEA noticed the unusual prescribing patterns and began to investigate the parties involved which led to a raid of the Mountain Medical Care Center office in March 2010 and the subsequent revocation of offending prescribers' medical licenses and arrest.

I choose Mingo County as a prime example of an area which had a significant overprescribing problem in order to then estimate the effect of physician discipline. Because much of the information regarding the timing events is public knowledge due to the high-profile nature of the litigation and local news reporting, Mingo County is a perfect case study for estimating the effects of physician discipline.

III. Data Description

Following the literature studying the effects of various opioid policies and the relationship between prescribing and future mortality levels, I mainly use detailed DEA Automation of Reports and Consolidated Order Systems (ARCOS) data obtained from the Washington Post as a proxy for opioid prescribing levels and opioid-related harms. When possible, I also use mortality data from the public-access CDC Wide-ranging Online Data for Epidemiologic Research (WONDER) system. Data on opioid-related physician arrests is created by web-scraping the Drug Enforcement Agency's (DEA) *Cases Against Doctors* reports.[9] Lastly, I supplement these data with location-

[9] Hereafter, "opioid-related physician arrests" and "physician arrests" will be used interchangeably.

and time-varying demographic and socioeconomic controls from various government datasets as well as state-level policies from a variety of sources.

Washington Post-Acquired ARCOS Data

I use detailed monthly and yearly county-level data on the distribution of prescription opioids between 2006 and 2014 as a proxy for prescribing behavior.[10] The Washington Post gained access to the ARCOS microdata following a legal battle.[11] The DEA publicly provides quarterly ARCOS data on opioid shipments at the 3-Zip level; however, the 2006-2014 Washington Post ARCOS data is more detailed because the unfiltered microdata can be aggregated to the county-month level. Given that physician arrests are local events – likely with small, local effects –, I leverage the county-level data processed by the Washington Post to more precisely measure the effect of physician discipline on opioid shipments in the immediate surrounding area.

Specifically, I use data both on the count of opioid-related transactions and the summed count of hydrocodone and oxycodone pills distributed.[12] The transactions variable represents the count of shipments from opioid distributors to pharmacies and the pills distributed serves as a proxy for opioid prescribing levels. To create a per capita measure of the quantity of pills, I merge this county-month data to county-year population data. This is required because counties with larger populations will naturally have more opioids prescribed in the county – calculating the per capita distribution of opioids is a way to account for this and is common in the opioids research literature.[13]

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[10]Many researchers have taken this approach when studying the effects of supply-side opioid policies on local rates of opioid prescribing (Alpert et al. 2018; Mallatt 2018).

[11]The Washington Post utilized the raw ARCOS microdata for a series of investigative reports. The cleaned data used for reporting was made publicly available by the Washington Post.

[12]While hydrocodone and oxycodone do not encompass all prescription opioids, these drugs represent 75% of prescription opioids (Higham et al. 2019). As such, I use this county-level, monthly data as opposed to publicly available quarterly ARCOS data at the 3-Zip-level that does include shipment data for other, less abused prescription opioids (e.g., methadone, codeine, etc.).

[13]I do not use morphine milligrams equivalent (MME) per capita, as is done by a variety of other researchers who also use opioid shipment data (Alpert et al. 2018; Evans et al. 2019). Because the Washington Post-acquired dataset only contains data on the combined shipments of oxycodone and hydrocodone, I cannot identify the type of pill to apply the MME conversion factors. However, given

Mortality Data

Yearly data on opioid-related mortality at the state-level is publicly available from the CDC WONDER system. Following the literature, I use mortality rates per 100,000 rather than the state-level mortality counts (Schuler et al. 2020). I focus on ICD-10 external cause of injury codes related to opioids (X40-X44, X60-X64, X85, Y10-Y14) and drug identification codes T40.0-T40.4, and T40.6 for years 2006-2014.[14] In doing so, I create three different mortality-related variables: mortality rates from all opioids (T40.0-T40.4, T40.6), mortality rates from prescription opioids (T40.2-T40.4), and mortality rates from illicit opioids (T40.1, T40.4). I include the T40.4 code – death from synthetic opioid analgesics besides methadone – under both prescription opioid mortality and illicit opioid mortality because fentanyl is a synthetic prescription opioid that has more-recently been illicitly produced for abuse. Due to data suppression for the cause-specific opioid mortality rates, I also used unsuppressed mortality data directly from West Virginia's Department of Health and Human Resources when the CDC WONDER data was suppressed (West Virginia Department of Health 2015).[15]

I do not use opioid mortality as an outcome for county-level analyses in Section VI due to significant data suppression. Over 85% of county-years had fewer than 10 opioid deaths – in these cases, the count is suppressed in the public-use CDC WONDER data. While processes exist to impute values for suppressed cases (Tiwari et al. 2018), there is far too much suppression for such methods to credibly represent the true counts of death.

that oxycodone and hydrocodone have nearly the same MME – 1 milligram and 1.5 milligrams respectively –, this is unlikely to make a significant difference in the interpretation of results.

[14]Given that I only have access to 2006-2014 county-level shipments data from the Washington Post source, I only use mortality data within this timeframe as well.

[15]These data are contained in Table 1 of the cited source. I sum the rows associated with each relevant group of drug identification codes to create the count of prescription opioid deaths and illicit opioid deaths when these categories were occasionally suppressed from the CDC WONDER data.

Incidence of Physician Arrests: Cases Against Doctors

Instances of opioid-related physician arrests from 2006-2014 are collected using the DEA *Cases Against Doctors* webpage.[16] The *Cases Against Doctors* webpage is regularly updated by the DEA Diversion Control Division and reports all actions taken by the DEA against registered prescribers. For example, consider the common case of a prescriber that was the target of an undercover operation and prescribed opioids without a legitimate medical reason – a typical report would include a summary of the operation and state if the prescriber lost their license to prescribe controlled substances. Notably, this excludes any actions taken by state-level or local agencies which are not shared with the DEA. This presumably would not happen often, if at all, given that these prescribers are still in violation of the CSA that is enforced by the DEA in addition to any other state or local violations.

As such, a large portion of these reports explicitly claim that prescribers violated the CSA by prescribing opioids without medical reason.[17] Because the reports provide case details, I am able to identify whether a report involves a prescriber that was found to be overprescribing opioids by web-scraping each report and indicating if particular phrases commonly related to opioid overprescribing are contained within the body of the report. Appendix Table A1 shows the phrases I use to identify if a report is related to nonmedical opioid prescribing. If a report contained one of these phrases, I deemed the case related to the nonlegitimate prescribing of opioids. Since each report is dated, I can also identify the time at which the disciplinary action took place. For cases in the same county that occur at different points in time, I apply the

[16]There are two webpages containing DEA *Cases Against Doctors* reports – one presents cases in table-format and was last updated in 2018 and the other is in report-format and was last updated in 2020. I utilize both pages to get the most-complete representation of DEA cases. Most cases are in both sources, but a few are only be in a single source. Because I already summarize the information to create county-level treatments, there are is no concern of double-counting the same case twice. I collect data from 2006-2018, but only use apply the data through 2014 given that the ARCOS data is only available for 2006-2014.

[17]In my web-scraping analysis, I find that the most commonly cited law in these reports is 21 U.S.C. Sec. 823(f) – a subsection of the Controlled Substances Act which prohibits the sale of controlled substances for nonmedical purposes. Nearly 50% of the DEA case reports included phrases related to opioids, which are defined in Appendix Table A1.

earliest case as the time of treatment. Using these self-collected data, I create a county-level, monthly panel dataset which indicates if a county has experienced an opioid-related physician arrest at each month throughout 2006-2014.

Controls

I create a set of demographic and socioeconomic county- and time-varying controls using data from the Surveillance, Epidemiology, and End Results (SEER) Program, Bureau of Labor Statistics, and the Census. I obtain data on county-level population from SEER to create variables for the following: total population; percent of population that is non-Hispanic white; percent of population that is male; and percent of population that is 15-29, 30-49, 50-64, and 65 or older. Additionally, I use unemployment rates from the BLS and annual median household income from the Census Small Area Income and Poverty Estimates. For the state-level analysis of mortality in Section V, I also obtain the state-level demographic and socioeconomic variables.

Lastly, in line with prior work and the recommendations of Schuler et al. (2020),[18] I apply additional policy controls for relevant state-level laws that are associated with the opioid crisis during the 2006-2014 period: the presence of mandatory-access PDMPs which require prescribers to check the state prescription drug monitoring database prior to writing any prescriptions for controlled substances and the presence of PMCLs which regulate medical practices that provide pain management services.[19] Data on must-access PDMPs are obtained from a RAND Opioid Policy Tools and

[18]Schuler et al. (2020) explain the need for more rigorous methods in the opioid policy field. The authors note that a strength of many studies is the inclusion of co-occurring policies. As such, they suggest including controls for confounding opioid policies when estimating the effect of other opioid-related interventions. I choose to include controls for PDMPs and PMCLs (also referred to as "pill mill laws") because these policies are most likely to be related with measures of opioid prescribing. Alpert et al. (2018) and Evans et al. (2019) also apply these policies as controls in their analyses of supply-side opioid policies.

[19]I specifically use must-access PDMP implementation as the control rather than general PDMP implementation due to recent work by Buchmueller and Carey (2018) which estimated that PDMPs only had a significant effect on prescribing and mortality when the provisions legally required prescribers to access the prescription drug monitoring database prior to each opioid prescription.

Information Center database and data on PMCLs are obtained from the Prescription Drug Abuse Policy System. Covariate balance tables are included in Tables A2 and A3.

IV. Empirical Methodology

I apply difference-in-differences models and event studies in order to estimate the effects of physician discipline. Both are frequently used in the opioid policy analysis literature (Schuler et al. 2020).

The basic difference-in-differences model is defined as follows:

$$Y_{ist} = \alpha(DISCIPLINE_{ist}) + \boldsymbol{\beta X_{ist}} + \boldsymbol{\gamma}(\boldsymbol{POLICIES_{st}}) + \phi_{is} + \tau_t + \epsilon_{ist}, \text{ (Eq. 1)}$$

Where Y_{ist} is an opioid-related outcome (pills per capita or number of opioid purchasing transactions) at the county-level *i* in a state *s* at month-year *t*.[20] $DISCIPLINE_{ist}$ is an indicator variable that turns on when there is an instance of DEA discipline (in Section V the measure of discipline is the Mingo investigation; in Section VI the measure of discipline is DEA-reported physician arrests) in county *i* of state *s* at time *t*=T and remains on for all periods *t*>T as well. $\boldsymbol{X_{ist}}$ is a vector of county-level demographic and socioeconomic controls including the following measures: total population, 5-group age distribution, proportion of males, proportion of non-Hispanic Whites, median household income, and the unemployment rate. Similarly, $\boldsymbol{POLICIES_{st}}$ is a set of state-level policy controls including must-access PDMPs and PMCLs. Lastly, ϕ_{is} and τ_t are county- and time-fixed effects and ϵ_{ist} is an error term clustered at the county-level to allow for autocorrelation within clusters. I also consider models incorporating county time trends and population weighting as robustness checks. Additionally, I apply a state-level version of (Eq. 1) in Section V to estimate the effects of the Mingo investigation on state-level opioid mortality.

[20] Throughout the paper, I use both levels and logs of the dependent variable to demonstrate that effects are robust to the selection of functional form.

The difference-in-differences model relies upon the parallel trends assumption. I will verify that this assumption is met using an event study model to check that there are no significant effects in the pre-treatment period. The event study model also provides a way to estimate if the treatment effect is dynamic and changes over time. This is an important point to consider because it is possible for short-term policy effects to differ from medium- and long-term effects (Maclean et al. 2020). The event study model is defined as follows:

$$Y_{ist} = \sum_k \alpha_k (I\{k = t - E_i\}) + \boldsymbol{\beta X_{ist}} + \boldsymbol{\gamma}(\boldsymbol{POLICIES_{st}}) + \phi_{is} + \tau_t + \epsilon_{ist}, \text{ (Eq. 2)}$$

Where E_i indicates the timing of a binary treatment (an instance of physician discipline) in county *i* in state *s*. Now, α_k measures the causal effect of discipline on Y_{ist} at different event times k. Again, I apply a state-level version of (Eq. 2) in Section V to estimate whether effects on state-level mortality in West Virginia changed as more time elapsed since the initial prescription opioid supply shock.

Recent literature suggests that the traditional two-way fixed effects difference-in-differences estimator (TWFE DD), commonly used when treatments are staggered, can introduce bias if treatment effects are also time-varying (Goodman-Bacon 2018; Sun and Abraham 2020). The TWFE DD estimator differs from the "simple 2x2 DD" in which treatment occurs only at one time period. Specifically, Goodman-Bacon (2018) decomposes the TWFE DD estimator to show that it is a weighted average of all possible 2x2 DD estimators and that negative weighting will arise if treatment effects vary over time. In this case, the TWFE DD estimate is typically biased away from the sign of the true treatment effect. As such, caution is necessary when using TWFE DD estimates to summarize time-varying effects. To address this issue, Callaway and Sant'Anna (2020) develop new methods to group estimates into multiple unbiased group-time average treatment effects on the treated (ATT). A key methodological difference is that the Callaway and Sant'Anna method does not use already treated groups as a comparison group because Goodman-Bacon (2018) showed that doing so introduces bias if effects are time-varying. There is a unique group-time ATT for each group of units treated at the same point in time – this allows the researcher to study whether treatment effects are time-varying. These group-time ATTs can be aggregated into a single unbiased estimate which is analogous to the TWFE DD estimate.

My analysis of Mingo County in Section V is an example of a simple 2x2 DD, so there is no concern regarding bias caused by time-varying effects. However, when I utilize staggered treatment adoption in Section VI, I mainly apply the Callaway and Sant'Anna regression method to better identify an overall ATT that can credibly be used for inference and apply the group-time ATTs to understand if effects vary dependent on both event and calendar time of treatment adoption.

V. Case Study: Mingo County, WV

Effects on the Supply of Opioids

I first consider the effects of the DEA investigation in Mingo County, beginning in 2009 with a subsequent raid of the investigated pill mill in March 2010, on the supply of opioids in Mingo County. Figure A1 illustrates the average county-level opioid prescribing in Mingo, the rest of West Virginia, and the rest of the United States during 2006-2014. Notably, prior to the investigation and raid, the per capita supply of opioids in Mingo County was between five and fifteen times higher than that in the average United States county. There is a clear drop in both the monthly supply of opioids and the monthly number of prescriptions following the beginning of the DEA investigation in 2009 and the eventual raid in March 2010. Descriptively, it is evident that opioid pills and transactions significantly decreased throughout the investigation period and then fell again at the time of the raid. Pills and transactions stayed at this new, lower level through 2014. As such, I consider Mingo County to have been treated in the first month of 2009 since this initial act of physician discipline led to the raid which further reduced opioid prescribing.

Employing the difference-in-differences model specified in (Eq. 1) with other counties in West Virginia as the comparison groups shows that the investigation and subsequent physician discipline resulted in a decrease of approximately 6-12 pills per capita each month following the beginning of the investigation. Given that the average pills per capita was 22.64 in Mingo prior to the investigation, the average treatment effect on treated is a decrease in pills per capita of 30-60%. And overtime, this effect equates to between approximately 11.5 million and 23 million fewer pills being distributed in Mingo County in 2009-2014 compared to if the investigation had not occurred.[21] Notably, the parallel pre-trends assumption is not met because there was a noticeable rise in Mingo County's opioid supply between 2006-2009 whereas the supply was relatively stable in the rest of West Virginia. This is evident in Figure A1. In this case, the lack of parallel pre-trends biases the difference-in-differences estimates towards zero. So, we must be careful in interpreting the treatment effect as a true causal effect – instead, the estimated treatment effect provides a lower bound for the magnitude of the true causal effect.

Applying a variety of specifications with and without controls, I first show in columns (1) - (2) of Table 1 that the investigation decreased pills per capita by 50-60% in models with the outcome in levels or logs. These estimates are not overly sensitive to functional form or the inclusion of controls, which further suggests that the estimates are robust. However, including county time trends significantly decreases the magnitude of the effect on PPC. Still, the estimated difference-in-differences effect including county time trends and population weighting is statistically significant and suggests that this particular instance of physician discipline decreased shipped pills per capita by at least 30%. Given that opioid pills per capita dramatically increased during the 2006-2008 pre-investigation period in Mingo County and remained flat in the rest of West Virginia, this estimate represents a lower-bound of the true causal effect.

[21]At an estimated average decrease of 6 pills per capita each month for six years and a population of approximately 26,500, the effect is calculated as 6 x 12 x 6 X 26,500 = 11,448,000 pills. A similar calculation is performed for the estimate of -12 monthly pills per capita.

Table 1: Effect of Mingo Investigation on Opioid Shipments

	Outcome in Levels				Outcome in Logs			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A: Pills Per Capita								
Investigation	-12.33***	-12.29***	-6.994***	-7.044***	-0.965***	-1.006***	-0.400***	-0.397***
	(0.121)	(0.244)	(0.259)	(0.244)	(0.0259)	(0.0460)	(0.0310)	(0.0252)
	[-54.5%]	[-57.36%]	[-30.9%]	[-31.1%]	[-61.9%]	[-63.4%]	[-33.0%]	[-32.8%]
Panel B: Transactions								
Investigation	-143.0***	-108.2***	-69.13***	-99.52***	-0.366***	-0.362***	-0.137***	-0.113***
	(12.75)	(15.98)	(15.50)	(27.72)	(0.0219)	(0.0395)	(0.0264)	(0.0224)
	[-30.1%]	[-22.8%]	[-14.5%]	[-21.0%]	[-30.6%]	[-30.4%]	[-12.8%]	[-10.7%]
Controls	N	Y	Y	Y	N	Y	Y	Y
County Time Trend	N	N	Y	Y	N	N	Y	Y
Weighting	N	N	N	Y	N	N	N	Y

Standard errors clustered at the county-level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

All models include county- and month-year-fixed effects. N=5939: Sample consists of each county in West Virginia, monthly from 2006-2014.

The pre-treatment average PPC and transactions count for the treated county (Mingo) are 22.64 and 474.36 respectively.

The calculated percentage treatment effect based on pre-treatment means is in brackets. For the log-linear models, I calculate ($100 \text{ x } \exp(\alpha) - 1$).

Interestingly, compared to the effect on pills per capita, the effect on the number of monthly shipment transactions is smaller in magnitude across all specifications. For example, column (2) under "Outcome in Logs" – the log-linear specification including controls along with county- and time-fixed effects – shows that the DEA investigation in Mingo County decreased the number of pills per capita shipped by an estimated 63.4% while the count of shipments from distributors only fell by an estimated 30.4%. These effects are consistent with Figure A1 and, as with the pills per capita estimates, the effects are robust across a variety of specifications. Because the log-linear models provide estimates which are more consistent and robust to slight changes in the specification, I will consider log-linear models to be the preferred specification for measuring effects on pills per capita and transactions in Section VI.

Reconciling the larger effects on pills per capita from the smaller effects on the number of transactions, my estimates suggest that the DEA investigation was effective in reducing both opioids prescribed and the frequency of opioid shipments from distributors. The fact that the effect on pills per capita is larger in magnitude than the effect on monthly transactions is not surprising since distributors will always need to make shipments to many buyers, even if a buyer is ordering fewer opioids. As such, the difference in magnitudes could also be explained by select pharmacies ordering high-quantities of prescription opioids pre-investigation drastically reducing their order volume. This is consistent with court reports documenting that a handful of pharmacies filled the vast majority of opioid prescriptions from the doctors at Mountain Medical Care Center who later lost their medical licenses in 2010 (Tug Valley Pharm. v. All Plaintiffs). However, these estimates do not disentangle whether the decrease in pills per capita and transactions are due only to the offending physicians being directly investigated and subsequently punished, other physicians being indirectly deterred from prescribing opioids for nonmedical and/or medical reasons, or some combination of these channels.

In the case of opioid prescribing – which is not necessarily criminal – deterrence may affect two distinct groups of prescribers. Clearly, physician discipline can deter other overprescribers from prescribing opioids without a valid medical purpose due to the fear of also being disciplined. This type of criminal deterrence is often considered in the economics of crime literature: Cameron (1988) surveys dozens of crime papers, with the majority finding a negative relationship between arrest rates and crime, supporting the deterrence hypothesis. But in the case of opioid prescribing, discipline of criminal overprescribers could also make a second group of physicians – who are not overprescribing opioids – less likely to prescribe opioids, even for valid medical reason, out of fear of being disciplined for doing something wrong. In fact, this is a concern also raised by Moyo et al. (2017) in an analysis of the effect of PDMPs on prescribing patterns. Moyo et al. find that PDMPs reduced opioid prescriptions overall and had smaller, but still statistically significant, negative effects on prescriptions for the 65-plus age group who are more likely to have a legitimate medical reason for using prescription opioids. While punishment is meant to deter the first group of physicians from prescribing opioids without medical reason, an unintended and concerning consequence would be if the second group also limited their prescribing of opioids to patients who truly need them. This phenomenon has not been shown empirically, but medical doctors have raised this concern of PDMPs deterring physicians from prescribing opioids when medically necessary (Dowell et al. 2019). These concerns are supported by anecdotal evidence in local news reporting (Aleccia 2016; O’Donnell and Alltucker 2019). Given the importance of this question, I assess the strength of these potential deterrence effects empirically through a study of counties with varying levels of opioid prescribing in Section VI.

Supply Shock's Effects on Opioid Mortality

Reporting suggests that dependent users travelled to Mingo County from across the state of West Virginia to get opioid prescriptions at Mountain Medical Care Center and fill them at nearby pharmacies.[22] As shown in Table 1, the investigation of these medical centers significantly decreased the shipped pills per capita. Given the large reduction in monthly opioid shipments – which is often used as a proxy for opioid supply –, it is possible that the investigation also had an effect on state-level opioid mortality. Past literature on supply-side opioid policies has also estimated the effect on opioid mortality and considerable attention has been paid to estimating the effects on mortality by opioid type given the potential for substitution from prescription opioids to illicit opioids like heroin or fentanyl.[23] Effects of the supply shock could either grow or shrink over time, so I estimate event study models to capture the effect of the initial supply shock resulting from the 2009 investigation over time. To address substitution effects, dynamic treatment effects, and test for pre-existing trends, I estimate event studies for the following causes of opioid mortality: all opioids, prescription opioids, and illicit opioids.[24]

Figure 1 presents the three main event studies using opioid mortality data from 2006-2014.[25] Pre-treatment estimates provide no evidence of a relationship between

[22]According to court documents, patients largely filled these prescriptions at nearby Tug Valley Pharmacy, Strosnider Drug Store, and B&K Pharmacies because patients knew other pharmacies might turn away prescriptions from the physicians at Mountain Medical Care Center (Tug Valley Pharm. V. All Plaintiffs).

[23]Evans et al. (2019) and Alpert et al. (2018) find large substitution effects towards heroin following the reformulation of OxyContin. Grecu et al. (2019) and Mallatt (2019) find smaller, but still significant, substitution effects towards both heroin and synthetic opioids following the passage of state PDMPs.

[24]When using the CDC WONDER data, deaths which are determined to be from both a prescription opioid and an illicit opioid (heroin or fentanyl) will be counted as both a prescription opioid death and an illicit opioid death.

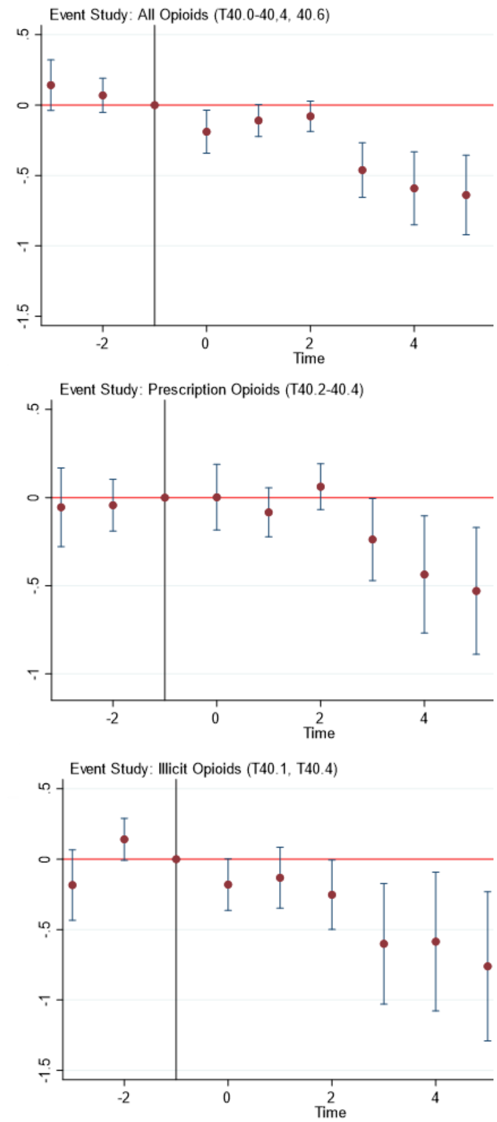
[25]Difference-in-differences estimates across a variety of specifications are also included in Table A4. The majority show a negative relationship between the investigation and opioid mortality.

mortality and the investigation in the pre-investigation period. This indicates that prior to the investigation, there was not already a differential trend in West Virginia compared to the rest of the United States in opioid mortality. So, there is no evidence of an invalid research design based on differing pre-trends. Alternative difference-in-differences estimates across a variety of specifications, including the usage of select comparison states based on geographic similarity and shared socioeconomic factors, are also included in the Appendix and generally support that the investigation had a negative effect on mortality.

The event study effects also follow similar paths with respect to time: the investigation had either very small or statistically insignificant effects on the mortality rate until 3 years after. By 2012 – three years after Mingo County was treated –, the effects on the mortality rate were significant across all opioid definitions and grew slightly over time. Interestingly, the effects on the illicit opioid rate, which includes heroin mortality, are larger than the effects on the prescription opioid rate. Consistent with these cause-specific findings, the effects on all opioids fall somewhere between these effects.

The lagged effect of the supply shock on per capita opioid mortality is consistent with foundational literature on the economics of addiction. Becker and Murphy (1988) develop a model in which rational addicts respond more to price in the long-run than in the short-run. Dependent prescription opioid users effectively faced a price change beginning in 2009 because it became more difficult to obtain prescription opioids due to the dramatic decline in opioid prescribing following the DEA investigation. Users now had to incur additional time and travel costs to find new methods of acquiring prescription opioids, either by directly obtaining a prescription from a new physician who had not been disciplined or searching on the secondhand black market. The Becker and Murphy (1988) model suggests that this price increase will decrease prescription opioid abuse and this effect will grow over time, with the permanent change in price having only a small initial effect on consumption. This theoretical outcome aligns with my results which indicate no statistically significant effect of the supply shock on mortality until 2011.

Figure 1: Dynamic Effects of 2009 DEA Investigation on West Virginia Mortality Rates



Notes: Each event study uses state- and year-fixed effects and state-time trends in addition to the set of controls discussed in the text. The outcome is the log of the per capita opioid mortality rate. Each point estimate is associated with a confidence band depicting the 95% confidence interval based on standard errors clustered at the state-level. The y-axis provides the effect on log(mortality), α_k – the percentage effect at each event time *k* can be computed by $100 \times (\exp(\alpha_k) - 1)$.

Notably, 2011 is also the first full-year after the offending physicians' offices were raided and physicians were officially disciplined by the DEA. Given the substantial diversion of prescription opioids for nonmedical use (Phillips et al. 2017 Powell et al. 2020),[26] it is likely that prescription opioids were readily available on the black market for a few years after the initial supply shock if secondhand sellers stockpiled prescription opioids. Notes from a confidential source in a similar case report following the closure of a pill mill in Florida state that street sellers often "traveled to American Pain [the aforementioned pill mill] in order to obtain controlled substances that were later sold … for $25 per pill" ("Jacobo Dreszer, M.D." 2011).[27] Because it was so easy to obtain prescription opioids in Mingo County (Johnson 2010), it is likely that this type of secondhand selling was also occurred in West Virginia and continued throughout the initial stages of the post-investigation period until secondhand sellers were no longer able to easily obtain prescription opioids and had run out of any stored supply. This descriptive information also supports the finding that prescription opioid mortality did not decline in West Virginia until a few years after the investigation began. After the initial lag period, the effect of the prescription opioid supply shock on opioid mortality rates is persistent through 2014.

Unlike recent analyses on supply-side opioid policies (Alpert et al. 2018; Evans et al. 2019; Powell and Pacula 2021), I find no indication of a substitution effect following the supply shock. In fact,, I find that the investigation and subsequent decrease in the supply of prescription opioids reduced the growth rate of illicit opioid mortality compared to other states beginning in 2011. The illicit opioids category consists of heroin (T40.1) and other non-methadone synthetic opioids (T40.4: e.g., fentanyl and tramadol). It is possible that synthetic opioid mortality – which, prior to the availability of illicit fentanyl that began around 2014, consists of some prescription

[26]Powell et al. (2020) show that opioid-related mortality among younger populations increased when prescription drug benefits were expanded for the elderly. Additionally, Phillips et al. (2017) estimate that 20-25% of prescribed opioids are used nonmedically with a significant portion being sold by the prescription recipient on the black market.

[27]This quote is directly from a 2011 *Cases Against Doctors* report regarding particular physicians who prescribed opioids nonmedically at the American Pain clinic in Florida.

opioids – declined following the supply shock while heroin mortality still increased. This would mask any potential substitution towards heroin. To verify that this is not the case, I estimate the effects of the 2009 Mingo investigation on each of these categories individually in Table 2.[28] Across statistically significant results from OLS and Poisson models, I consistently find that both heroin and synthetic opioid mortality decreased by approximately 20-40% following the negative supply shock. This provides further evidence that there was no substitution from prescription opioids to heroin or synthetic opioids when the supply of prescription opioids significantly decreased in West Virginia. While recent research has highlighted the presence of substitution towards illicit opioids following the adoption of supply-side policies, my results suggest that not all policies limiting prescription opioid access to lead to higher use of illicit opioids.

Table 2: Effect of Mingo Investigation on Illicit Opioid Mortality

	(1)	(2)	(3)	(4)
Panel A: Heroin				
Investigation	-0.0440	-0.203**	-0.366**	-0.368*
	(0.0979)	(0.0993)	(0.186)	(0.201)
Panel B: Synthetic Opioids				
Investigation	-0.197*	-0.292***	-0.378**	-0.403*
	(0.105)	(0.100)	(0.150)	(0.224)
Model	OLS	OLS	Poisson	Poisson
Weighting	N	Y	N	Y

Standard errors clustered at the state-level in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$
All models include controls and state- and year-fixed effects.
N=390: Sample includes all state-years with cause-specific mortality count greater than 9.
OLS models use log(death rate) as the outcome.
Poisson models use death count as the outcome, with state population as the exposure.

Considering these results in context, 2010 marks the beginning of the second wave of the opioid crisis which is characterized by a dramatic increase in heroin deaths. It is possible that this early intervention reduced widespread prescription opioid access and subsequently slowed the onset of new cases of prescription opioid addiction. Fewer cases of prescription opioid addiction would also mean fewer users switching from prescription opioids to heroin (Muhuri et al. 2013). Further, research

[28]I do not apply the event study model to illustrate this point because many states would need to be dropped to keep a balanced panel as suggested in the literature (Borusyak and Jaravel 2017), so event study estimates would be overly noisy due to large standard errors.

suggests that patients treated by doctors who have a higher propensity to prescribe opioids are more likely to be long-term opioid users and become dependent (Barnett et al. 2017). If these doctors are directly disciplined or are deterred and decrease their propensity to prescribe opioids, we would expect fewer dependent prescription opioid users and thereby fewer heroin users and lower opioid mortality overall.

While the inclusion of fixed effects and state time trends in the event studies should allow for unobservable differences across states, we should still be wary of concluding that the Mingo County investigation reduced opioid mortality rates by nearly 50%. Because the only source of variation is the timing of the investigation and there is only one treated state, it is possible that another factor – outside of the DEA investigation in 2009 – led to the decline in West Virginia's opioid mortality rates. Lastly, given the problems with states' opioid mortality reporting (Ruhm 2018), more research using alternative outcomes data is required to further test the validity of this finding.

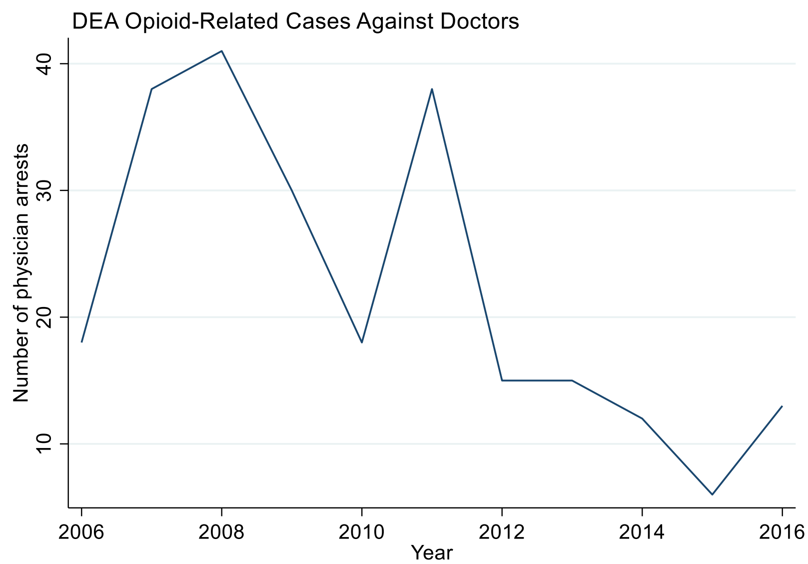
VI. Effects of Physician Arrests Across the United States

The main identification concern raised in Section V is whether the estimated effects are due to the DEA investigation specifically or some other change that occurred in 2009. A common method to address this is issue is exploiting variation across groups of units that receive treatment at different times (Goodman-Bacon 2018) – these studies rely on "staggered adoption" of treatment policies to estimate the average treatment effect. Using cases related to opioids in the *Cases Against Doctors* reports, I estimate the general effect of physician arrests on opioid shipments. As introduced in Section IV, recent literature on staggered treatment adoption designs has shown that the traditional TWFE DD estimator provides an average of treatment effects that is often biased (Goodman-Bacon 2018; Sun and Abraham 2020). To adjust for this bias, I also run models applying the outcome regression method introduced by Callaway and Sant'anna (2020). Given that researchers believe some opioid policies may have heterogenous effects dependent on calendar time,[29] this method also allows me to explore such heterogeneity and provide a framework for opioid policy researchers to more rigorously consider potential heterogeneity in policy treatment effects.

I first present a summary detailing the number of physician arrests per year during the 2006-2014 period of interest in Figure 2 below. The graph shows that physician arrests have significantly fallen beginning in 2012, which seemingly contradicts recent literature on drug enforcement. Specifically, Mulligan (2020) argues that a 2013 memo from Attorney General Eric Holder – which directed federal lawyers to stop prosecuting nonviolent drug crimes – reduced law enforcement efforts and subsequently led to a rise in opioid mortality; however, my analysis of published DEA case reports suggests that a nontrivial decline in physician arrests preempted this memo. While it is possible that the Holder memo reduced other types of drug crime enforcement, I question whether the memo had any effect on the prosecution of physicians that prescribed opioids without legitimate medical reason. The decline coincides with the continued increase in heroin mortality and the emergence of fentanyl, so it is possible that fewer physicians were prescribing opioids nonmedically as dependent users shifted towards nonprescription opioids. It is also possible that DEA enforcement efforts shifted towards the black market rather than focusing on pill pushers as nonprescription opioid usage began to dramatically rise.

[29]For example, Powell et al. (2018) propose that state-level marijuana policies passed prior to the "Ogden memo" may have had a stronger effect on opioid mortality than policies passed after the memo since states' marijuana policies were more restrictive following the memo. Similarly, Mulligan (2020) argues that the "Holder memo" decreased drug enforcement which partly caused the emergence of fentanyl and subsequent rise in synthetic opioid mortality.

Figure 2:



Notes: The graph provides the yearly count of opioid-related physician arrests after web-scraping the DEA *Cases Against Doctors* reports. Essentially this shows how treatments are staggered throughout the time period of analysis. I only consider cases from 2006 through 2014 because the county-level ARCOS data is limited to this time period.

Table 3 presents difference-in-differences estimates for the staggered treatment case using both the traditional difference-in-differences approach and the Callaway and Sant'Anna (2020) aggregated ATT parameter. As described by Callaway and Sant'Anna and briefly in Section IV, the aggregated ATT parameter is a weighted average of parameters estimating the effect of physician arrest for each group-time.[30] I generally find that physician discipline had a negative effect on pills per capita and the number of transactions across both estimators.[31] Even when adjusting for bias, the effect of physician arrests is still negative and significant. In fact, the bias-adjusted Callaway and Sant'Anna (2020) estimates are comparably larger in magnitude which suggests that the TWFE DD estimates likely understate the true causal effect of physician discipline on opioid shipments. Goodman-Bacon (2018) proves that

[30]Callaway and Sant'Anna (2020) define the group-time average treatment effect parameter as the average treatment effect for some group *g* at time *t*, where each group is defined by the time period in which units are first treated. I present a more-detailed explanation of the parameter in the Methodology section.

[31]I am not able to present specifications using county-time trends as done in Table 1 because there are too few treated observations in each month. As such, including county-time trends results in a highly singular covariance matrix with indefinite standard errors, which is not ideal for inference.

difference-in-differences estimates are typically biased away from the sign of the true treatment effect when treatment effects vary over time – this is consistent with my findings and further introduces caution against using a single TWFE DD coefficient to summarize effects that are time-varying. Overall, the bias-adjusted results indicate that counties which experienced a physician arrest had an average of 22.1% fewer shipped opioid pills per capita and 3.5% fewer transactions from distributors following the DEA's disciplinary action. This suggests that CSA enforcement had a sizable effect on prescribing behavior and even resulted in distributors making fewer shipments to these counties.

Table 3: Effect of Physician Arrest on Opioid Shipments

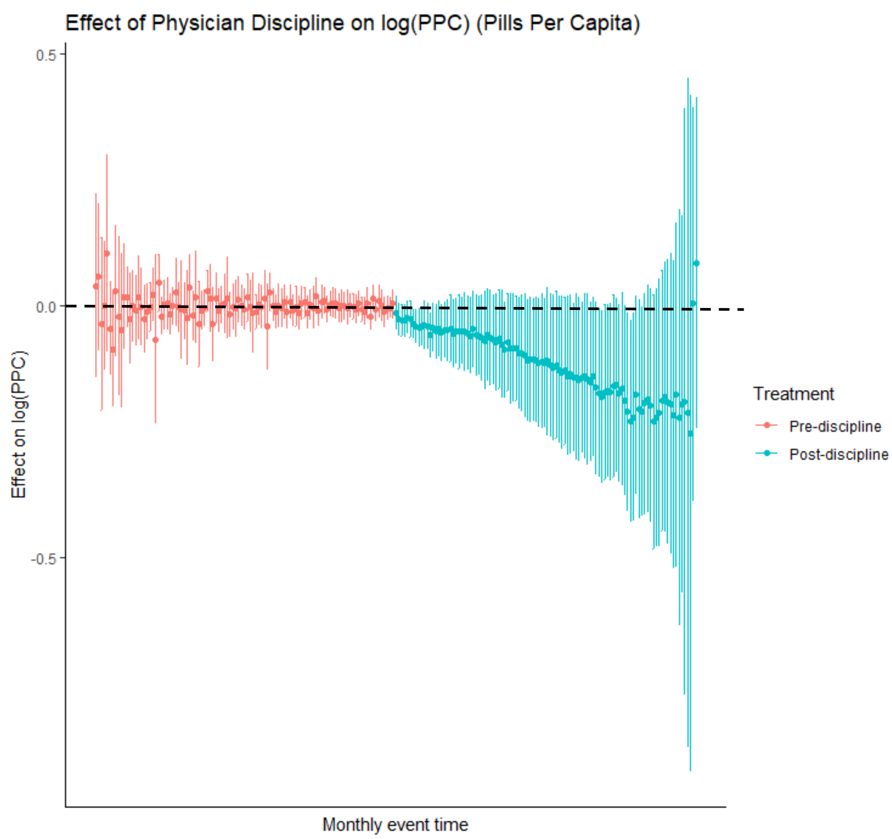
	TWFE DD	C&S 2020
Panel A: Pills Per Capita		
Arrest (ATT)	-0.0557***	-0.249**
	(0.0131)	(0.0931)
Panel B: Transactions		
Arrest (ATT)	-0.0258***	-0.0347**
	(0.010)	(0.0174)

Standard errors clustered at the county-level in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$
All models include controls as defined in the text and county- and month-year-fixed effects.
N=321,581: Sample consists of each county in United States, monthly from 2006-2014.
All models are log-linear. The TWFE DD column presents estimates using the traditional two-way fixed effects DD estimate.
The C&S 2020 column provides estimates using the Callaway and Sant'Anna (2020) regression method.

To understand if disciplinary action had both short-term and longer-term effects on opioid prescribing behavior, I present an event study estimating the effects of physician arrest on pills per capita using the Callaway and Sant'Anna (2020) regression approach in Figure 3. This approach relies on the conditional parallel trends assumption, so the event study also provides a way to test that pre-trends for the treated group are not significantly different from pre-trends for the comparison group. Estimates in the pre-treatment period are centered around zero, indicating that the conditional parallel trends assumption is satisfied. So, the estimates in Figure 3 and the second column of Table 3 are valid. Figure 3 clearly shows that pills per capita fell consistently following DEA disciplinary action. The estimates are noisy because there were few treated counties in which a physician arrest occurred, but the pattern in the post-discipline period is starkly different from the pattern in the pre-discipline period.

There is no evidence that counties simply "revert" back to their original opioid levels a few months or years following the physician arrest. Rather, the physician arrest has a stronger negative effect on the local opioid supply as more time passes. While pills per capita only falls by approximately 1% in the first month following a physician arrest, pills per capita falls by over 25% a few years after the physician arrest. Figure A3 estimates the same model, but with the outcome in levels rather than logs, and the results are consistent in showing this pattern. Given that the effects are consistently and increasingly negative as time passes, the results suggest that overprescribers are not simply replaced by new overprescribers after being taken out of the system. This could either be caused by a permanent decrease in prescribing resulting from incapacitation of the offending overprescriber, a deterrence effect that lasts far past the initial instance of discipline, or some combination of both factors.

Figure 3:

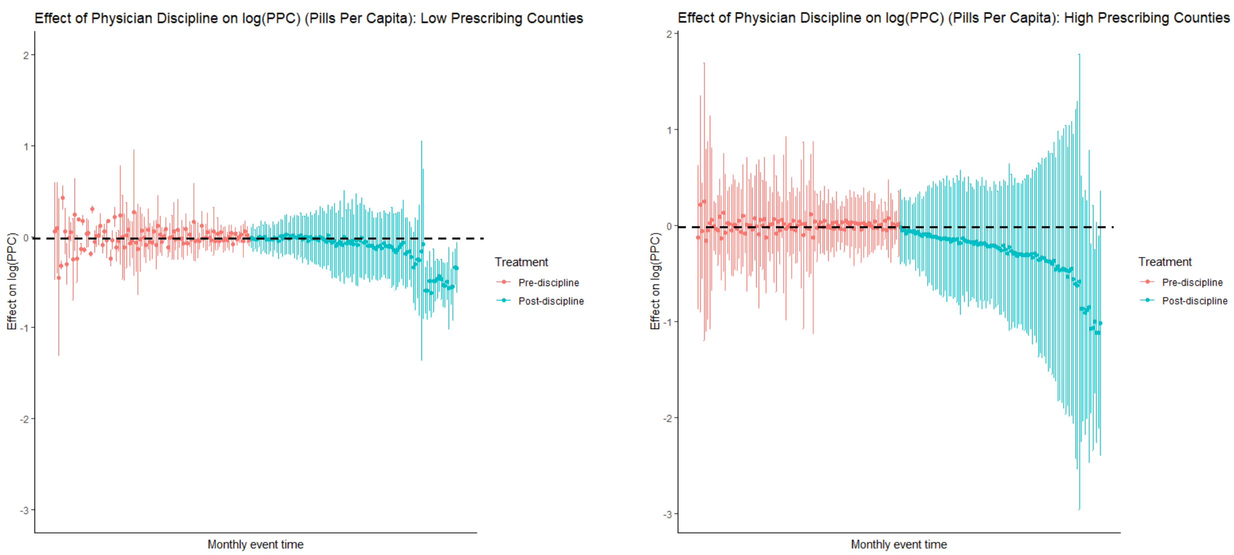


Notes: The plot provides event study estimates for the effect of physician arrests on the log of pills per capita over 107 pre- and post-discipline event time periods k = [-107, 107]. Each point estimate is associated with a confidence band representing the 95% confidence interval using clustered bootstrapped standard errors at the county-level to account for autocorrelation. The y-axis provides the effect on log(pills per capita), α_k – the percentage effect at each time period can be computed by $100 \times (\exp(\alpha_k) - 1)$.

Next, I assess potential heterogeneity in the strength of the discipline effect across counties with different levels of opioid prescribing. Taking a subset of low prescribing and high prescribing counties – where low prescribing counties had pills per capita less than than the 10th percentile for at least six months and high prescribing counties had pills per capita greater than the 90th percentile for at least six months during the county's pre-treatment period –, I find that there is little to no effect of physician discipline in counties that already had a low level of pills per capita. There are significant negative effects towards the event of the post-treatment period, but the point estimates are largely indistinguishable from zero until the last few periods. Since the sample consists of counties with very low levels of opioid prescribing, the prescriptions in this county are far more likely to be medical than nonmedical. As such, these estimates suggest that physician arrests do not significantly deter legitimate medical opioid prescribing.

And while the effects of physician discipline in high prescribing counties are noisy because there are relatively few high prescribing counties in the United States, the point estimates suggest that physician discipline had a much larger effect in counties which had high rates of opioid prescribing. For example, compared to the Figure 3 event study which estimates the effect of physician discipline across all counties in the United States, the effects in high prescribing counties towards the end of the event study period are nearly four times as large. Additionally, the negative slope of the post-treatment effects line in the High Prescribing Counties panel of Figure 4 is dramatically steeper than that in the Low Prescribing Counties panel and Figure 3, demonstrating that each additional time period results in a larger percentage decrease in the opioid prescribing rate. Assuming that counties with higher pills per capita also have more overprescribers, my results support that deterrence plays a role in decreasing opioid prescribing rates following DEA discipline. This is consistent with the foundational literature on the economics of crime and deterrence which establishes a strong association between increased enforcement and lower levels of crime (Cameron 1988; Levitt 1998).

Figure 4:



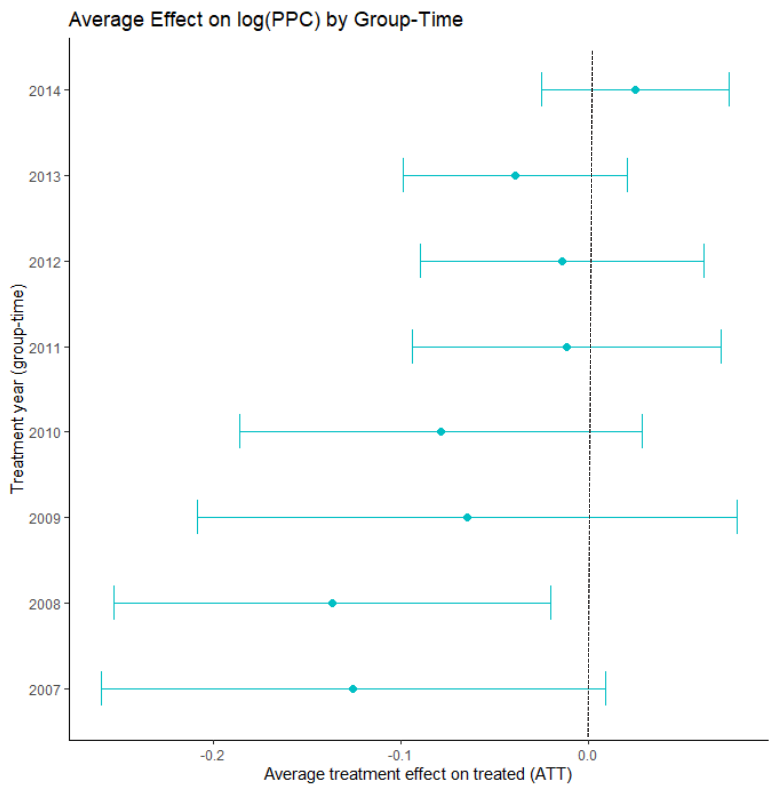
Notes: The plot provides event study estimates for the effect of physician arrests on the log of pills per capita over 107 pre- and post-discipline event time periods k = [-107, 107] in "low prescribing counties" and "high prescribing counties". Low prescribing counties are counties in which there were at least six pre-treatment months of fewer than 1 pill per capita shipped to the county (the 10^{th} percentile of monthly pills per capita). Likewise, high prescribing counties are counties in which there were at least three pre-treatment months of more than 6.5 pills per capita shipped to the county (the 90^{th} percentile of monthly pills per capita).

Each point estimate is associated with a confidence band representing the 95% confidence interval using clustered bootstrapped standard errors at the county-level to account for autocorrelation. The y-axis provides the effect on log(pills per capita), α_k – the percentage effect at each time period can be computed by $100 \times (\exp(\alpha_k) - 1)$.

Lastly, I apply the Callaway and Sant'Anna (2020) outcome regression method to yearly county-level data in order to create a set of group-time estimates for the ATT on pills per capita across different arrest years.[32] This allows us to effectively compare the effects of physician arrest in, for example, 2007 to the effects of physician arrest in 2014. Estimates become less noisy over time because there are more comparison groups after each year. The group-time point estimates suggest that yearly pills per capita fell by around 6-12% for counties which witnessed a physician arrest between 2007-2010. However, counties which experienced physician arrests beginning in 2011 saw no statistically distinguishable effect on pills per capita. While the earlier effects are noisy and some do include 0 in the 95% confidence intervals, the findings are

[32]If a physician arrest occurred in the second-half of the year (July or after), I consider the county to be have been treated in the following year.

significant at the 10% level and consistent with what we know about the opioid crisis: beginning in 2011, dependent users began to shift towards heroin as it became more accessible. If many dependent users were no longer seeking prescription opioids, then discipling overprescribers through arrest would have less of an effect on the opioid supply during this time period. Additionally, it is possible that overprescribing in earlier years was more extreme than overprescribing in later years. Similarly, arrests in earlier years may have first targeted the most egregious cases of overprescribing – so, effects of arrests on opioid prescribing moved to zero as arrested physicians' overprescribing became less extreme.



Notes: The plot provides group effects for the effect of physician arrests on pills per capita using a model using yearly data rather than monthly data. The vertical represents a zero effect. Effects can be interpreted as the average treatment effect on pills per capita for counties treated in each treatment year from 2007 to 2014. Cases in which counties experienced a physician arrest in 2006 are dropped because untreated potential outcomes are never observed for this group. Each point estimate is associated with a confidence band representing the 95% confidence interval using clustered bootstrapped standard errors at the county-level.

VII. Conclusion

Considerable attention has been paid in the literature to the effects of state-level supply-side policies that are implemented in an attempt to curb the opioid crisis (Buchmueller and Carey 2018; Grecu et al. 2018; Kaestner and Ziedan 2020; Mallatt 2018; Patrick et al. 2016; Rutkow et al. 2015). Primarily, these studies exploit variation in the timing of policy adoption to estimate effects on opioid prescribing, mortality, and other opioid-related outcomes. In this paper I instead focus on the effects of enforcing the federal CSA and subsequently disciplining physicians who prescribe opioids without a legitimate medical purpose. While other work has considered related questions by analyzing the effects of PDMPs or PMCLs that occasionally have a law enforcement component, previous analyses have not been able to identify the effects of enforcement specifically. By web-scraping DEA arrest reports, I develop a dataset that can be used to estimate the direct effects of opioid-related physician arrests – a proxy for CSA enforcement – on local opioid prescribing behavior.

I find strong and consistent evidence that counties which experienced instances of physician discipline had reduced levels of opioid prescribing. The Mingo County case study demonstrates that the DEA investigation decreased pills per capita by at least 30% and that monthly pills per capita have remained at levels significantly lower than pre-investigation. Similarly, I find that DEA opioid-related physicians arrests decreased monthly pills per capita by approximately 20% in the post-discipline period after adjusting for bias stemming from the usage of a traditional TWFE DD. Given that Phillips et al. (2017) estimate 20-25% of prescription opioids are diverted for nonmedical use, my findings suggest that CSA enforcement may have reduced prescription opioid diversion to essentially zero in counties that experienced a physician arrest since evidence suggests the reduction in prescribing was predominantly from overprescribers.

Event study estimates demonstrate the effects of physician arrests on opioid prescribing are consistently negative, grow over time, and are larger in counties which had historically high rates of opioid prescribing. Both the case study estimates and the staggered adoption estimates support that arrests continually deterred other physicians from beginning to overprescribe because the post-arrest effects were consistently negative in the post-treatment period and larger in high prescribing counties that presumably had higher levels of opioid diversion. Furthermore, I show that the effects of arrests diminished to effectively zero for counties treated in 2011 and thereafter. Based on the evolution of opioid crisis and the DEA's incentive to arrest the worst offending overprescribers first, I propose that these results are reasonable and further evidence that opioid policy effects can vary dependent on when the policy is first adopted.

These findings are largely consistent with studies analyzing the effects of PDMPs and PMCLs. For example, recent studies estimate that implementing must-access PDMPs reduced states' opioid shipments by between 10-15% (Buchmueller and Carey 2018; Mallatt 2018). Similarly, Kaestner and Ziedan (2020) estimate that implementing PMCLs reduced opioid shipments by 15-50%. Given that I find physician discipline to have decreased opioid shipments by 30-60% in the extreme case of Mingo County and by approximately 20% for general cases of physician discipline measured through instances of a DEA-reported arrest, my results are plausible.

Because the United States opioid crisis is characterized by the overwhelming and increasing number of opioid-related deaths each year, significant attention is paid to the effects of supply-side policies on opioid mortality. The broad consensus in the literature is that opioid prescribing and opioid mortality are positively correlated (Bohnert et al. 2011; Pacula and Powell 2018) – as such, actions which decrease opioid prescribing should be associated with decreases in opioid mortality. Unfortunately, because I apply a treatment which is adopted at the county-level, there is too much data suppression – over 85% of county-year observations – to empirically test this using the public-use CDC WONDER mortality data. In lieu of this analysis, I instead estimate the effect of the DEA investigation of Mountain Medical Care Center and associated pharmacies on opioid mortality in West Virginia. I generally find that the investigation had minimal short-run effects, but began to decrease opioid mortality a few years after the investigation started and one year following the pill mill raid. This is consistent with a study by Delcher et al. (2015) which finds that the Florida PDMP caused an abrupt decline in oxycodone prescribing that subsequently caused a significant decline in opioid mortality.

Contrasting recent literature finding that the reformulation of OxyContin led to substitution towards heroin in terms of increased heroin mortality (Alpert et al. 2018; Evans et al. 2019), I find that the prescription opioid supply shock did not cause illicit opioid mortality to increase in West Virginia. In fact, illicit opioid mortality significantly decreased in West Virginia compared to other states. However, there may have been other factors contemporaneous to the investigation that caused the decrease in opioid mortality. Similarly, opioid mortality reporting varies across states and other outcomes – such as nonfatal overdoses or emergency room visits for varying opioid-related causes – may be better suited to understanding if there is a substitution effect. Nonetheless, my results suggest that supply-side policies do not always result in substitution and increased illicit opioid mortality. Given the limitation of my county-level analysis, a natural avenue for future research would be to continue the analysis of DEA arrests with unsuppressed county-level mortality data or opioid overdose emergency room visits. This would improve our understanding of physician discipline's impact on nonmedical opioid use and provide policymakers with better-measured effects on opioid-related harms that could be compared to the effects of other state-level policies.

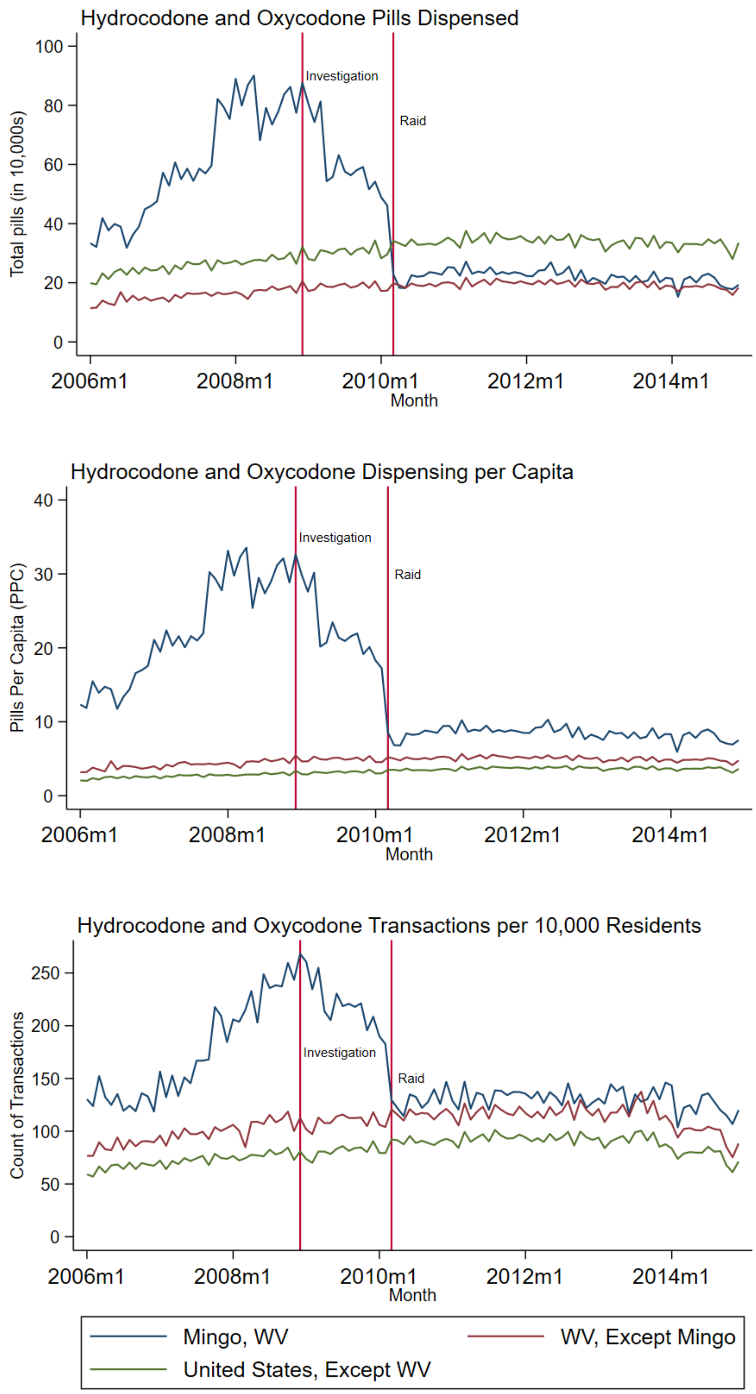
By applying the Callaway and Sant'Anna (2020) method to deal with bias resulting from time-varying effects, my analysis also provides a new set of parameters along with reasoning for why future opioid policy researchers may want to consider using new difference-in-differences methods that account for time-varying effects. For example, Powell et al. (2018) suggest that medical marijuana laws passed before 2010 may have a different effect on opioid mortality than those passed after because laws prior to 2010 were laxer and thereby would have provided broader access to medical marijuana. Hypotheses like this can be rigorously tested using group-time average treatment effects which allow researchers to infer if the average treatment effect for units treated in one time period is significantly different from the average treatment effect for units treated in some other time period. Given the everchanging nature of the opioid crisis, group-time average treatment effects provide researchers with a way to better understand the complexity of policies' effects. So, I recommend that this method be applied in future opioid policy research.

Additionally, future work may seek to assess the effects of physician arrest on local drug-related crime. Specifically, crime related to the black market sale of prescription opioids or heroin. I would hypothesize that physician arrests may increase crime related to the sale of prescription opioids in the short-term – since dependent users are no longer able obtain prescription opioids directly from the incapacitated overprescriber and must now rely on the black market –, but could decrease black market sale of prescription opioids in the long-term since secondhand sellers would no longer have access to as many prescription opioids. The effects on heroin-related crime are ambiguous and potentially dynamic given that some current users may switch to heroin while there should be fewer future heroin users because areas with fewer prescription opioids per capita also have lower rates of heroin use. This analysis was not feasible with the crime data currently publicly available, but would be possible with forthcoming detailed data on criminal events from the national Criminal Justice Administrative Records System initiative.

This work demonstrates that physician discipline can have significant negative effects on local prescribing behavior and opioid mortality. Considering ongoing discussion regarding possible policies to curb the opioid crisis, policymakers should not rule out increasing enforcement in areas where opioid prescribing levels remain unusually high to revoke overprescribers' medical licenses and deter other physicians from overprescribing. Like PDMPs and PMCLs, physician discipline may also reduce opioid-related harms. Importantly, there is little evidence of targeted physician discipline deterring proper medical prescribing while there is evidence of PDMP and PMCL legislation sometimes deterring physicians from prescribing opioids when medically necessary. However, effects of CSA enforcement today may be minimal compared to CSA enforcement which occurred in 2006-2010 because the opioid crisis has since evolved and illicit substitutes are more easily accessible. At a minimum, however, my results suggest that physician discipline has had a meaningful impact on reducing local overprescribing and that the lasting effects of these actions have so far been overlooked by researchers and policymakers.

Appendix

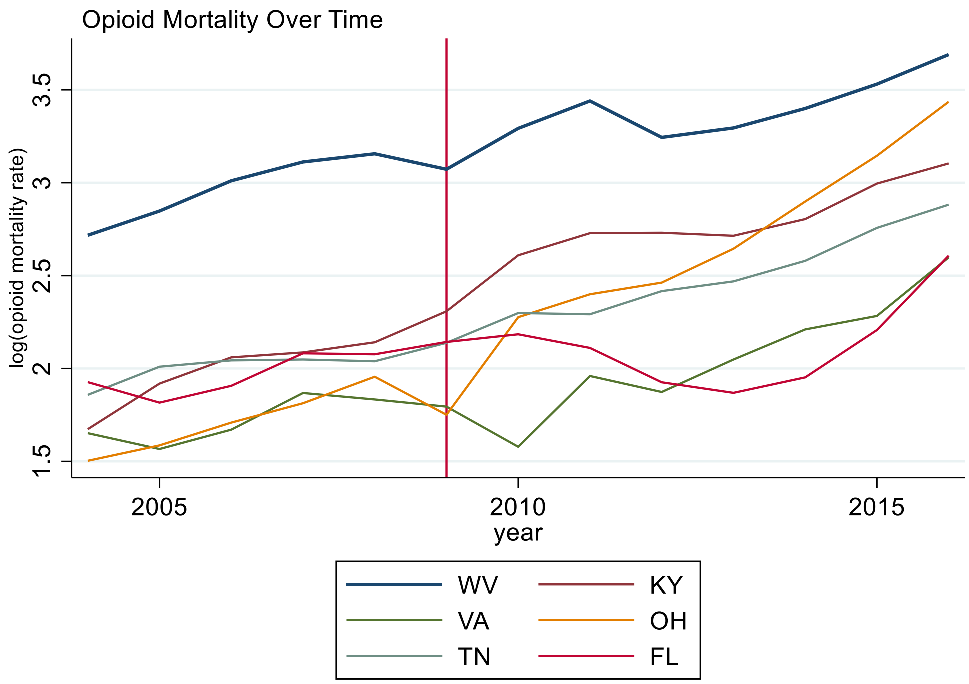
Figure A1:



Source: ARCOS Washington Post Data

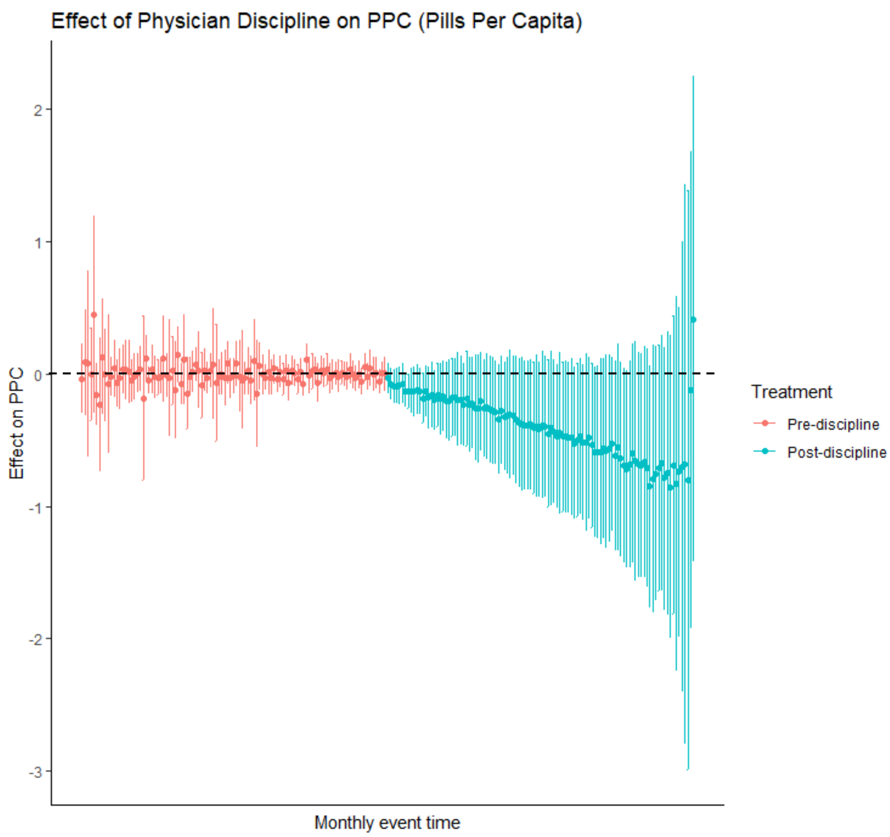
Notes: Lines show the average for each respective group. For example, the green line in the top panel illustrates the total number of hydrocodone and oxycodone pills (in 10,000s) across the United States, not including West Virginia. Vertical lines indicate the timing of the Mingo County investigation and raid respectively.

Figure A2:



Notes: Lines show the yearly log opioid-related mortality rate for West Virginia and a variety of other comparison states. The comparison states are selected based on proximity to West Virginia and/or documented similarities in terms of social or economic factors (e.g., proportion of population that is Non-Hispanic White, unemployment rate, etc.) that are related to the opioid crisis.

Figure A3:



Notes: The plot provides event study estimates for the effect of physician arrests on pills per capita (in levels) over 107 pre- and post-discipline event time periods *k* = [-107, 107]. Each point estimate is associated with a confidence band representing the 95% confidence interval using clustered bootstrapped standard errors at the county-level to account for autocorrelation.

Table A1:

Incidences of Opioid-Related Phrases in DEA Cases Against Doctors Reports

String Expressions	**Count**
"oxycodone", "hydrocodone", "opioid", "oxycontin", "percocet"	126
"dispensing/prescribing to a drug dependent", "conspiracy to distribute controlled"	52
"legitimate medical purpose"	155
"drug addiction"	41

Notes: This table shows the phrases used to identify if a Cases Against Doctors report was related to opioids. The **Count** column identifies the number of reports which contained particular string expressions.

Table A2: Covariate Balance for Section V County-Level Case Study

	Control (WV, Non-Mingo Counties)				Treatment (Mingo County)			
	Pre-DEA		Post-DEA		Pre-DEA		Post-DEA	
% White	96.11	(3.50)	85.64	(3.81)	96.11	(0.08)	96.92	(0.21)
% 15-29	17.99	(3.21)	17.42	(3.28)	18.28	(0.32)	16.99	(0.33)
% 30-49	26.92	(1.40)	25.47	(1.64)	27.87	(0.28)	26.78	(0.46)
% 50-64	21.49	(1.46)	22.62	(1.63)	22.08	(0.61)	23.41	(0.30)
% 65+	16.31	(3.13)	17.29	(3.24)	16.57	(0.36)	17.42	(0.35)
% Unemp.	4.85	(1.01)	8.3	(1.98)	4.8	(0.52)	9.88	(1.26)
Total Pop.	33477.54	(32618.11)	33826.89	(33324.53)	27016.33	(125.09)	26375.67	(430.99)
Income	35240.38	(6549.522)	37657.77	(6288.69)	28470.67	(1207.66)	31284	(1352.86)
Pill Mill	0	(0)	0.5	(0.5)	0	(0)	0.5	(0.5)
Must-Access PDMP	0	(0)	0.42	(0.45)	0	(0)	0.42	(0.45)

I provide weighted sample means with standard errors in parentheses. The county-month level data is weighted by the population in each county. The pre-DEA columns contain data from 2006-2008 while the post-DEA columns contain data from 2009-2014, following the DEA investigation in Mingo County.

Table A3: Covariate Balance for Section VI Staggered Treatment County-Level Analysis

	Control (Never Disciplined)			Treatment (Disciplined)			
	Pre-Discipline		Post-Discipline	Pre-Discipline		Post-Discipline	
% White	80.24	(18.89)		71.52	(20.61)	65.50	(20.29)
% 15-29	18.96	(4.19)		20.27	(3.26)	20.73	(3.33)
% 30-49	25.13	(2.74)		27.43	(2.54)	27.02	(2.36)
% 50-64	20.58	(2.69)		18.99	(2.53)	19.24	(1.94)
% 65+	13.45	(1.78)		13.01	(1.78)	13.99	(1.34)
% Unemp.	7.08	(2.98)		6.75	(3.34)	7.61	(2.37)
Total Pop.	659666.67	(133459.9)		441480.7	(684725)	759631.7	(1159332)
Income	43479.18	(10976.4)		50424.86	(13490.44)	54056.68	(14286.26)
Pill Mill	0.15	(0.35)		0.08	(0.27)	0.21	(0.41)
Must-Access PDMP	0.03	(0.15)		0.02	(0.14)	0.04	(0.17)
N	300718	[2841]		8734	[175]	12650	[175]

I provide weighted sample means with standard errors in parentheses for the pre- and post-period of counties which experienced a case of physician arrest and were disciplined at varying times.
There is no post-discipline data for the control counties because they are never disciplined and the timing of arrest is differentiated, occurring in various months starting in 2006 and through 2014.
The values in brackets in the N row represent the number of unique counties in each group.

Table A4: Effect of Supply Shock on Opioid Mortality, Across Various Specifications

	(1) All	(2) All	(3) All	(4) All	(5) Rx	(6) Rx	(7) Rx	(8) Rx	(9) Illicit	(10) Illicit	(11) Illicit	(12) Illicit
Investigation	-0.0532	-0.252***	-0.250***	-0.242	-0.129*	-0.0315	0.0299	-0.0652	-0.162	-0.282**	-0.281**	0.0731
	(0.0907)	(0.0677)	(0.0714)	(0.335)	(0.0697)	(0.0723)	(0.0765)	(0.324)	(0.140)	(0.115)	(0.122)	(0.436)
Unemployment	0.0347	0.0600***	0.0591***	0.0433	0.0370	0.0604***	0.0601**	0.0326	-0.0124	0.00527	0.00470	-0.0748
	(0.0227)	(0.0189)	(0.0202)	(0.0368)	(0.0225)	(0.0213)	(0.0226)	(0.0484)	(0.0275)	(0.0286)	(0.0306)	(0.0380)
Income	0.00451	0.000839	0.000731	-0.0164	0.0284	0.0168	0.0168	-0.0345	-0.0390	-0.0146	-0.0150	-0.0296
	(0.0208)	(0.0171)	(0.0186)	(0.0138)	(0.0189)	(0.0181)	(0.0195)	(0.0219)	(0.0262)	(0.0238)	(0.0257)	(0.0612)
Total Pop.	-0.0193***	-0.0110***	-0.0117***	-0.0883*	-0.0202***	-0.0111*	-0.0115*	-0.119**	-.0104**	-0.0150**	-0.0153**	-0.00136
	(0.00587)	(0.00358)	(0.00399)	(0.0362)	(0.00547)	(0.00582)	(0.00636)	(0.0439)	(0.00345)	(0.00667)	(0.00726)	(0.0556)
Percent Male	0.528**	0.979***	0.987**	-0.718	0.461*	0.736	0.738	-1.705*	2.525***	2.050***	2.064***	0.713
	(0.257)	(0.362)	(0.386)	(0.641)	(0.245)	(0.448)	(0.479)	(0.813)	(0.552)	(0.575)	(0.612)	(1.559)
Percent White	0.000260	0.0848**	0.0842**	-0.110	-0.0368	0.0110	0.0110	-0.261	-0.0331	-0.00964	-0.0111	-0.0702
	(0.0379)	(0.0350)	(0.0373)	(0.144)	(0.0327)	(0.0369)	(0.0395)	(0.200)	(0.0595)	(0.0525)	(0.0569)	(0.146)
Percent 15-29	-0.261***	-0.188**	-0.181**	-0.0586	-0.190**	-0.117	-0.114	0.379	-0.288*	-0.181	-0.170	-0.347
	(0.0960)	(0.0817)	(0.0870)	(0.219)	(0.0920)	(0.111)	(0.117)	(0.243)	(0.154)	(0.129)	(0.136)	(0.224)
Percent 30-49	-0.464***	-0.382***	-0.381***	0.454	-0.320***	-0.154	-0.154	0.707	-0.929***	-0.723***	-0.721***	-0.779
	(0.108)	(0.106)	(0.112)	(0.387)	(0.0978)	(0.131)	(0.140)	(0.414)	(0.159)	(0.184)	(0.197)	(0.545)
Percent 45-54	-0.240***	-0.398***	-0.398***	0.147	-0.194***	-0.286***	-0.286***	0.904	-0.645***	-0.520***	-0.519***	-1.297**
	(0.0721)	(0.0823)	(0.0881)	(0.386)	(0.0593)	(0.0881)	(0.0947)	(0.474)	(0.103)	(0.125)	(0.133)	(0.381)
Percent Over 65	-0.207*	-0.346***	-0.341***	-1.076**	-0.152	-0.177	-0.175	-0.694	-0.200*	-0.195*	-0.185	-2.665**
	(0.106)	(0.0869)	(0.0920)	(0.322)	(0.0949)	(0.132)	(0.139)	(0.370)	(0.115)	(0.108)	(0.114)	(0.810)
Pill Mill	0.102	0.0710	0.0714	-0.0291	0.0683	0.0242	0.0245	-0.0492	0.0263	0.0590	0.0585	-0.0760
	(0.0910)	(0.0783)	(0.0839)	(0.0600)	(0.0654)	(0.0654)	(0.0699)	(0.0670)	(0.112)	(0.125)	(0.134)	(0.0626)
PDMP	0.0742	0.0141	0.0102	-0.246*	0.0281	0.0187	0.0155	-0.151	0.302**	0.209	0.205	0.0812
	(0.0764)	(0.0740)	(0.0793)	(0.0971)	(0.0841)	(0.0830)	(0.0890)	(0.109)	(0.122)	(0.152)	(0.164)	(0.0989)
N	418	418	418	54	414	414	414	54	390	390	390	390
Weighted	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
State Time Trend	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Comparison Group	U.S.	U.S.	U.S.	Similar	U.S.	U.S.	U.S.	Similar	U.S.	U.S.	U.S.	Similar

This table presents difference-in-differences estimates for the effect of the prescription opioid supply shock in West Virginia caused by the investigation of overprescribers in Mingo County. All models include time-fixed effects and those without state time trends include state-fixed effects. Columns (1)-(4) estimate the effect on all opioid mortality; columns (5)-(8) estimate the effect on prescription opioid mortality; columns (9)-(12) estimate the effect on illicit opioid mortality. I generally use all states in which the cause-specific death counts were not suppressed as the comparison group, but also run models using select states (FL, KY, OH, TN, and VA) as the only comparison group since these states are geographically close to West Virginia and share similar demographic and socioeconomic characteristics.

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