Beyond Opioids: The Effect of Prescription Drug Monitoring Programs on Non-Opioid Drug Prescribing*

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Abstract

A growing literature has examined how mandatory access prescription drug monitoring programs (MA PDMPs), laws that require providers to consider a patient's prescription history before prescribing controlled substances, affect opioid-related outcomes. However, evidence of their impact on non-opioid-related prescribing is mixed. This paper investigates the effect of MA PDMPs on prescribing patterns of stimulants and benzodiazepines. Using updated difference-in-differences methodology, we show that MA PDMPs led to decreases in stimulant prescribing but had no significant effects on benzodiazepine prescribing. Our findings highlight that MA PDMPs do have effects on non-opioid drug prescribing, but these effects differ substantially across drug types.

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1 Introduction

Over the past 25 years, the United States has undergone the most devastating drug crisis in its history. Between 1999 and 2020, drug overdose deaths have increased more than 500 percent, with nearly 92,000 deaths in 2020 alone.[1] Since opioids have been the primary driver of these increases, relatively little attention has been paid to non-opioid drugs. Although in many cases non-opioid drugs are not as fatal as opioids, the ingestion of multiple drugs or simultaneous use along with opioids could dramatically increase the risk of adverse outcomes, such as misuse and overdose (Ruhm, 2017). Unfortunately, overdose deaths involving non-opioid drugs have increased almost as fast as those involving opioids: overdose deaths involving non-opioid drugs rose 274 percent from 1999 to 2016 (Ruhm, 2019). This increase is attributable to a rise in polydrug use; that is, the simultaneous use of multiple drugs for enhanced recreational benefits. Over half of overdose deaths currently involve polydrug use, generally combining opioids with stimulants (e.g., amphetamines) or sedatives (e.g., benzodiazepines) (Ruhm, 2017, 2019).

As drug overdose deaths have continued to soar, policy makers have implemented a myriad of laws and regulations in an attempt to stem the tide of rising deaths. Many of these policies specifically and exclusively target prescription opioid use and misuse.[2] In contrast, other policies such as prescription drug monitoring programs (PDMPs)—state-run databases which allow prescribers to view a patient's prescription history before prescribing controlled substances—target a wide range of prescription drugs. PDMPs track prescriptions of schedule II–V controlled drugs, including stimulants and benzodiazepines. By 2019, all but one state had established a PDMP, and more than 40 states have enacted laws that require prescribers to access the PDMP database before prescribing opioids and/or other controlled substances, commonly called mandatory access (MA) PDMPs. Because prior research has found that MA PDMPs are especially effective at changing prescribing behavior, we also focus our attention on MA PDMPs.[3]

Understandably, the vast majority of prior research has examined the impact of MA PDMPs and other policies with a primary focus on opioid-related outcomes.[4] Unfortunately, the misuse and abuse of other non-opioid substances have become increasingly common. Bachhuber et al. (2016) document a 37 percent increase in the fraction of adults filling a benzodiazepine

[1] Centers for Disease Control and Prevention (`https://www.cdc.gov/drugoverdose/prevention/index.html`).

[2] For example, the reformulation of OxyContin and the rescheduling of hydrocodone-combination products.

[3] Prior research has shown that, when not mandated, PDMP engagement among prescribers is low (Haffajee et al., 2015; Kreiner et al., 2014).

[4] See Section 2.1 for a discussion of this literature.

prescription from 1996 to 2013, with a corresponding five-fold increase in the overdose death rate.[5] Safer (2016) report a 580 percent increase in the number of stimulant prescriptions dispensed between 1993 and 2014. This is concerning in light of alarming rates of prescription stimulant misuse—particularly among adolescents and young adults—with estimates ranging from 16 to 35.6 percent of survey respondents reporting stimulant misuse at some point in their lives (Babcock and Byrne, 2000; Low and Gendaszek, 2002; McCabe et al., 2017). Using data from the 2015-16 wave of the National Survey on Drug Use and Health (NSDUH), we estimate that approximately 2 percent of individuals over the age of 12 reported misusing benzodiazepines in the past year, with a similar percentage reporting misuse of prescription stimulants. This misuse accounts for approximately 18 percent of all benzodiazepine use and up to 40 percent of all stimulant use.[6] The striking rise in prescription stimulant and benzodiazepine consumption over time, along with the disconcerting rates of misuse, underscore the importance of investigating factors that influence their use. However, there is limited evidence on the extent to which existing policies have influenced the prescribing of these drugs.

In this paper, we estimate the effect of MA PDMPs on the prescribing of stimulants and benzodiazepines using a difference-in-differences event study framework, exploiting the staggered adoption of MA PDMPs across states over time. Since recent literature has shown a variety of potential biases in two-way fixed effects (TWFE) models in the presence of staggered adoption, our main estimates rely on the estimator proposed by Callaway and Sant'Anna (2021). Our analysis uses administrative data on the legal supply of stimulants from the Drug Enforcement Administration (DEA)'s Automation of Reports and Consolidated Orders System (ARCOS) and data on the prescribing of stimulants and benzodiazepines from the Medicaid State Drug Utilization Data over the period 2008–2017.[7] Our outcomes of interest are amphetamine-equivalent stimulant grams per 100 population and benzodiazepine prescriptions per 100 Medicaid enrollees.[8]

Ex ante, MA PDMPs could reduce non-opioid drug prescribing through several different channels. First, PDMPs are designed to affect prescribing by providing information on patients' prescription history, which allows providers to identify inappropriate prescribing trends. Second,

[5] Likewise, Agarwal and Landon (2019) find that the use of benzodiazepines in ambulatory care nearly doubled from 2003 to 2015.

[6] Compton et al. (2018) estimates similar rates of prescription stimulant use and misuse.

[7] The DEA does not track benzodiazepine shipments.

[8] We also report the results obtained using an alternative measure of stimulant prescribing constructed using data from Medicaid (i.e., stimulant prescriptions per 100 Medicaid enrollees).

Alpert et al. (2020) suggest that the "hassle cost" of required access to the PDMP database could deter physicians from prescribing controlled substances under any circumstances. Our finding that MA PDMPs reduce stimulant prescribing may reflect these two channels. However, there are also mechanisms by which MA PDMPs could actually increase the utilization of certain drugs. For example, reductions in the availability of commonly diverted drugs may increase demand for substitute drugs (such as benzodiazepines). Several studies have shown that MA PDMPs have unintended consequences of shifting users toward illicit opioids, which are often taken in conjunction with benzodiazepines (Meinhofer, 2018; Kim, 2021). Similarly, as MA PDMPs limit access to opioids, they could increase the share of patients with opioid withdrawal symptoms, who may seek benzodiazepines to treat opioid withdrawal.[9]

Overall, our estimates indicate that MA PDMPs led to decreases in the legal supply of stimulants. The results patterns are similar pooling across different stimulants and examining different types of stimulants separately (e.g., amphetamine and lisdexamfetamine). Five years following policy implementation, MA PDMPs were associated with a 20.6 percent decrease in amphetamine-equivalent stimulant grams per 100 population relative to the mean one year before treatment. Our point estimates range between a 20.3–21.9 percent decrease when we examine each type of stimulant separately.[10] We find qualitatively similar results when implementing the method of Sun and Abraham (2021), as well as a traditional TWFE model.

In contrast, our results suggest that MA PDMPs did not lead to substantive changes in benzodiazepine prescribing. These results are similar using methodology proposed by Sun and Abraham (2021). Interestingly, we find somewhat different results in a TWFE model, highlighting the importance of recent advances in the difference-in-differences literature. This null result is likely due to the fact that many MA PDMPs do not require physicians to check the PDMP before prescribing Schedule IV drugs.

Despite a large literature on the effect of PDMPs on opioid-related outcomes (e.g., Meinhofer, 2018; Buchmueller and Carey, 2018), only a handful of papers have examined the effects of MA PDMPs on non-opioid-related outcomes. Consistent with our findings, Meinhofer (2018) finds that MA PDMPs lead to a decrease in the supply of prescription stimulants. Concurrent work by

[9] Stein et al. (2016) surveyed those who used benzodiazepines in the month prior to initiating inpatient opioid detoxification; among the 176 survey participants, 10.2% reported the reason for benzodiazepine use as 'to decrease opioid withdrawal.'

[10] Our estimates indicate that stimulant grams per 100 population decrease by 1.14 for amphetamine (20.7% in terms of the mean one year before the treatment, $p-$value<0.001), by 0.65 for methylphenidate (20.3%, $p-$value$=0.075$), and by 0.10 for lisdexamfetamine (21.9%, $p-$value$=0.027$) five years following policy implementation.

Gunadi and Shi (2023a,b) also finds reductions in stimulant supply, although they find no change in stimulant prescriptions among Medicaid beneficiaries. Several studies have examined the effects of MA PDMPs on benzodiazepine-related outcomes and produced mixed results. Meinhofer (2018) shows that benzodiazepine-involved deaths decline following MA PDMPs. Other studies in the medical literature find no effect of MA PDMPs on benzodiazepine deaths (Liang and Shi, 2019), benzodiazepines dispensed, dosage, or spending (Liang et al., 2021).[11]

The findings of this paper contribute to this literature in two key ways. First, all of the above papers rely on TWFE models. However, recent literature highlights potential biases arising from TWFE models in the presence of staggered adoption (Goodman-Bacon, 2021; Roth et al., 2023). In this paper we rely on more recent methodology which explicitly accounts for these potential biases. Our main results are based on Callaway and Sant'Anna (2021), and we also verify all of our results using the estimator proposed by Sun and Abraham (2021). Second, unlike most prior work, which was only able to investigate the short-term effect of the policy, we use a longer period to analyze how MA PDMPs affect non-opioid prescribing in the medium term. For example, Meinhofer (2018)—the most closely related paper—only covers PDMPs implemented by 2013. The majority of MA PDMPs have been relatively recently implemented and having both additional states and years of data allows us to better understand their policy effects. While Meinhofer (2018) presents event study regression estimates of PDMPs up to two years after the implementation of a PDMP, we are able to use our longer sample period to trace out their effects up to five years after implementation.[12] This is especially important for stimulants, which exhibit stronger responses as time passes.

Our findings inform the policy discussion surrounding MA PDMPs along two key dimensions. First, our results highlight the fact that MA PDMPs impact drug prescribing patterns for a variety of non-opioid drugs. This is important in light of the complicated interrelationships between various drugs. The effect of PDMPs on drug prescribing depends not only on the direct effects of the PDMP on physician behavior, but also on the demand response which is a function of the substitutability or complementarity of a myriad of different drugs. Second, these effects are not uniform across drug types. While we find decreases in stimulant prescribing in response to

[11] Liang and Shi (2019) use the Medicaid State Drug Utilization Data to study the impact of PDMP mandates for use of benzodiazepine records on benzodiazepine prescribing. Using an event study design, they find no evidence for the association between the mandates and quantity, dosage, and Medicaid spending of benzodiazepine prescriptions per 100 enrollees in a quarter-period.

[12] Although Gunadi and Shi (2023a,b) use a longer panel, they focus on estimating the effects of PDMPs in the first two years following implementation.

MA PDMPs, we can largely rule out such decreases for benzodiazepines. This heterogeneity could be the result of differences in regulatory scrutiny for these different drug types (e.g., stimulants are typically Schedule II drugs, while benzodiazepines are Schedule IV), or they may reflect important differences in how these drugs relate to each other. For example, if MA PDMPs reduce access to certain drugs, then we may expect to see increases in demand for substitutes. This would be true even if the substitute drug is also covered by the PDMP. Therefore, it is unlikely that PDMPs will uniformly decrease prescribing of all commonly misused drugs.

Our paper proceeds as follows: in Section 2, we discuss the institutional details of PDMPs as well as some of the most closely related literature. We also provide background information on stimulants and benzodiazepines, the two drug classes of interest in this paper. We describe our data in Section 3 and our identification strategy in Section 4. We present our main results in Section 5 and conclude in Section 6.

2 Background

2.1 Prescription Drug Monitoring Programs and Related Literature

PDMPs A PDMP is a state-level database that collects information on patients' scheduled prescription medications at the point of prescribing or dispensing.[13] PDMPs are designed to help providers identify inappropriate use of scheduled prescription medications. Authorized providers are able to access the database for patients' controlled substance prescription histories before prescribing. By 2017, all states but Missouri had a modern electronic PDMP system in operation (Horwitz et al., 2021).[14] However, when providers are not required to access the database before prescribing, provider participation rates are low (Haffajee et al., 2015).[15] In response to the low participation rates, 26 states implemented a mandatory access provision between 2007 and 2017.[16] MA PDMPs legally require providers to use the PDMP database before controlled substance prescribing under certain conditions. Provider utilization has substantially increased following the implementation of MA PDMPs. For example, the number of active users in New York reached 67,779 in the first six months of policy implementation, while it had only

[13] Controlled substances are placed into one of 5 "schedules" reflecting their medical efficacy and potential for misuse. Schedule I drugs are federally illegal, while Schedule II-V drugs are available only via prescription, with lower numbered schedules reflecting higher potential for misuse.

[14] See Horwitz et al. (2021) for more information.

[15] The utilization rate among healthcare providers in states without the mandates is about 14 to 25 percent (Alexander, 2015).

[16] See Table 1 and Appendix Figure A1.

5,087 users prior to the mandate (PDMP Center of Excellence, 2014).

Although PDMPs have historically been considered as a means to combat prescription opioid diversion and misuse, they typically encompass a variety of different drugs. For example, of the 26 mandatory access PDMPs that were implemented between 2007–2017, 12 of them require the prescriber to query the PDMP prior to prescribing any Schedule II substances (e.g., stimulants).[17] Likewise, 17 explicitly require the prescriber to query the PDMP to prescribe benzodiazepines (schedule IV). In our primary specification, we construct our treatment variable as an indicator for whether the state has a mandatory access provision for any drug. Prior research has highlighted hassle costs as an important mechanism by which PDMPs reduce drug prescribing, even if the information provided by the PDMP does not necessarily warrant the reduction (Alpert et al., 2020). Our results are consistent with a large role for hassle costs, with drug prescriptions falling as a result of PDMPs even for drugs that are not explicitly included.[18]

Related Literature A rapidly expanding literature has documented the effects of PDMPs on a variety of outcomes. Early work in this area that did not distinguish between voluntary and mandatory access programs produced mixed results on an impact of PDMPs on opioid-related outcomes. For example, Meara et al. (2016) find no statistically significant effect of PDMPs on various opioid-prescribing outcomes among Medicare beneficiaries. In contrast, other work shows that PDMPs were associated with reduced opioid-related mortality (Kilby, 2016) and reduced rates of opioid prescribing in ambulatory care settings (Bao et al., 2016).

Studies examining states where prescribers are required to query the PDMP prior to prescribing—commonly referred to as mandatory access PDMPs—have shown significant reductions in prescription opioid misuse (Buchmueller and Carey, 2018; Grecu et al., 2019; Kim, 2021; Mallatt, 2018; Meinhofer, 2018; Wen et al., 2019). For example, Buchmueller and Carey (2018) show that MA PDMPs reduce measures of excessive opioid consumption and doctor shopping among Medicare beneficiaries. Likewise, Mallatt (2018) finds that the implementation of a MA PDMP reduces oxycodone shipments by 8 percent. Shakya and Ruseski (2023) show that counties in states with a MA PDMP have 5.5 fewer retail opioid prescriptions per 100 persons

[17] Data on what drugs are included in the mandate are from the Pew Charitable Trusts. For more details, see: https://www.pewtrusts.org/en/research-and-analysis/data-visualizations/2018/when-are-prescribers-required-to-use-prescription-drug-monitoring-programs, last accessed April 24, 2022.

[18] In results not presented here, we find little evidence of heterogeneity by whether the law explicitly requires prescribers to check the PDMP prior to prescribing stimulants or benzodiazepines. One potential explanation for this is that prescribers are unaware of the finer details of the law and mistakenly believe that all controlled substances are covered.

compared to those in states with a voluntary-access PDMP. Consistent with this reduction in opioid prescribing, Grecu et al. (2019) find a 20–26 percent decline in admissions to drug treatment facilities following the implementation of a MA PDMP. In addition, these reductions in prescribing have resulted in fewer overdose deaths involving prescription opioids. For example, Meinhofer (2018) shows that prescription opioid-related deaths decrease by 9 percent following MA PDMP implementation.

Later work has considered the impact of mandatory access PDMPs beyond opioid prescribing and overdose deaths. Several recent papers have examined substitution toward illicit substances, especially heroin and fentanyl, in response to reduced prescription opioid access as a result of mandatory access PDMPs. Meinhofer (2018) and Kim (2021) find that mandatory access PDMPs led to increases in heroin overdose deaths, offsetting reductions in prescription opioid overdose deaths. Likewise, Mallatt (2018) shows that PDMPs led to increases in heroin-related crime in counties with high levels of pre-PDMP prescription opioid use.

Given the broad scope of PDMPs and their impacts on opioid prescribing, it is plausible that they could alter prescription patterns for other drugs as well. There is, however, a dearth of evidence on the effects of PDMPs on non-opioid prescriptions. Meinhofer (2018) shows that MA PDMPs lead to a decrease in the supply of prescription stimulants. In contrast, several studies have examined the effects of MA PDMPs on benzodiazepine-related outcomes and produced mixed results: Meinhofer (2018) shows that benzodiazepine-involved deaths decline following MA PDMPs; Winstanley et al. (2018) find that Ohio's mandate led to a statistically significant decrease in benzodiazepines dispensed; other studies in the medical literature find no effect of MA PDMPs on overdose deaths involving benzodiazepines (Liang and Shi, 2019), benzodiazepines dispensed, dosage, or spending (Liang et al., 2021).

2.2 Benzodiazepines and Stimulants

Benzodiazepines Benzodiazepines are a class of drugs that are most commonly prescribed to treat anxiety and panic disorders, although they are widely used to treat other ailments. Benzodiazepines are commonly referred to as "benzos", and include drugs such as alprazolam (brand name "Xanax"), diazepam (brand name "Valium"), and clonazepam (brand name "Klonopin"). These drugs work by suppressing the activity of nerves in the brain, relieving symptoms of various psychological problems. While benzos do not typically produce euphoric effects common in recreational drugs, they are frequently misused for their calming and sedative

properties. Fatal overdoses are uncommon when using benzos in isolation. However, benzos interact strongly with depressants such as alcohol and opioids. These interactions amplify the recreational properties of the drugs, but they also greatly increase the probability of respiratory depression and death. In fact, opioids were involved in the vast majority of benzodiazepine overdose deaths (Ruhm, 2019).

Stimulants Stimulants refer to a broad class of legal and illegal drugs that act on the central nervous system to increase alertness and energy. Stimulants range from ubiquitous drugs such as caffeine to prescription drugs including amphetamine (brand name "Adderall") and methylphenidate (brand name "Ritalin") to Schedule I drugs such as MDMA. Prescription stimulants are commonly used to treat attention deficit hyperactivity disorder (ADHD), but are also used for their recreational effects. Taken in high doses, stimulants can produce intense feelings of euphoria. Stimulants are also used as appetite suppressants and as "study-drugs", enhancing the user's ability to focus for long periods of time. However, stimulants use can also lead to agitation and anxiety, among other adverse behavioral effects. Physically, stimulants can elevate blood pressure to dangerous levels and lead to heart attack or stroke.

Benzodiazepines and stimulants are widely consumed. Figure 1 displays the rates of use and misuse for various drugs from the 2015-2016 wave of the National Survey on Drug Use and Health (NSDUH). White bars represent any use of the drug (including legitimate medical use), while gray bars represent misuse. Approximately 11 percent of respondents reported using benzodiazepines at some point in the previous year. A little under one-fifth of these users reported misusing benzodiazepines, that is, use without a legitimate prescription or for the sole purpose of recreation. The most commonly used and abused benzodiazepine was alprazolam, commonly sold under the brand name Xanax. Stimulant use, on the other hand, was reported by about 5 percent of respondents, a little under half the fraction of benzodiazepine use. However, despite the lower overall prevalence, stimulants were misused at nearly the exact same rate as benzodiazepines. For the sake of comparison, this figure also includes analogous numbers for the two most commonly prescribed opioids, oxycodone and hydrocodone. Over 35 percent of respondents reported consuming these opioids at some point in the last year, with about 5 percent reporting misuse. Therefore, while opioid use and misuse is more prevalent than the use and misuse of benzodiazepines or stimulants, the fraction of users who misuse the drug is higher for these latter drug classes.

3 Data

3.1 Prescribing Data

ARCOS Our primary dataset measuring the distribution of various stimulants is the Automated Reports and Consolidated Ordering System (ARCOS). These data are reported at the state-by-quarter level by the Drug Enforcement Agency (DEA). They are constructed from reports sent to the DEA by distributors and manufacturers, who are required by law to report all transactions of certain controlled substances.[19]

We obtain information about the weight in grams of amphetamine, methylphenidate, and lisdexamfetamine distributed to each state for each quarter from 2008-2018.[20] Although these data do not directly measure the amount of each substance consumed in each period, prior research has shown that measures of drug distribution from ARCOS are highly correlated with measures of consumption from other datasets (Beheshti, 2021). We also create an aggregate measure of stimulant supply by pooling together each stimulant, weighted by potency. Specifically, we create a measure of amphetamine-equivalent milligrams using the conversion factors listed in Appendix Table A1.

We display the aggregate distribution of each stimulant in Appendix Figure A2. From 2008 to 2017, the per capita supply of amphetamine and lisdexamfetamine more than doubled. In contrast, the quantity of methylphenidate distributed in each quarter remained relatively constant over this period. Since the DEA does not track benzodiazepine sales, we are unable to examine trends in benzodiazepine shipments over time.

Medicaid We use the Medicaid State Drug Utilization Data from the Centers for Medicare and Medicaid Services (CMS) over the period 2008–2017. The data provide state-quarter level counts of prescriptions reimbursed by Medicaid (both fee-for-service and managed care) separately by National Drug Code (NDC). We first categorize NDCs into generic types using the product name and then collapse the NDC-state-quarter aggregate prescription records into generic type-state-

[19] Title 21, United States Code, Section 827(d)(1), and Title 21, Code of Federal Regulations, Section 1304.33.

[20] There exist ARCOS reports back to 2000, although prior to 2008 different forms of amphetamines are reported separately, making comparisons prior to 2008 difficult.

year level data.[21][22]

For benzodiazepines, we include in our analysis the generic types alprazolam, clonazepam, lorazepam, diazepam, and temazepam; for stimulants, we include amphetamine, methylphenidate, and lisdexamfetamine. Our outcome of interest is the number of prescriptions per 100 Medicaid enrollees for each generic type of benzodiazepine and stimulant.[23] We also create an aggregate measure of benzodiazepine prescribing by adding together the number of each type of benzodiazepine prescription.[24] Data on Medicaid enrollment are obtained from the Kaiser Family Foundation.[25] We show time series figures of the rates of stimulant and benzodiazepine prescriptions in panels (a) and (b) of Appendix Figure A3, respectively.[26]

We present summary statistics on each of our primary outcome variables in Table 2. The odd-numbered columns display the average value across all years from 2008 to 2017, while the even-numbered columns display the associated standard deviations. The first two columns use data from the entire sample, while columns (3) and (4) show only those states that adopted a MA PDMP at some point in our sample period. Likewise, columns (5) and (6) present summary statistics for states which did not adopt a MA PDMP until 2018. This table also includes demographic information such as age and race compositions, which we include as control

[21]For each NDC-state-quarter record, the Medicaid State Drug Utilization Data provide the first 10 characters of product name that is approved by the Food and Drug Administration (FDA). A product name contains either a generic name or a brand name. Using this product name, we categorize NDCs into generic types. In Appendix Table A2, we list brand names for each generic type that we use for our categorization. The list of brand names is adapted from FDA and several other sources. We do not list the brand names if no corresponding records are included in the 2008–2017 Medicaid State Drug Utilization Data. Note that we do a partial string matching, so any product names that contain a given brand name are included in our sample. For example, both the product names "XANAX XR" and "XANAX .25M" are identified by the brand name "XANAX" and thus included in our sample.

[22]For each generic type, we only include state-years that consistently report in all four quarters (around 96.5% of all generic-state-year observations). CMS suppresses NDC-state-quarter observations if there are less than eleven counts. We replace suppressed observations with zero, but results are similar if we set these values to be five instead.

[23]Liang and Shi (2019) analyze the impact of MA PDMPs on the prescribing of benzodiazepine and find similar patterns across number of prescriptions, dosage of prescriptions, and spending on benzodiazepine prescriptions. In our analysis, we focus on the number of prescriptions.

[24]We set the value of the aggregate measure as missing if information on any of these types is missing.

[25]We use monthly Medicaid and CHIP enrollment measured in June. Data on total monthly Medicaid and CHIP enrollment over the period June 2014–June 2017 are taken from: https://www.kff.org/health-reform/state-indicator/total-monthly-medicaid-and-chip-enrollment/?currentTimeframe=0&sortModel=%7B%22colId%22:%22Location%22,%22sort%22:%22asc%22%7D, last accessed March 7, 2022. Data on total monthly Medicaid enrollment over the period June 2008–June 2013 are taken from: https://www.kff.org/medicaid/issue-brief/medicaid-enrollment-june-2013-data-snapshot/view/print/, last accessed March 7, 2022. Data on total monthly CHIP enrollment over the period June 2008–June 2013 are taken from: https://www.kff.org/medicaid/issue-brief/chip-enrollment-june-2013-data-snapshot/view/print/, last accessed March 7, 2022.

[26]The sudden decrease in benzodiazepine prescriptions from 2012 to 2013 is attributable to Medicare Part D beginning to cover benzodiazepines. This led to a decline in benzodiazepine prescriptions paid for by Medicaid among those enrolled in both programs (Liang et al., 2021). Dropping states treated in 2012 and 2013 does not significantly affect our results.

variables.

NSDUH We obtain data on the use and misuse of benzodiazepines and stimulants from the National Survey on Drug Use and Health (NSDUH). NSDUH has collected nationally representative data on prescription drug use and misuse, among the randomly sampled non-institutionalized US civilians aged 12 or older. We construct the measures for the overall use and misuse of stimulants and benzodiazepines among NSDUH respondents over age 12 in the 2015 and 2016 survey years (N=114,043).[27]

We display these rates in Figure 1, along with rates of opioid (mis)use for comparison.[28] The light bars show the fraction of respondents over the age of 12 who report any use of the drug, including legitimate medical use. The dark bars indicate the fraction who explicitly report misusing the drug. The second column indicates that around 11.3 percent of individuals used benzodiazepines in 2015-2016. Approximately 18.3 percent of those reported misusing the drug. The next four columns break out this estimate by the four most common types of benzodiazepines. Rates of stimulant use are much lower, around five percent. However, nearly 36.5 percent of this was misuse, putting overall stimulant misuse almost identical to the overall rate of benzodiazepine misuse. Both of these are lower than the corresponding rates for opioids, consistent with the larger research focus on opioid misuse.

3.2 PDMPs

Table 1 shows the effective dates of the laws used in this paper, taken from Sacks et al. (2021). Appendix Figure A1 presents the trends in the total number of states with MA PDMPs. By the end of 2017, 26 states had passed MA PDMP laws.

4 Research Design

Our empirical strategy for estimating the causal impact of MA PDMPs exploits variation in the timing of adoption across states. We begin by considering a static two-way fixed effects (TWFE)

[27] We focus on the 2015–2016 data to construct the consistent measures. NSDUH survey was partially redesigned in 2015 to collect more detailed and complete information on the use and misuse of prescription drugs, including stimulants and benzodiazepines. Prior to 2015, NSDUH definition of prescription drug misuse was limited to "nonmedical use," but the 2015 definition of misuse was revised to use a drug "in any way a doctor did not direct." For more details, see: https://www.samhsa.gov/data/sites/default/files/NSDUH-TrendBreak-2015.pdf, last accessed April 24, 2022.

[28] We proxy for this with (mis)use of either oxycodone or hydrocodone. Inconsistency across questions for different drugs prevents us from including a broader set of opioids. These two drugs make up the majority of opioid prescriptions in the United States, and are the most commonly misused prescription opioids.

model of the form:

$$Y_{st} = \alpha_s + \gamma_k + \beta \cdot \mathbf{1}(MA\ PDMP_{st}) + \epsilon_{st}, \tag{1}$$

where Y_{st} is the outcome variable measured at the state-by-year level, and α_s and γ_t are state and year fixed effects, respectively. The variable $\mathbf{1}(MA\ PDMP_{st})$ is an indicator that is equal to one for all state-year combinations in which a MA PDMP is in effect, and zero otherwise.

There are two important limitations to this model. First, a burgeoning literature has shown that the β parameter from equation 1 may be biased in settings with staggered adoption and dynamic and/or heterogeneous treatment effects (e.g., Goodman-Bacon, 2021).[29] Specifically, Goodman-Bacon (2021) shows that the β coefficient from equation 1 is a weighted average of all possible two-group/two-period difference-in-differences (DD) estimators in the data, with no guarantee of non-negative weights. These DD estimators include three distinct classes: (1) treated units with never treated units as a control, (2) early treated units with late treated units as a control, and (3) late treated units with early treated units as a control. Bias arises in (2) and (3) in the presence of dynamic or heterogeneous treatment effects, as the control groups are contaminated by the treatment. To examine the source of identification and potential bias, we estimate equation 1 and compute the Goodman-Bacon decomposition for each outcome. The full decomposition results are presented in Appendix Figures A4–A6. The results indicate that our overall β estimate incorporates a large fraction of the comparisons from classes (2) and (3), often called "forbidden comparisons." For example, the decomposition for the aggregate supply of stimulants reveals that slightly over 30 percent of the weight is attributed to these forbidden comparisons.

Second, the static TWFE formulation will average over potentially important dynamic treatment effects. Prior research has found delayed effects of MA PDMPs on a variety of outcomes, suggesting that we may expect important dynamics in our context.

In order to account for both of these issues, we present event study estimates following the methodology from Callaway and Sant'Anna (2021). Specifically, we define the average treatment effect on the treated in time period t for the cohort first treated in time period g as:

$$ATT(g,t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i = \infty] \tag{2}$$

In words, the first term represents the expected difference between the outcome from period t to

[29] Sun and Abraham (2021) show that the lead and lag coefficients from dynamic "event study" formulations of equation 1 suffer from similar problems. See Roth et al. (2023) for an overview of this literature.

period $g-1$ for cohort g (i.e., $G_i = g$) minus the same difference for the never treated cohort (i.e., $G_i = \infty$). Each of these terms are estimated using their sample analogs.

In order to present event study type estimates, we aggregate the ATTs e periods away from treatment as follows:

$$ATT_e = \sum_g w_g \cdot ATT(g, g+e) \tag{3}$$

where w_g is the weight assigned to cohort g.[30] We then plot the ATT_e estimates for values of e between -5 and 5.[31] These ATT_e terms indicate the difference in outcome between treatment and control states in period e, relative to the last pre-policy period.

We trim all post-periods after the fifth ($e > 5$) and all pre-periods more than nine years prior ($e < -9$). Observations are weighted by state population (Medicaid enrollment) in our analysis for stimulant distribution (Medicaid prescribing) outcomes. Standard errors are clustered at the state level.

Our analysis sample consists of 24 treatment states that implemented MA PDMPs between 2009 and 2017 and 22 control states that did not implement the policy until 2018.[32] Since states implemented the policy with different timing, our sample of states and years is unbalanced in relative periods.

The key identifying assumption in this model is that, absent the implementation of a MA PDMP, control and treatment states would have trended in parallel. We assess the plausibility of this assumption by plotting the ATT_e coefficients which allows us to examine whether treatment and control states were trending in parallel prior to treatment.

[30] We use the `csdid` command in Stata to carry out our estimation (Rios-Avila et al., 2023). It is worth noting that, by default, this command uses the period immediately prior to treatment as the base period for all post-treatment time periods, while using the previous time period as the base for all pre-treatment time periods. This produces potentially misleading event studies (Roth, 2024). We use the `long2` option in all of our event studies, which sets the base for all time periods as the period immediately preceding treatment. This provides event study estimates with the usual interpretation.

[31] Note that, by construction, $ATT(x, x-1) = 0$, which implies $ATT_{-1} = 0$.

[32] As shown in Table 1, 29 states implemented a MA PDMP until 2018. Since our data covers 2008–2017, our analysis focuses on states that adopted a mandate during our sample period. We drop four states that enacted the law outside the period 2008–2017 (i.e., either pre-2008 or 2018), but our results are similar if we include these "already treated" or "not yet treated" states. In addition, we drop one treatment state which implemented a MA PDMP in 2008, for which we do not observe any pre-treatment period in our data. Our results are robust to including this state. Our final analysis sample consists of 24 treatment states and 22 control states.

5 Results

A rapidly growing literature has considered the effect of MA PDMPs on opioid prescribing and related outcomes. Although voluntary access PDMPs had limited efficacy in reducing prescriptions, studies focusing on MA PDMPs have shown stronger effects.[33] Given the consistent finding of this prior work, we do not discuss our replication of this finding here.[34] We instead focus our discussion on stimulants and benzodiazepines, drug categories that have not been considered to the same extent as opioids.

Stimulants We first consider the effect of MA PDMPs on stimulant prescribing. There are three different types of stimulants included in the ARCOS dataset: amphetamine, methylphenidate, and lisdexamfetamine. We present the results from equation 3 for each of these outcomes, as well as our measure aggregating across these three types, in Figure 2.[35] We consider our aggregate measure in panel (a). Prior to the implementation of a MA PDMP, each of the coefficients is small in magnitude and statistically indistinguishable from zero. This pattern of coefficients lends plausibility to our identifying assumption, that treated states would have trended in parallel to untreated states in the absence of treatment. Immediately after the implementation of a MA PDMP, however, the coefficients become negative and continue to grow in magnitude as time passes. After five years, the coefficient is equal to -1.89. Relative to the mean of 9.179 one year before treatment, this is a decrease of 20.6 percent. In the remaining panels, we present the results for each type of stimulant separately. We consider the number of grams of amphetamine per 100 individuals in panel (b). The pattern of coefficients is nearly identical to panel (a), revealing no evidence of pre-existing trends. After five years, the coefficient is equal to -1.14, a decrease of 20.7 percent. In panel (c), we turn our attention to methylphenidate. Five years after the treatment begins, the point estimate of -0.65 indicates a reduction of 20.3 percent. Finally, we consider lisdexamfetamine in panel (d). The pattern is again nearly identical to what we observed in panels (b) and (c), and indicates a

[33] See Maclean et al. (2020) for a review.
[34] These results are available upon request, and fall within the range of estimates in the existing literature.
[35] The estimates are listed in table form in Table 3.

reduction of about 21.9 percent five years after the implementation of a MA PDMP.[36]

Next, we consider an alternate measure of prescribing using data from Medicaid. This measure captures the number of prescriptions written per 100 enrollees. By measuring the number of prescriptions per enrollee as opposed to the weight of the drug distributed per capita, we complement our measure of intensive margin prescribing with a measure focusing on extensive margin prescribing. However, since we are now examining Medicaid enrollees as opposed to the general population, we cannot rule out any differences in our results being due to differences in the sample composition rather than the difference in the intensive versus extensive margin. The results from this exercise are shown in Appendix Figure A7.[37] Beginning with panel (a), we observe a point estimate of -7.52 five years after adoption, relative to a mean of 20.5, a 36.7 percent decrease. This is somewhat larger than what we observe when using our main measure of prescribing. Panels (b) and (d) show results that are quite similar to Figure 2, although substantially larger in magnitude. Examining panel (c), however, we find results that are qualitatively different. Each of the post-period estimates are statistically insignificant, and close to zero.

Overall, these results demonstrate a consistent reduction in stimulant prescribing after the implementation of MA PDMPs, similar to what is typically reported in studies that examine opioid prescribing. This is consistent with either information provision— prescribers learning about potential misuse or diversion— or hassle costs— prescribers simply not wanting to engage with the PDMP. The overall welfare effects are unclear, however, as we cannot separately identify reductions in unnecessary prescribing from reductions in appropriate prescribing.

Benzodiazepines Next, we turn our attention to benzodiazepines. We consider these drugs for three reasons. First, we are inherently interested in benzodiazepines due to the increased frequency of overdose deaths involving benzodiazepines. Second, given their less stringent regulatory status, we are interested in whether MA PDMPs have differential effects relative to opioids and stimulants. Finally, benzodiazepines act as both a complement to other recreational drugs (e.g, enhancing

[36] In comparison, Meinhofer (2018) finds that stimulant grams decrease by 10 percent in the first two years of MA PDMP. Our results indicate that these effects continue to grow up to five years after MA PDMP implementation, highlighting the benefits of using a longer panel. One potential explanation for this increasing effect size is that both stimulant users and providers need time to adjust their behavior. Comparable dynamics are found in previous studies examining the health and economic impacts of opioid-related policies (Alpert et al., 2018; Kim, 2021; Park and Powell, 2021; Powell and Pacula, 2021; Beheshti, 2021), although the delay in our setting is particularly pronounced. In results not shown here, we obtain similar results dropping states that are treated late in the sample period, alleviating concerns of our results being driven by compositional changes over time.

[37] We report the coefficients in table form in Appendix Table A3.

the euphoric effects of opioids) as well as a substitute (e.g., alleviating the negative symptoms of withdrawals). There are therefore ambiguous theoretical effects of MA PDMPs.

Since benzodiazepines are Schedule IV drugs, benzodiazepine shipments are not tracked by the DEA. We therefore only consider prescriptions per 100 Medicaid enrollees. We present the estimates from equation 3 for all benzodiazepines pooled together in panel (a) of Figure 3, followed by alprazolam, clonazepam, lorazepam, diazepam, and temazepam separately in panels (b) through (f), respectively.[38] In contrast to the stimulant results, the pattern of estimates suggest that benzodiazepine prescriptions were not substantially altered by the passage of MA PDMPs. For example, in panel (a) each of the estimates is statistically indistinguishable from zero, with the exception of a single positive coefficient in the first period post policy implementation. Results are broadly similar for the remaining panels.

Overall, these figures show that, contrary to what we observed for stimulants and what has commonly been found for opioids, benzodiazepine prescriptions did not fall following the implementation of a MA PDMP. This is

6 Conclusion

Prescription drug monitoring programs have emerged as one of the key tools that policy makers have used to combat surging drug overdose death rates. A rapidly growing literature has examined the effectiveness of PDMPs on opioid prescribing, misuse, and overdose deaths. Other work has considered downstream effects including heroin-related crime and overdose deaths, as well as labor market conditions. However, the literature considering the effect of these programs on the consumption of other drugs is still limited.

In this paper, we expand upon this literature by considering how mandatory access PDMPs have affected the consumption of prescription stimulants and benzos. Using a variety of econometric specifications, we find robust evidence that MA PDMPs led to decreases in the availability of prescription stimulants. In contrast, we find no evidence of substantial changes in benzodiazepine prescribing.

Our paper highlights two important aspects of mandatory access PDMPs. First, we show that PDMPs have effects on non-opioid drugs. These effects exist even for drugs that are not explicitly included in the PDMP. Next, this paper shows that the effects differ across drug types. We find

[38] Estimates are shown in table form in Table 4.

qualitatively different responses for stimulants and benzodiazepines. This is consistent with important interaction effects and substitution patterns across drug types.

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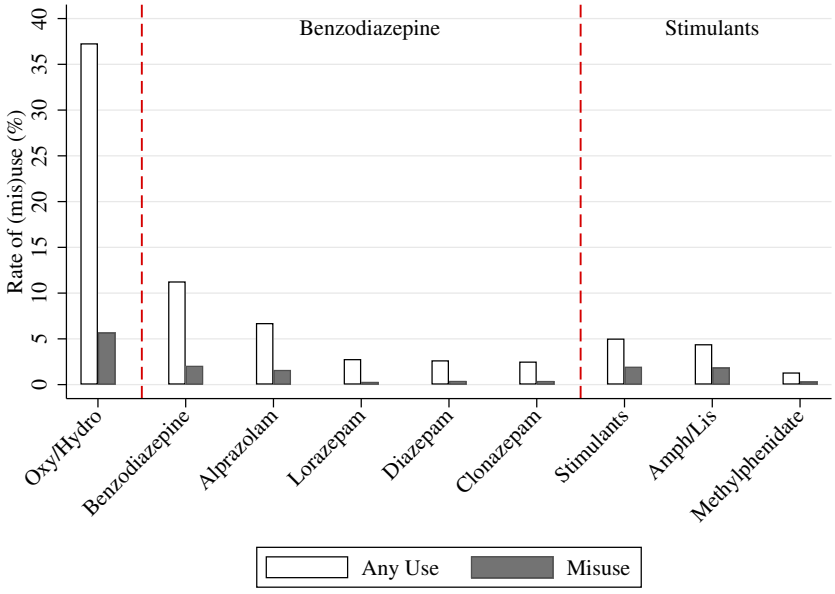
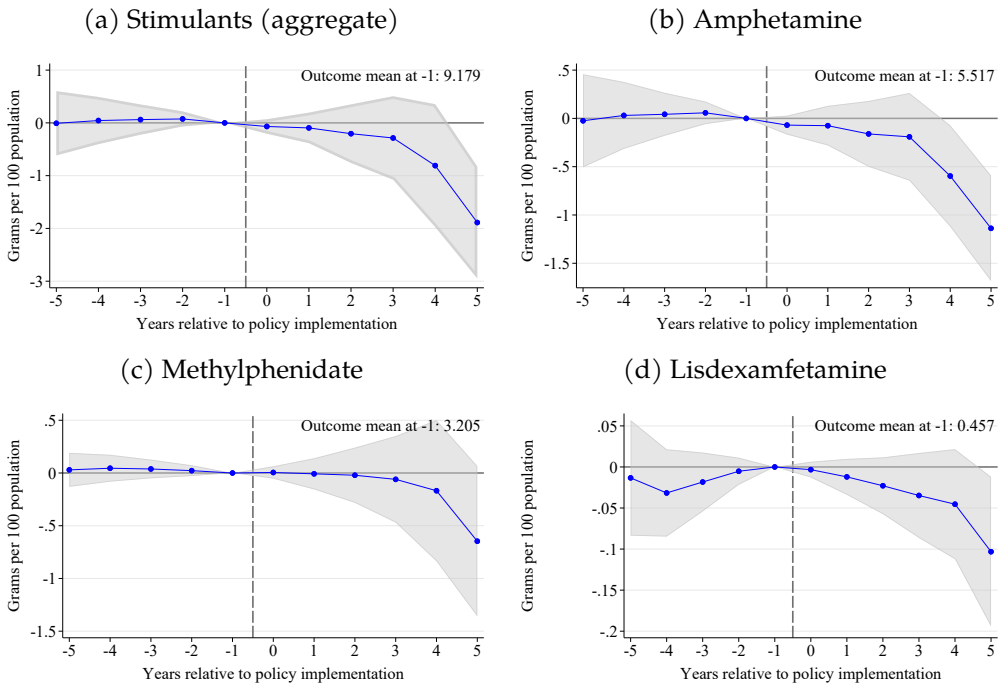
7 Figures and Tables

Figure 1: Benzodiazepine and Stimulant Use and Misuse



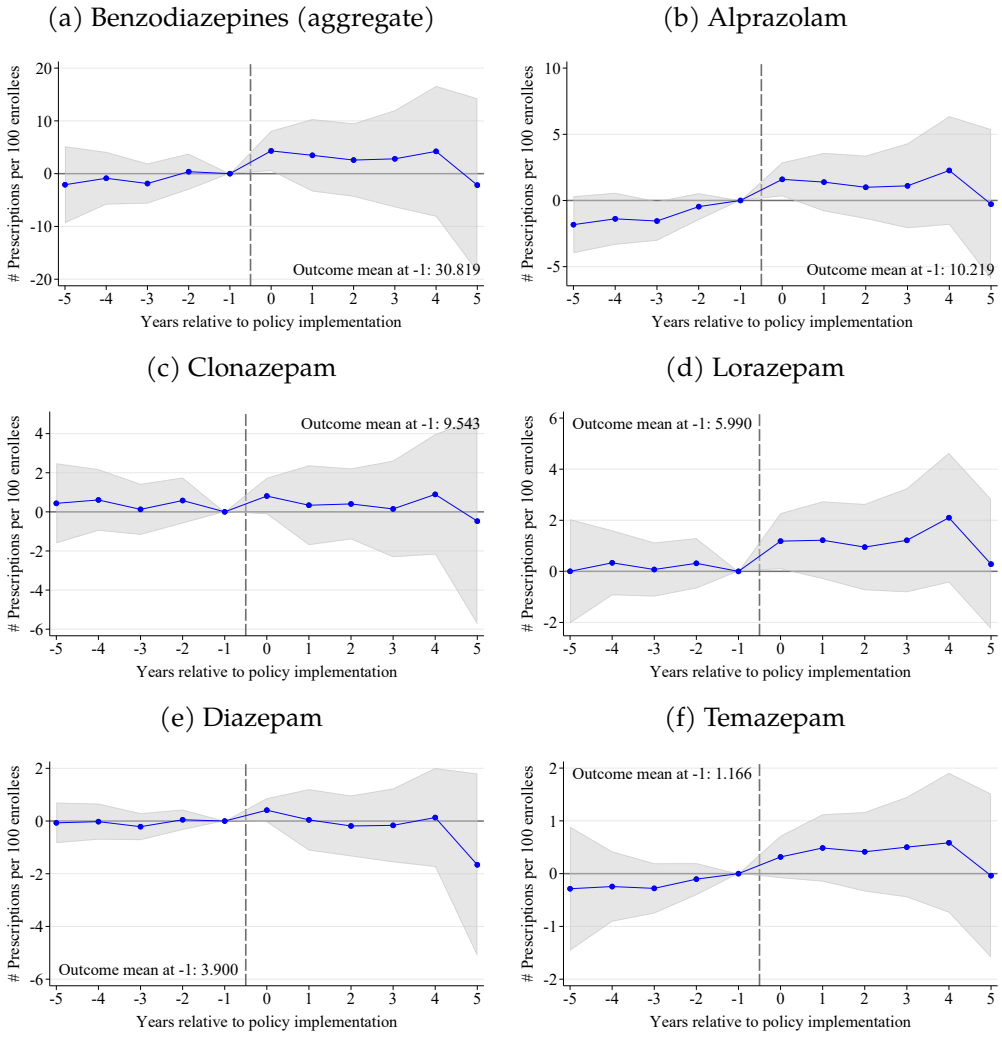
Notes: The figure presents the prevalence of use and misuse of prescription benzodiazepines and stimulants among 2015 and 2016 NSDUH respondents over age 12 (N=114,043).

Figure 2: Effects of MA PDMPs on Stimulant Distribution (ARCOS Data)



Notes: The figure presents the ATT_e estimates and 95% confidence intervals following the methodology from Callaway and Sant'Anna (2021) for each of the years before and after policy implementation. The year before the policy implementation is the omitted category. Standard errors are clustered at the state level. Each dependent variable is measured in amphetamine-equivalent grams per 100 population.

Figure 3: Effects of MA PDMPs on Benzodiazepine Prescribing (Medicaid Data)



Notes: The figure presents the ATT_e estimates and 95% confidence intervals following the methodology from Callaway and Sant'Anna (2021) for each of the years before and after policy implementation. The year before the policy implementation is the omitted category. Standard errors are clustered at the state level. Each dependent variable is the number of prescriptions per 100 Medicaid enrollees.

Table 1: State Laws

State	Effective Date
Alabama	
Alaska	2017m7
Arizona	2017m10
Arkansas	2017m1
California	2018m4
Colorado	
Connecticut	2015m10
Delaware	2012m3
District of Columbia	
Florida	
Georgia	2014m7
Hawaii	
Idaho	
Illinois	2018m1
Indiana	2014m7
Iowa	
Kansas	
Kentucky	2012m7
Louisiana	2008m1
Maine	
Maryland	2018m7
Massachusetts	2014m7
Michigan	
Minnesota	2017m1
Mississippi	
Missouri	
Montana	
Nebraska	
Nevada	2007m10
New Hampshire	2016m1
New Jersey	2015m11
New Mexico	2012m9
New York	2013m8
North Carolina	
North Dakota	
Ohio	2012m3
Oklahoma	2011m3
Oregon	
Pennsylvania	2017m1
Rhode Island	2016m6
South Carolina	2017m5
South Dakota	
Tennessee	2013m7
Texas	
Utah	2017m5
Vermont	2015m5
Virginia	2015m7
Washington	
West Virginia	2012m6
Wisconsin	
Wyoming	

Notes: This table reports the start dates of state laws enacted until December 31, 2018. The dates are obtained from Sacks et al. (2021).

Table 2: Summary Statistics

	All States		Treated States		Control States	
	Mean	SD	Mean	SD	Mean	SD
Outcome (mean, 2008–2017)	(1)	(2)	(3)	(4)	(5)	(6)
The legal supply of stimulants (amphetamine equivalent grams) per 100 population						
Stimulants (aggregate)	8.543	(2.538)	8.637	(2.632)	8.435	(2.429)
Amphetamine	5.041	(1.856)	5.075	(1.913)	5.002	(1.792)
Methylphenidate	3.091	(0.870)	3.171	(0.878)	3.001	(0.854)
Lisdexamfetamine	0.410	(0.197)	0.392	(0.203)	0.431	(0.188)
The number of prescriptions per 100 Medicaid enrollees						
Stimulants (aggregate)	19.790	(9.629)	18.560	(9.593)	21.505	(9.441)
Amphetamine	5.249	(3.713)	4.952	(3.523)	5.635	(3.923)
Methylphenidate	9.197	(4.426)	9.134	(4.461)	9.280	(4.390)
Lisdexamfetamine	4.947	(3.645)	4.332	(3.525)	5.809	(3.645)
Benzodiazepine (aggregate)	32.936	(17.460)	33.895	(17.250)	31.745	(17.688)
Alprazolam	10.542	(6.341)	10.406	(5.950)	10.716	(6.817)
Clonazepam	9.259	(5.193)	9.918	(5.574)	8.393	(4.515)
Lorazepam	6.757	(4.890)	6.766	(5.025)	6.745	(4.720)
Diazepam	3.972	(2.426)	3.947	(2.535)	4.004	(2.283)
Temazepam	1.617	(1.601)	1.407	(1.316)	1.878	(1.866)
Age and race/ethnicity compositions						
0–14	0.197	(0.018)	0.195	(0.016)	0.200	(0.019)
15–24	0.139	(0.007)	0.139	(0.006)	0.139	(0.007)
25–44	0.261	(0.014)	0.260	(0.011)	0.262	(0.017)
45–64	0.261	(0.015)	0.264	(0.015)	0.257	(0.014)
65–84	0.123	(0.018)	0.123	(0.014)	0.123	(0.022)
85+	0.019	(0.004)	0.019	(0.004)	0.018	(0.004)
Non-Hispanic White	0.667	(0.135)	0.691	(0.115)	0.640	(0.151)
Non-Hispanic Black	0.130	(0.077)	0.134	(0.074)	0.125	(0.080)
Hispanic	0.142	(0.112)	0.113	(0.084)	0.175	(0.130)
Observations	460		240		220	
Number of states	46		24		22	

Notes: This table presents average characteristics for all states (columns 1–2), treated states (columns 3–4), and control states (columns 5–6) included in our baseline analysis. The table reports the mean and standard deviation. Each panel describes the balanced panel of state-years from 2008 to 2017. For stimulant supply outcomes and state demographic characteristics, observations are weighted by state population. For Medicaid prescribing outcomes, observations are weighted by Medicaid enrollment. The first two columns include all states, columns 3–4 includes the 24 treated states that implemented a MA PDMP between 2009–2017. The last two columns include the 22 control states that did not implement a MA PDMP until December 2018.

Table 3: Effects of MA PDMPs on Stimulant Distribution

	Aggregate (1)	Amphetamine (2)	Methyl. (3)	Lisdexamf. (4)
Dependent variable: amphetamine equivalent stimulant grams per 100 population				
Immediate effect	-0.07	-0.07	0.00	-0.00
	(0.07)	(0.05)	(0.03)	(0.00)
1-year effect	-0.10	-0.08	-0.01	-0.01
	(0.15)	(0.10)	(0.08)	(0.01)
2-year effect	-0.20	-0.16	-0.02	-0.02
	(0.28)	(0.17)	(0.13)	(0.02)
3-year effect	-0.29	-0.19	-0.06	-0.03
	(0.40)	(0.23)	(0.21)	(0.03)
4-year effect	-0.81	-0.60**	-0.17	-0.05
	(0.59)	(0.27)	(0.34)	(0.03)
5-year effect	-1.89***	-1.14***	-0.65*	-0.10**
	(0.54)	(0.28)	(0.36)	(0.05)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Time-varying covariates	N	N	N	N
Outcome mean at -1	9.179	5.517	3.205	0.457
Observations	414	414	414	414

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect obtained following the methodology from Callaway and Sant'Anna (2021). We only report the estimates for the post-period ATT_es above for brevity. The year before the policy implementation is the omitted category. Observations are weighted by state population. In column (1), the dependent variable is aggregate amphetamine equivalent stimulant grams per 100 population. In columns (2)-(4), the dependent variables are the amphetamine equivalent grams of amphetamine, methylphenidate, and lisdexamfetamine per 100 population, respectively. The mean of each dependent variable is calculated using observations from the treated sample measured at the last pre-policy period. Standard errors are clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

Table 4: Effects of MA PDMPs on Benzodiazepine Prescribing

	Aggregate (1)	Alprazolam (2)	Clonazepam (3)	Lorazepam (4)	Diazepam (5)	Temazepam (6)
Dependent variable: Number of benzodiazepine prescriptions per 100 enrollees						
Immediate effect	4.31**	1.60**	0.81*	1.18**	0.41*	0.32
	(1.94)	(0.65)	(0.47)	(0.56)	(0.23)	(0.20)
1-year effect	3.48	1.39	0.34	1.22	0.04	0.49
	(3.50)	(1.12)	(1.04)	(0.77)	(0.59)	(0.32)
2-year effect	2.58	1.00	0.40	0.95	-0.19	0.41
	(3.54)	(1.22)	(0.92)	(0.86)	(0.59)	(0.38)
3-year effect	2.81	1.11	0.15	1.22	-0.16	0.50
	(4.69)	(1.63)	(1.26)	(1.04)	(0.71)	(0.48)
4-year effect	4.23	2.27	0.89	2.10	0.13	0.58
	(6.31)	(2.09)	(1.57)	(1.29)	(0.96)	(0.68)
5-year effect	-2.17	-0.28	-0.48	0.28	-1.66	-0.04
	(8.39)	(2.89)	(2.69)	(1.30)	(1.77)	(0.79)
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Time-varying covariates	N	N	N	N	N	N
Outcome mean at -1	30.819	10.219	9.543	5.990	3.900	1.166
Observations	407	411	412	411	412	408

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect obtained following the methodology from Callaway and Sant'Anna (2021). The year before the policy implementation is the omitted category. Observations are weighted by the number of Medicaid enrollees. In column (1), the dependent variable is the total number of benzodiazepine prescriptions per 100 Medicaid enrollees. In columns (2)–(6), we examine each type of benzodiazepine separately. The regressions include state and year fixed effects, as well as time-varying covariates (age and race compositions). The mean of each dependent variable is calculated using observations from the treated sample measured at the last pre-policy period. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

A APPENDIX - FOR ONLINE PUBLICATION

A.1 Additional Robustness Tests and Analyses

Alternate Econometric Specifications Recent literature has highlighted that traditional difference-in-differences estimates identified on staggered treatment timing can be biased as a result of treatment effect heterogeneity (Goodman-Bacon, 2021). Our Callaway and Sant'Anna (2021) methodology explicitly accounts for this possibility. In this section, we examine the robustness of our results to an alternative estimator proposed by Sun and Abraham (2021).[39] We also show how these estimates compare to a standard TWFE model.

We present the results for stimulants in Appendix Figure A8. The Callaway and Sant'Anna (2021), Sun and Abraham (2021), and TWFE coefficients are all quite close . This suggests that our findings of decreased stimulant prescribing are not an artifact of treatment effect heterogeneity, and not sensitive to the particular estimation method we chose. These consistent findings point towards a reduction in stimulant prescribing as a result of MA PDMPs.

Next, we repeat this exercise for benzodiazepines and show the results in Appendix Figure A9. Interestingly, the effects are more muted using the updated difference-in-differences methodology relative to a traditional TWFE model. The post-period point estimates are generally positive, but the magnitudes are notably smaller using the updated methodology. This highlights the importance of recent methodological improvements.[40]

Additional Controls Another potential policy which could affect our results are triplicate laws. These were programs which required physicians prescribing certain narcotics to file three separate physical prescriptions, adding significant hassle costs and perceived oversight to these prescriptions. Alpert et al. (2022) show that states which had triplicate laws on the books in the 1990s were largely shielded from the opioid crisis. In principle, they could have had different prescribing patterns surrounding stimulants as well. We investigate this by dropping all triplicate states from our analysis and present the results in Appendix Figure A11. As can be seen in the figure, this has almost no effect on our results.

[39] Sun and Abraham (2021) propose the interaction-weighted estimator, which is calculated using a three step procedure. First, cohort-time specific treatment effect is estimated by using a linear two-way fixed effects specification with interactions of relative time dummies with cohort dummies (where cohort is defined based on their initial treatment timing). Second, the weights are estimated by sample shares of each cohort in a given period. Finally, the interaction-weighted estimator is estimated by taking the weighted average over all estimates for cohort-time specific effect obtained from step 1 multiplied by the weight estimates from step 2.

[40] For completeness, we also include the results for stimulant prescriptions in Medicaid in Appendix Figure A10.

Additional Robustness Another concern is that there are unobserved changes in policy and prescribing behavior, unrelated to MA PDMPs, which could be driving our results. In Appendix Figure A12, we explore the robustness of our results to the inclusion of census region-by-year fixed effects. Because the Callaway and Sant'Anna (2021) estimator does not allow for fixed effects, we do this is in two steps. First, we regress each outcome on the full interaction of census region and year fixed effects. Second, we take the residuals from the first regression and use them as the outcome in our CS estimation. This process is mechanically equivalent to including these fixed effects in a TWFE regression. Interestingly, the point estimates barely change. This suggests that secular trends within census regions are unlikely to explain our results.

In a related exercise, we consider how our estimates compare to estimates where we randomize state policy paths. Specifically, we randomly assign treatment to 24 states (matching the number of treated states in our sample), and randomly assign treatment to a year between 2009 and 2017, inclusive. Using these placebo laws, we obtain our CS estimates and plot them as gray lines in Appendix Figure A13. We repeat this exercise 200 times for each outcome, and include the empirical 95 percent confidence interval as solid black lines. Our original estimates using the actual policy timing is shown as blue circles. As one would expect from our main estimates, the latter coefficients fall outside of the confidence intervals, indicating that it would be unlikely to obtain such extreme estimates by chance.

Mortality Given the changes in stimulant and benzodiazepine prescribing behavior documented above, a natural follow-up question is what happens to overdose deaths associated with these drugs? However, there are several factors that complicate this analysis. First, the vital statistics data do not report prescription stimulant deaths separately from other stimulant deaths. This is especially concerning given the high prevalence of methamphetamine use over our study period. Virtually all recreational methamphetamine is produced illicitly, and illicit methamphetamine use accounts for at least 85 to 90 percent of stimulant overdose deaths (Drug Enforcement Agency, 2018).[41] Since MA PDMPs do not directly affect illicit methamphetamine production, this biases us against detecting any mortality changes.[42]

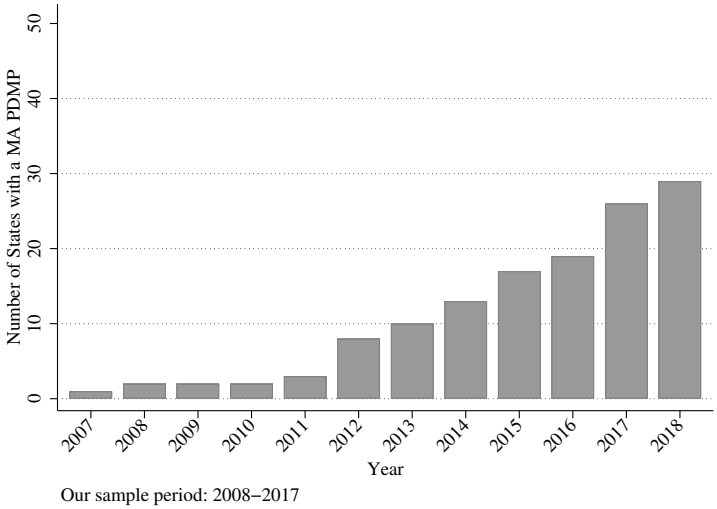
We run into similar complications when examining benzodiazepine overdose deaths. Specifically, overdose deaths solely from benzos are incredibly rare. Almost all benzodiazepine

[41] The ICD-10 code for these deaths is T43.6, "psychostimulants with abuse potential."

[42] In results not presented here, we examine deaths related to stimulants and find no effects within four years of a MA PDMP being implemented.

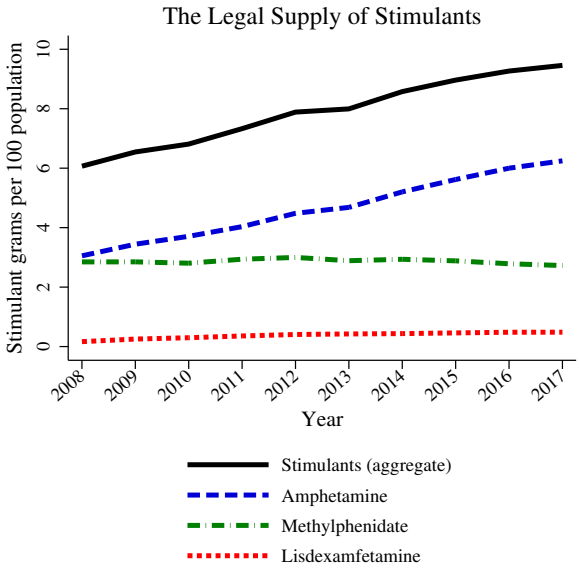
deaths involve other drugs, in particular depressants such as opioids. Since opioid availability is directly affected by MA PDMPs, and moves in the opposite direction as benzodiazepine prescriptions, this makes it difficult to interpret any changes in benzodiazepine overdose deaths. In results not presented here, we separately estimate the effects of MA PDMPs on benzodiazepine overdose deaths that include as well as exclude opioid use, and find somewhat conflicting results. We observe a slight increase in total benzodiazepine-involved mortality, but a decrease in benzodiazepine-only mortality, although both sets of results exhibit notable pre-trends, making it difficult to draw strong conclusions.

Figure A1: Trends in the Number of States with MA PDMPs



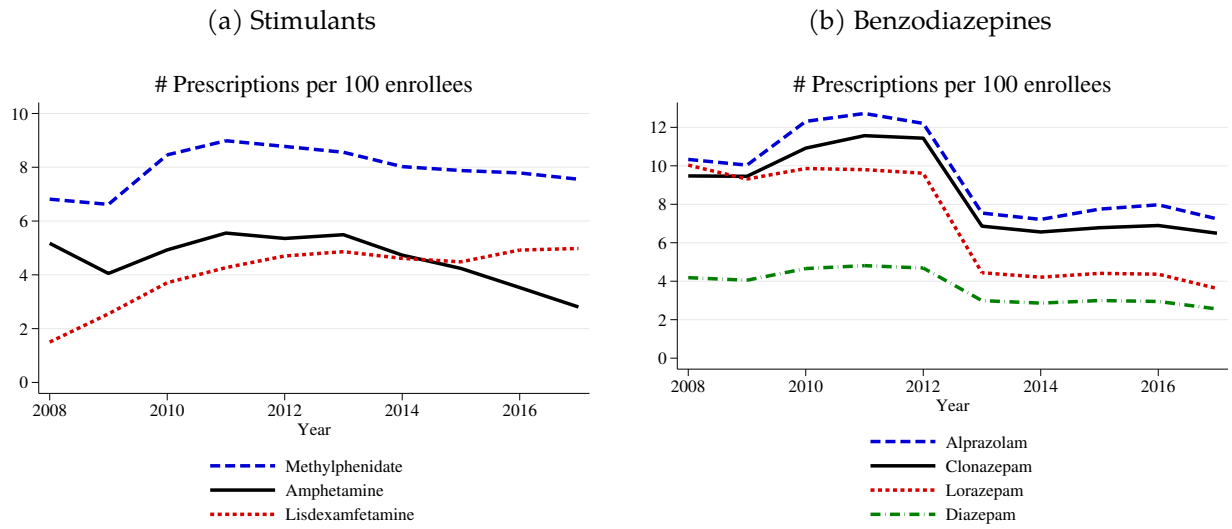
Notes: This figure shows how the number of states with a MA PDMP changes over time. The number of treated states are calculated using the effective dates of MA PDMPs reported in Table 1.

Figure A2: Raw Trends in Stimulant Distribution



Notes: This figure plots the raw trends in the legal supply of stimulant grams per 100 population separately for all stimulants (black solid line), amphetamine (blue dashed line), methylphenidate (green dash-dot line), and lisdexamfetamine (red short-dashed line). Stimulant grams are adjusted for potency and converted into amphetamine-equivalent grams (see Table A1).

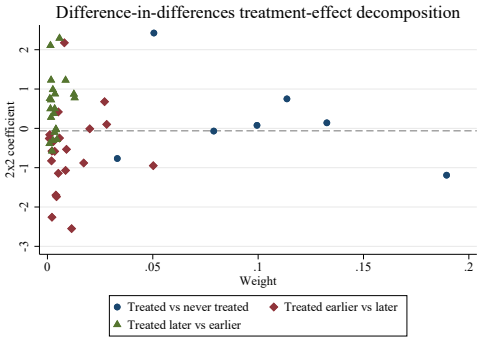
Figure A3: Medicaid RX Time Series



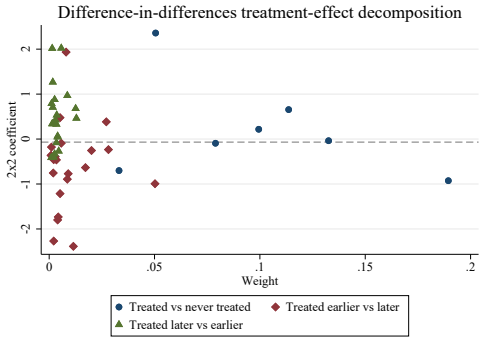
Notes: The figure plots the raw trends in the number of prescriptions per 100 Medicaid enrollees.

Figure A4: Goodman-Bacon Decomposition: Stimulant Distribution (ARCOS Data)

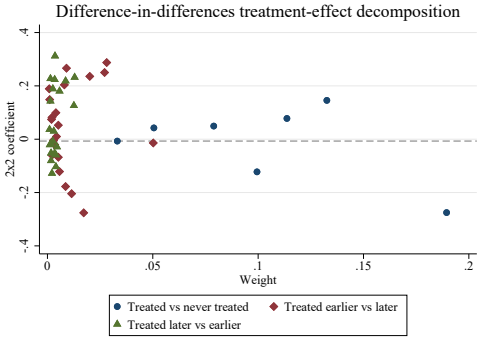
(a) Stimulants (aggregate)



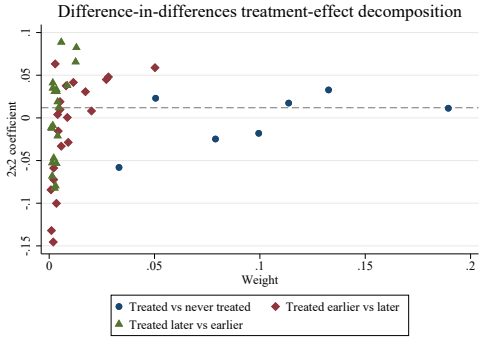
(b) Amphetamine



(c) Methylphenidate



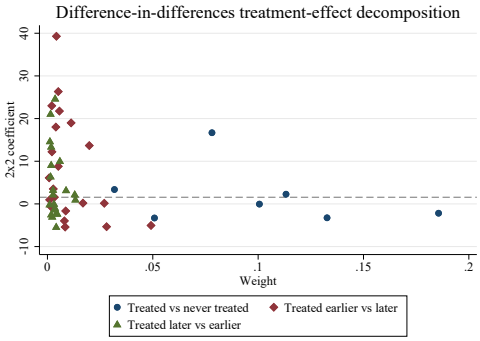
(d) Lisdexamfetamine



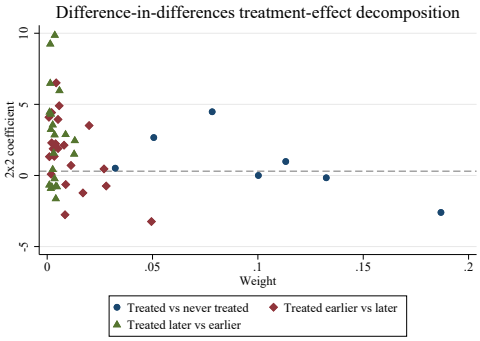
Notes: The figure reports the full decomposition for our ARCOS stimulant distribution outcomes. It shows each of the 2X2 DD coefficients and the associated weights from the Goodman-Bacon decomposition of β from equation 1. Blue circles represent comparisons between treated and never treated, red diamonds show earlier versus later treated comparisons, and green triangles show later versus earlier treated comparisons. Weights are shown allow the x-axis and coefficient values are shown along the y-axis.

Figure A5: Goodman-Bacon Decomposition: Benzodiazepine Prescribing (Medicaid Data)

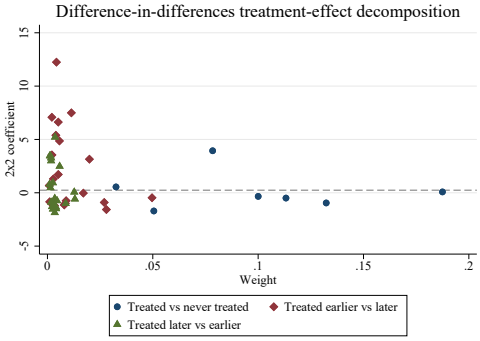
(a) Benzodiazepines (aggregate)



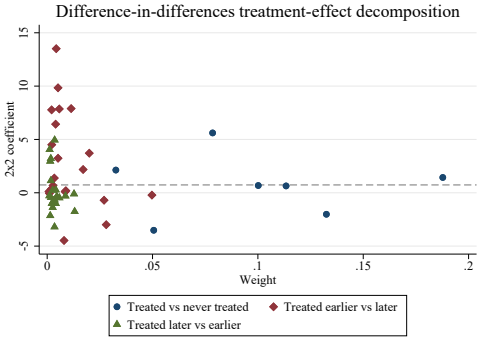
(b) Alprazolam



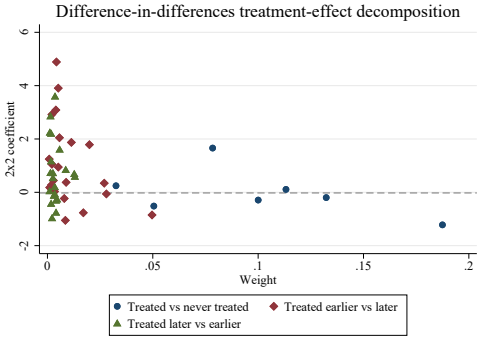
(c) Clonazepam



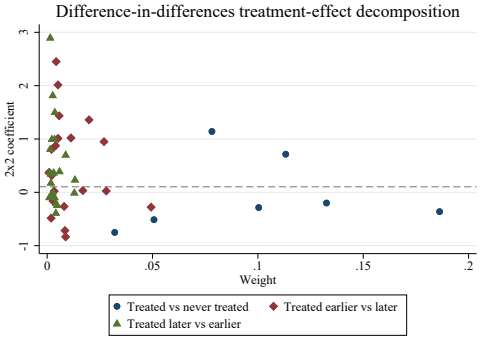
(d) Lorazepam



(e) Diazepam



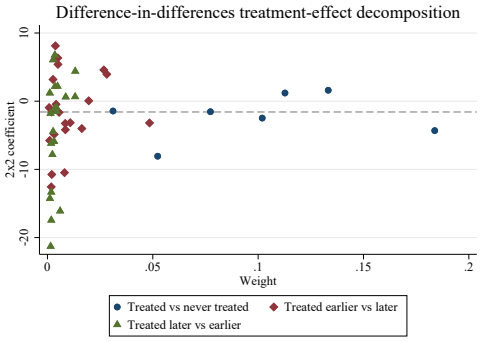
(f) Temazepam



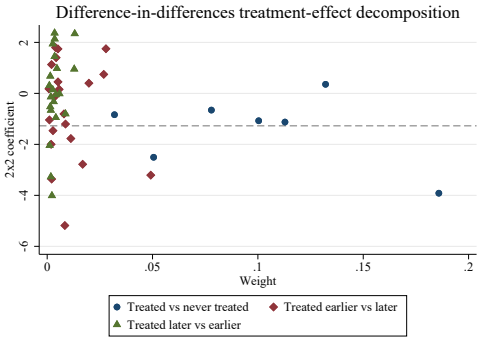
Notes: The figure reports the full decomposition for our Medicaid benzodiazepine prescribing outcomes. It shows each of the 2X2 DD coefficients and the associated weights from the Goodman-Bacon decomposition of β from equation 1. Blue circles represent comparisons between treated and never treated, red diamonds show earlier versus later treated comparisons, and green triangles show later versus earlier treated comparisons. Weights are shown allow the x-axis and coefficient values are shown along the y-axis.

Figure A6: Goodman-Bacon Decomposition: Stimulant Prescribing (Medicaid Data)

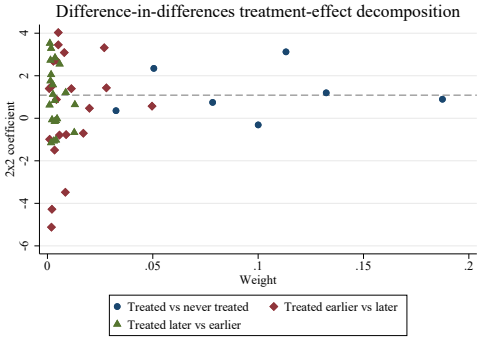
(a) Stimulants (aggregate)



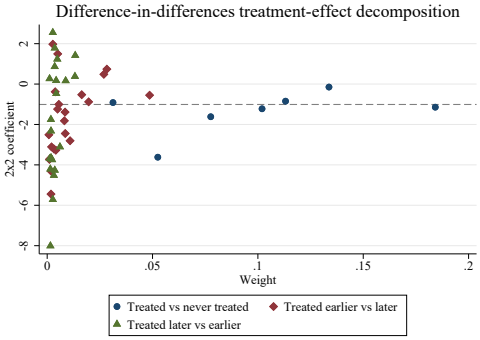
(b) Amphetamine



(c) Methylphenidate

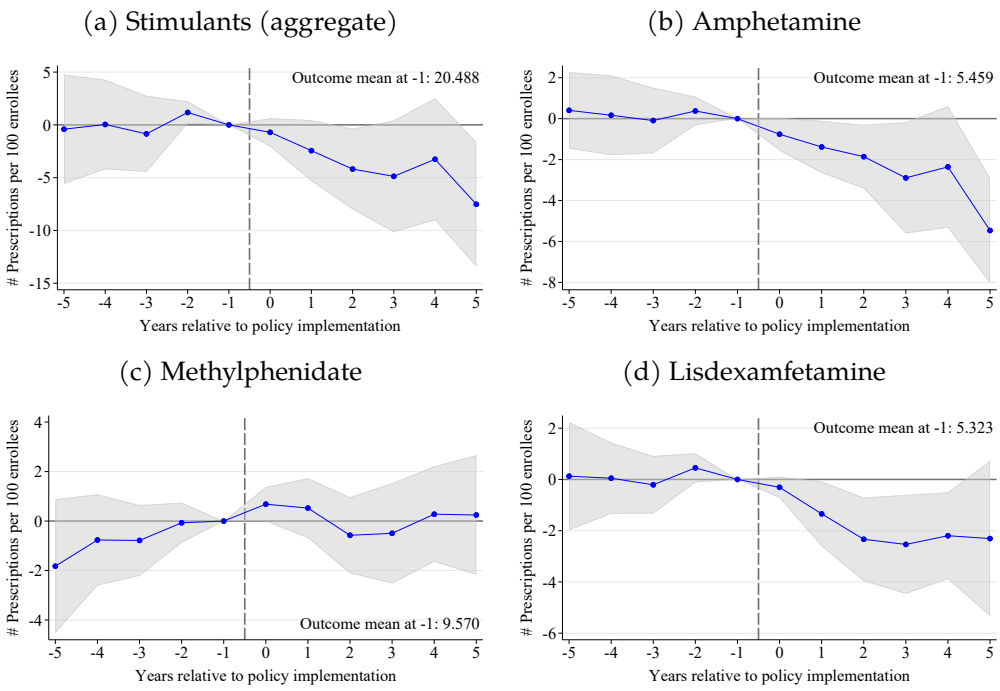


(d) Lisdexamfetamine



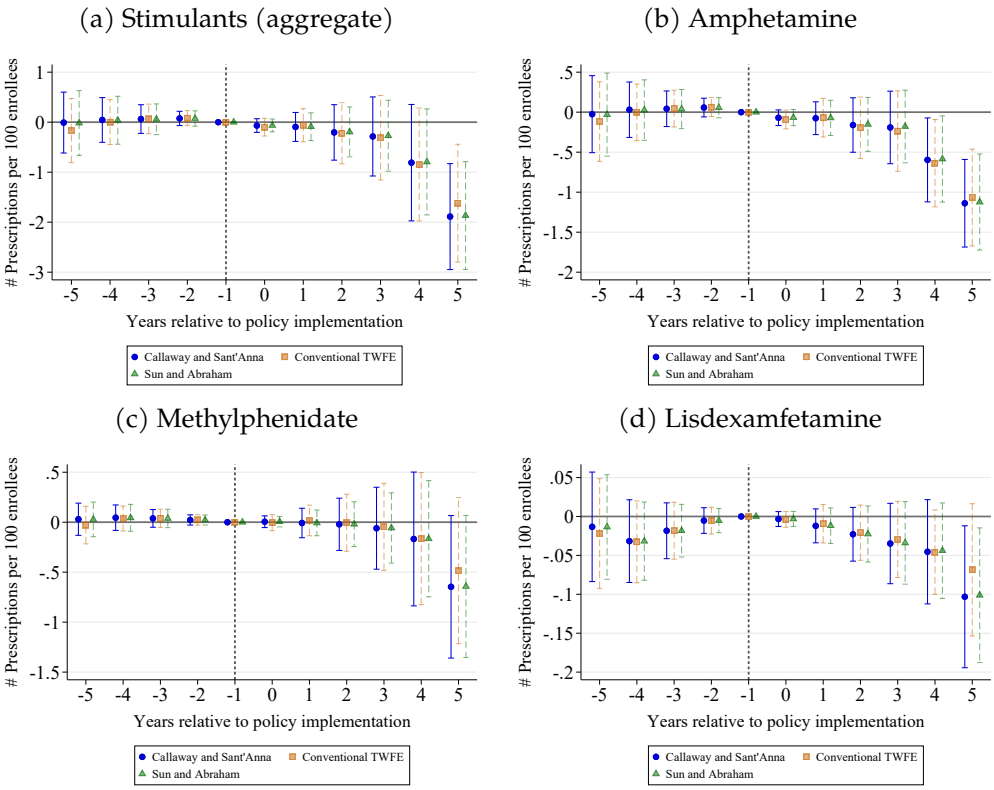
Notes: The figure reports the full decomposition for our Medicaid stimulant prescribing outcomes. It shows each of the 2X2 DD coefficients and the associated weights from the Goodman-Bacon decomposition of β from equation 1. Blue circles represent comparisons between treated and never treated, red diamonds show earlier versus later treated comparisons, and green triangles show later versus earlier treated comparisons. Weights are shown allow the x-axis and coefficient values are shown along the y-axis.

Figure A7: Effects of MA PDMPs on Stimulant Prescribing (Medicaid Data)



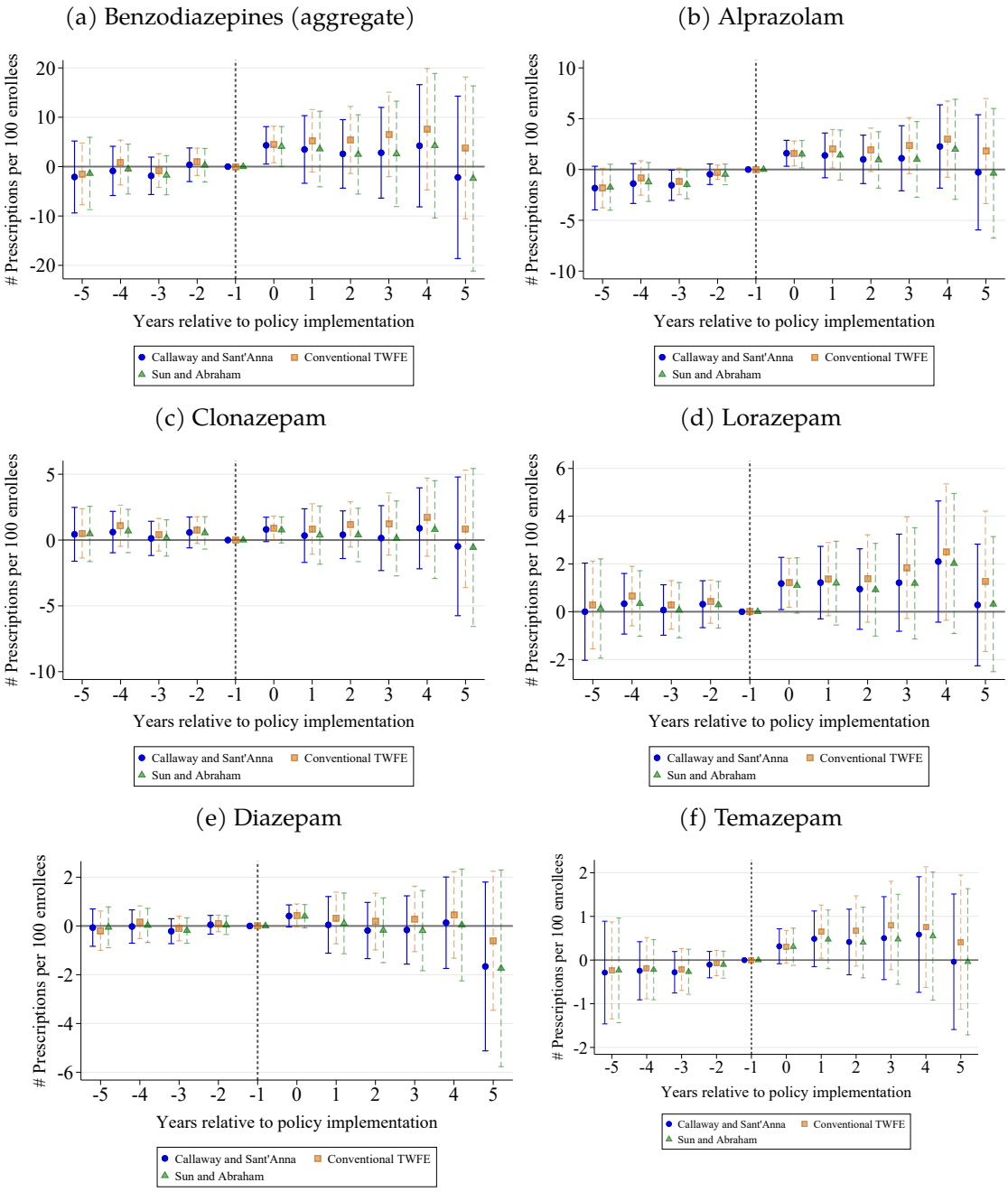
Notes: The figure presents the coefficients and 95% confidence intervals on the interactions between the indicator for treated states and the indicators for each of the years before and after policy implementation obtained following the methodology from Callaway and Sant'Anna (2021). The year before the policy implementation is the omitted category. The regressions include state and year fixed effects. Standard errors are clustered at the state level.

Figure A8: Robustness of the Stimulant Distribution Results to Alternative Estimators (ARCOS Data)



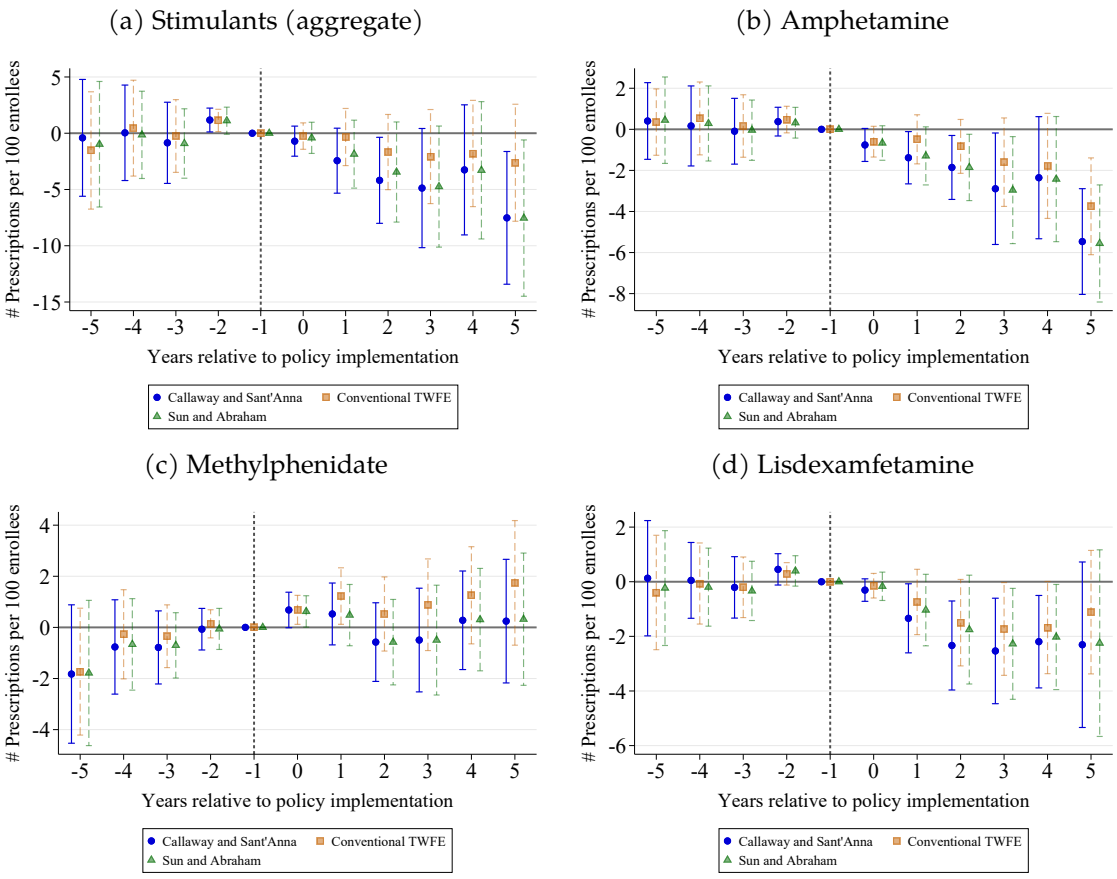
Notes: The figure presents the robustness of our ARCOS stimulant distribution results to alternative estimators. Our baseline estimates using Callaway and Sant'Anna (2021) are shown as blue circles, while estimates from Sun and Abraham (2021) and a TWFE model are shown as green triangles and orange squares, respectively.

Figure A9: Robustness of the Benzodiazepine Prescribing Results to Alternative Estimators (Medicaid Data)



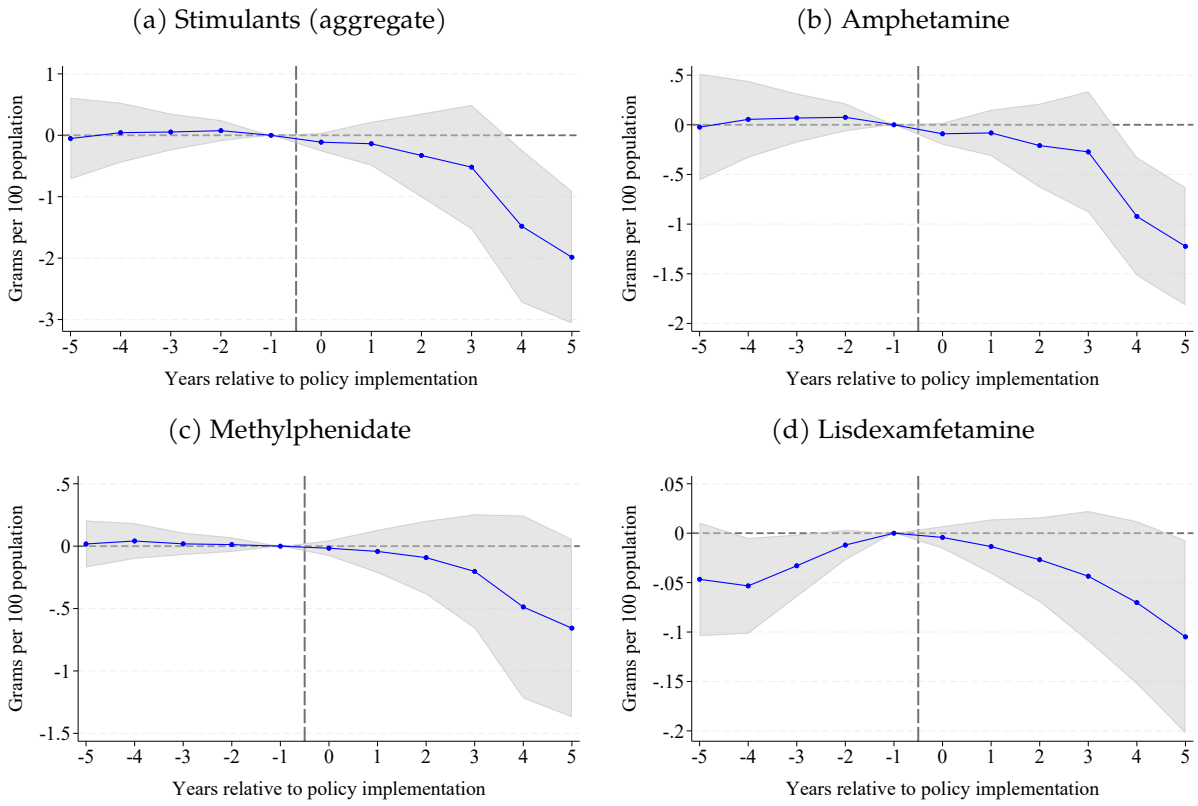
Notes: The figure presents the robustness of our Medicaid benzodiazepine prescribing results to alternative estimators. Our baseline estimates using Callaway and Sant'Anna (2021) are shown as blue circles, while estimates from Sun and Abraham (2021) and a TWFE model are shown as green triangles and orange squares, respectively.

Figure A10: Robustness of the Stimulant Prescribing Results to Alternative Estimators (Medicaid Data)



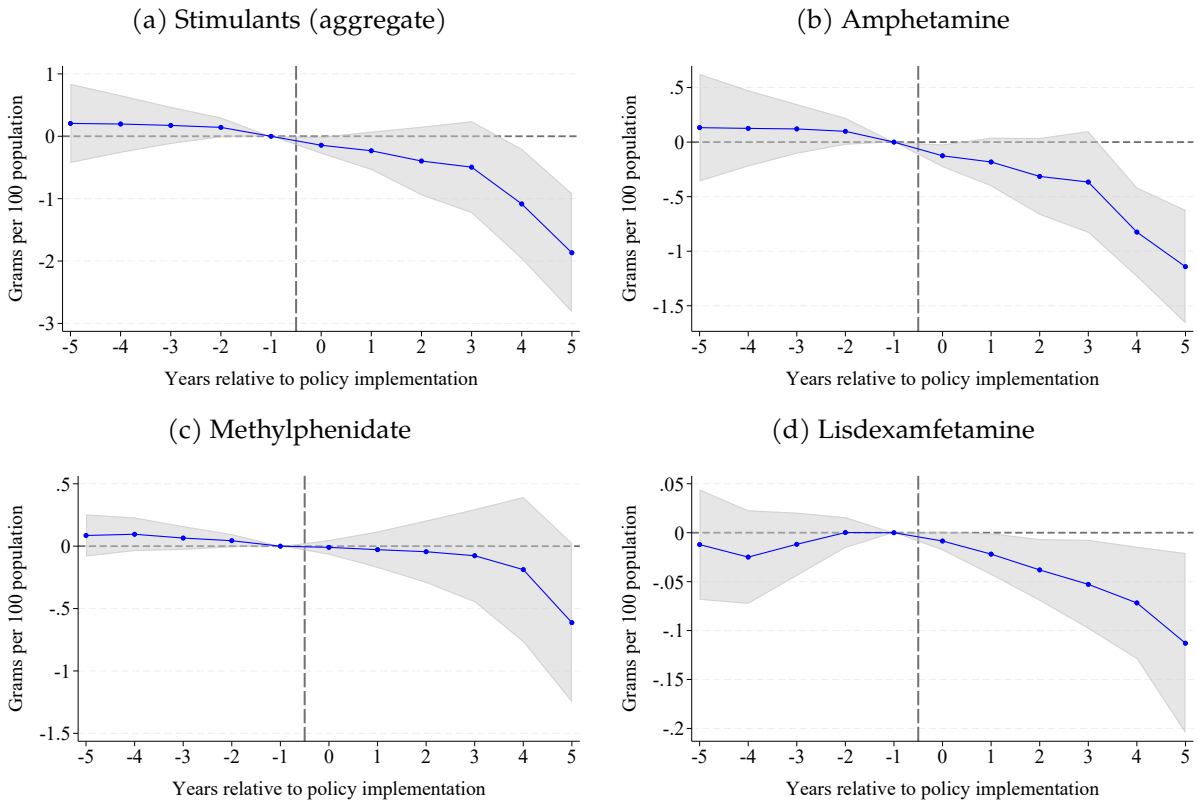
Notes: The figure presents the robustness of our Medicaid stimulant prescribing results to alternative estimators. Our baseline estimates using Callaway and Sant'Anna (2021) are shown as blue circles, while estimates from Sun and Abraham (2021) and a TWFE model are shown as green triangles and orange squares, respectively.

Figure A11: Robustness of the Stimulant Distribution Results to Dropping Triplicate States (ARCOS Data)



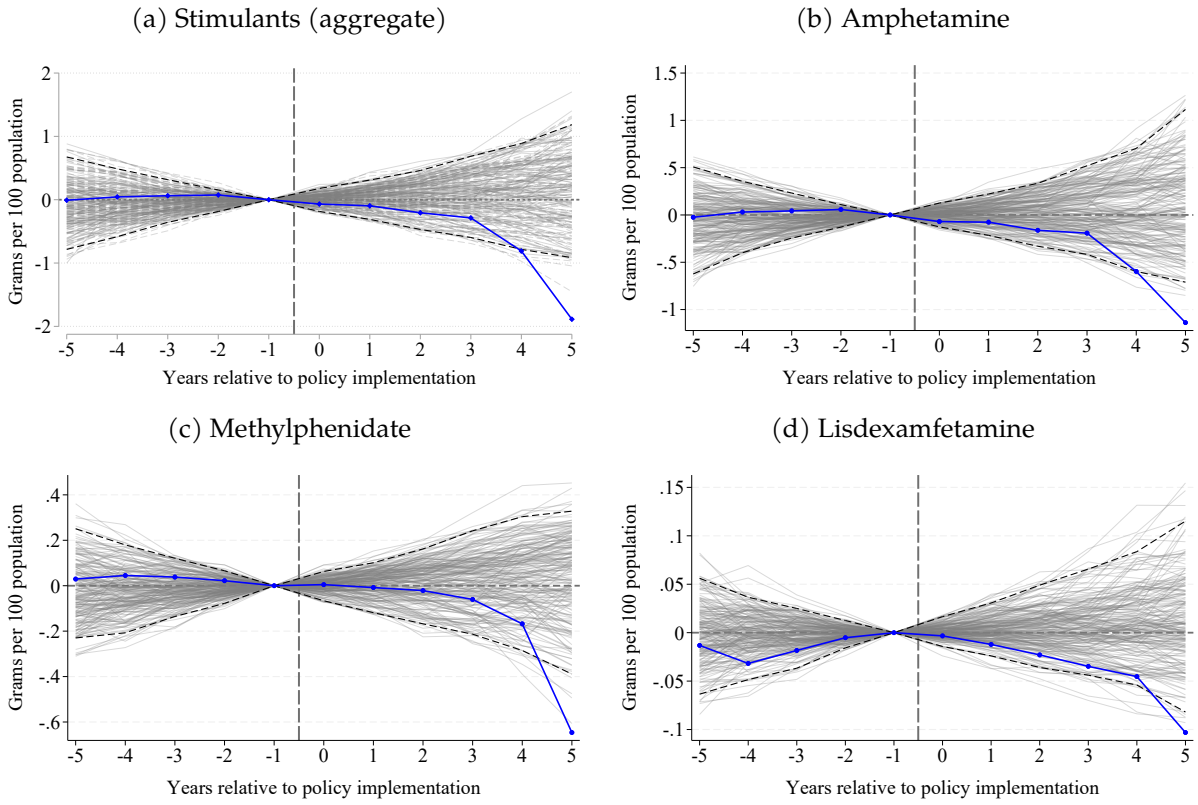
Notes: The figure shows the robustness of our stimulant distribution results to dropping all triplicate states.

Figure A12: Robustness of the Stimulant Results to the Inclusion of Census Region-by-Year Fixed Effects (ARCOS Data)



Notes: The figure shows the robustness of our stimulant results to allowing for arbitrary census region-by-year trends.

Figure A13: Randomization Inference: Effects of MA PDMPs on Stimulant Distribution (ARCOS Data)



Notes: This figure presents the results of the randomization inference exercise for our ARCOS stimulant outcomes. Each gray line represents the Callaway and Sant'Anna (2021) ATT_e estimators from a version of our data in which MA PDMPs are quasi-randomly assigned. The dashed black lines represent the empirical 95 percent confidence interval based on 200 simulations.

Table A1: Dose Equivalence for Stimulants

Drug	Milligram
Amphetamine	5
Methylphenidate	10
Lisdexamfetamine	30

Notes: This table lists dose equivalents in milligrams for stimulants, taken from Meinhofer (2018) and ADHD Medication Calculator (http://www.adhdmedcalc.com).

Table A2: List of Stimulants and Benzodiazepines

Generic Name	Brand Name
Stimulants	
Amphetamine	Adderall
Methylphenidate	Ritalin, Methylin, Metadate, Concerta, Daytrana, Aptensio
Lisdexamfetamine	Vyvanse
Benzodiazepines	
Alprazolam	Xanax, Niravam
Clonazepam	Klonopin
Diazepam	Diastat, Valium
Lorazepam	Ativan
Temazepam	Restoril

Notes: This table lists brand names for each generic type of stimulant and benzodiazepine that are used to construct Medicaid prescribing outcomes.

Table A3: Effects of MA PDMPs on Stimulant Prescribing (Medicaid Data)

	Aggregate (1)	Amphetamine (2)	Methyl. (3)	Lisdexamf. (4)
Dependent variable: Number of prescriptions per 100 enrollees				
Immediate effect	-0.70	-0.76*	0.68*	-0.31
	(0.68)	(0.41)	(0.35)	(0.21)
1-year effect	-2.44*	-1.38**	0.53	-1.34**
	(1.47)	(0.65)	(0.62)	(0.65)
2-year effect	-4.19**	-1.86**	-0.58	-2.33***
	(1.95)	(0.79)	(0.78)	(0.83)
3-year effect	-4.87*	-2.89**	-0.50	-2.54**
	(2.70)	(1.38)	(1.03)	(0.99)
4-year effect	-3.26	-2.36	0.28	-2.20**
	(2.95)	(1.52)	(0.98)	(0.86)
5-year effect	-7.52**	-5.46***	0.24	-2.31
	(3.01)	(1.31)	(1.23)	(1.55)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Time-varying covariates	N	N	N	N
Outcome mean at -1	20.488	5.459	9.570	5.323
Observations	395	409	412	396

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect obtained following the methodology from Callaway and Sant'Anna (2021). The year before the policy implementation is the omitted category. Observations are weighted by state population. The dependent variables are stimulant prescriptions per 100 Medicaid enrollees. The mean of dependent variable is calculated using observations from the treated sample measured in the last pre-policy period. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.