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, little is known about their impact on non-opioid-related outcomes. In this paper, we examine the effect of MA PDMPs on prescribing patterns of stimulants and benzodiazepines. Using a difference-in-differences event study design, we show that MA PDMPs led to decreases in stimulant prescribing. In contrast, we find suggestive evidence that benzodiazepine prescriptions increase following the implementation of a MA PDMP. 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. For example, in the 2015-16 wave of the National Survey on Drug Use and Health (NSDUH), approximately 2 percent

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[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.

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. The increasing trend in polydrug misuse is particularly salient with regards to opioids along with benzodiazepines and stimulants, which now contribute to a large fraction of overdose deaths. 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. 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.[5] Our outcomes of interest are amphetamine-equivalent stimulant grams per 100 population and benzodiazepine prescriptions per 100 Medicaid enrollees.[6]

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 16.6 percent decrease in amphetamine-equivalent stimulant grams per 100 population relative to the mean one year before treatment. Our point estimates range between a 15.9–21.5 percent decrease when we examine each type of stimulant separately.[7] We find qualitatively similar results when implementing the method of Sun and Abraham (2021), suggesting that our results are not driven by treatment effects that are heterogeneous over time (i.e., dynamic treatment effects). Likewise, we find qualitatively similar results using a synthetic controls approach, which allows for the construction of control groups for each state that more closely match the pre-treatment dynamics.

Interestingly, we find opposite signed effects when we examine benzodiazepine prescriptions. Using a two-way fixed effects regression framework, we find that the implementation of a MA PDMP leads to an immediate increase in benzodiazepine prescribing. In the year of MA PDMP

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[5]The DEA does not track benzodiazepine shipments.

[6]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).

[7]Our estimates indicate that stimulant grams per 100 population decrease by 0.91 for amphetamine (16.5% in terms of the mean one year before the treatment, $p-$value=0.006), by 0.51 for methylphenidate (15.9%, $p-$value=0.137), and by 0.10 for lisdexamfetamine (21.5%, $p-$value=0.073).

implementation, we observe a 9.75 percent increase in benzodiazepine prescriptions per 100 Medicaid enrollees relative to the mean one year before the treatment.[8] In addition, we find suggestive evidence that five years following policy implementation, MA PDMPs were associated with a 5.97 percent increase in aggregate benzodiazepine prescribing, although the effects are heterogeneous across benzodiazepine types. However, none of these long-run effects are statistically significant.[9] Overall, our results are qualitatively similar using a synthetic controls approach, but are significantly attenuated using the estimator proposed by Sun and Abraham (2021). We therefore view the benzodiazepine results as more suggestive relative to the clear effects we document with stimulants.

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, 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.[10]

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. In contrast, several

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[8]Our estimates indicate that the number of prescriptions per 100 Medicaid enrollees increase by 1.58 for alprazolam (15.5% in terms of the outcome mean one year before the treatment, $p-$value=0.011), by 0.91 for clonazepam (9.54%, $p-$value=0.047), by 1.21 for lorazepam (20.2%, $p-$value=0.02), by 0.43 for diazepam (11%, $p-$value=0.063), and by 0.24 for temazepam (19.2%, $p-$value=0.219), in the year of policy implementation.

[9]We find that the number of prescriptions per 100 Medicaid enrollees increase by 1.75 for alprazolam (17.1% in terms of the outcome mean one year before the treatment, $p-$value=0.5), 0.86 for clonazepam (9%, $p-$value=0.7), 1.31 for lorazepam (21.9%, $p-$value=0.371), and 0.37 for temazepam (29.6%, $p-$value=0.634). The number of diazepam prescriptions per 100 enrollees decreased by 0.7 (18%, $p-$value=0.623) five years after the treatment.

[10]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.'

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, 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. This is especially important for stimulants, which exhibit stronger responses as time passes. Second, while other studies only focus on aggregate measures for the prescribing of stimulants or benzodiazepines, we investigate the policy effects on the prescribing of each type of drug separately.[12] While drugs belonging to the same class have similar properties, there are important differences that could result in heterogeneous responses to policy shocks. For example, alprazolam and clonazepam are both benzodiazepines and are commonly prescribed to treat anxiety. However, alprazolam has a relatively faster onset and is associated with a better subjective high, making it the preferred drug among most recreational users (Lader, 2011). In contrast, clonazepam has a slower onset but longer-lasting effects, making it more preferred for treating opioid withdrawals (Stein et al., 2016). While we find that most of the different generic drugs within a broader category have similar responses to MA PDMPs, there is some degree of heterogeneity, especially for benzodiazepines.

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

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[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. The key difference between our work and Liang and Shi (2019) is that they focus explicitly on the states having PDMP mandates for use of benzodiazepine records, while we include a broader set of PDMP mandates in our analysis. As discussed above, there are several different channels through which any PDMP mandate (e.g., mandate for use of opioid records only) can affect benzodiazepine prescribing.

[12]We analyse amphetamine, methylphenidate, and lisdexamfetamine separately in our stimulant analysis. Likewise, we analyze alprazolam, clonazepam, lorazepam, diazepam, and temazepam separately in our benzodiazepine analysis.

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, our results suggest increases in benzodiazepine prescriptions in response to MA PDMPs. 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. In section 3, we describe our identification strategy. We describe our data 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

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[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).

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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 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

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[16]See Table 1 and Appendix Figure A3.

[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.

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. 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   Research Design

Our empirical strategy for estimating the causal impact of MA PDMPs exploits variation in the timing of adoption across states. Specifically, we estimate event study difference-in-differences regressions of the form:

$$Y_{st} = \alpha_s + \beta_t + \sum_{k \neq -1} \gamma_k \mathbf{1}(MA\ PDMP_{sk}) + X_{st}\delta + \varepsilon_{st}, \tag{1}$$

where Y_{st} is the outcome variable measured at the state-by-year level, and α_s and β_t are state and year fixed effects, respectively. The indicator variable $\mathbf{1}(MA\ PDMP_{sk})$ is set equal to 1 if state s enacted a MA PDMP k years ago. The coefficients of interest, γ_k, indicate the difference in outcome between treatment and control states in period k, relative to the last pre-policy period, conditional on the other control variables. We trim all post-periods after the fifth ($k > 5$) and all pre-periods more than nine years prior ($k < -9$). X_{st} is a vector of time-varying covariates. In our regressions for stimulant distribution outcomes, we control for race/ethnicity composition (the share of the population that is non-Hispanic white, non-Hispanic Black, Hispanic, other) and age composition (the share of ages under 15, 15–24, 25–44, 45–64, 65–84, over 85).[19] Observations are weighted by state population (Medicaid enrollment) in our regressions 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.[20] Since states implemented the policy with different timing, our sample of states and years is unbalanced

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[19]We do not include any time-varying covariates in our regressions for Medicaid prescribing outcomes, but our results are nearly identical when we control for them.

[20]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" units. 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, to be consistent with our synthetic control analysis in Section 3.1. Our results are robust to including this state. Our final analysis sample consists of 24 treatment states and 22 control states.

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 conditional on the covariates. We assess the plausibility of this assumption by plotting the γ_k coefficients which allows us to examine whether treatment and control states were trending in parallel prior to treatment. As we will show in section 5, this assumption appears reasonable for many of our outcome variables. However, for certain outcomes we find evidence of pre-existing trends, casting doubt on this assumption. Therefore, we supplement our event study approach with a synthetic controls analysis.

3.1 Synthetic Control Analysis

We complement our event study regressions with a synthetic control analysis. The idea is to construct a comparable synthetic control state for each treated state based on pre-period data in such a way that the synthetic control state has similar trends in outcomes to the treated state prior to policy implementation. If the results from our synthetic control analysis are similar to the baseline results, it will imply that pre-treatment differences between the treated and control groups are not likely to be responsible for our results.

While a synthetic control approach has been more widely conducted for a single treated unit or multiple units with the same treatment timing, this method has recently been adopted for the case of multiple units with differential treatment timing (e.g., Kleven, 2019; Acemoglu et al., 2016). To conduct a synthetic control analysis, we first construct a synthetic control state for each treated state and then create a sample so that both the treated and synthetic control samples are strongly balanced in relative periods.

For each of these treated states, we construct a synthetic control state from the 22 control states included in the baseline sample, matching on an outcome variable measured in each of the pre-treatment periods.[21] Note that the number of pre-treatment periods differs across states, and we use all available pre-treatment periods to construct a synthetic control. Each synthetic control state is composed of a weighted average of observations from the subset of the 22 control states.[22,23]

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[21]We use the Stata command `synth` to construct a synthetic control. For more details, see: https://fmwww.bc.edu/RePEc/bocode/s/synth.html.

[22]Tables reporting the makeup of the synthetic state for each treated state and for each outcome are available upon request.

[23]As we describe in Section 4, we only include in our sample the generic type-state-year observations that consistently report in all four quarters (around 96.5% of all generic type-state-year observations). Since we construct a synthetic control unit by matching on an outcome variable measured in each of the pre-treatment periods, requiring consistent observations over the pre-periods, we use the linear interpolation and/or extrapolation methods to impute the dropped values.

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Using observations from the treated and synthetic control groups, we create a sample so that each treated and the matched synthetic control sample are strongly balanced from relative period $-x_{1s}$ to $+x_{2s}$, where $-x_{1s}$ $(+x_{2s})$ is the earliest (latest) relative period available for the state.[24] For each treated state and each relative period, we calculate the difference in the outcome variable between the treated state and matched synthetic control. Finally, for each relative period, we take the average difference in the outcome between the treated states and synthetic controls, weighting by state population (or Medicaid enrollment for Medicaid prescribing outcomes) measured in 2008 (i.e., the baseline period). In the results section, we show how the average difference in the outcome between the treated and synthetic control groups change around the time of policy implementation.

4 Data

4.1 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.[25]

We obtain information about the weight in grams of amphetamine, methylphenidate, and lisdexamfetamine distributed to each state for each quarter from 2008-2018.[26] 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 A1. From 2008 to 2017, the per capita supply of amphetamine and lisdexamfetamine more than doubled. In contrast,

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[24]For example, if a treated state implemented the policy in 2011, $-x_{1s}$ is equal to -3 and $+x_{2s}$ is equal to $+6$, since our sample period is 2008–2017.

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

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

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-year level data.[27,28]

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.[29] We also create an aggregate measure of benzodiazepine prescribing by adding together the number of each type of benzodiazepine prescription.[30] Data on Medicaid enrollment are obtained from the Kaiser Family Foundation.[31] We show time series figures of the rates of stimulant and benzodiazepine prescriptions in panels (a) and (b) of Figure A2, respectively.

We present summary statistics on each of our primary outcome variables in Table 2. The

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[27]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.

[28]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.

[29]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.

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

[31]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.

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 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).[32]

We display these rates in Figure 1, along with rates of opioid (mis)use for comparison.[33] 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.

4.2   PDMPs

Table 1 shows the effective dates of the laws used in this paper, taken from Sacks et al. (2021). Appendix Figure A3 presents the trends in the total number of states with MA PDMPs. By the end

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[32]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.

[33]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.

of 2017, 26 states had passed MA PDMP laws.

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.[34] Given the consistent finding of this prior work, we do not discuss our replication of this finding here.[35] 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 regression coefficients from equation 1 for each of these outcomes, as well as our measure aggregating across these three types, in Figure 2.[36] 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 -1.52. Relative to the mean of 9.179 one year before treatment, this is a decrease of 16.6 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 -0.91, a decrease of 16.5 percent. In panel (c), we turn our attention to methylphenidate. Five years after the treatment begins, the point estimate of -0.51 indicates a reduction of 15.9 percent, very similar to what we observed for amphetamine. Finally, we consider lisdexamfetamine in panel (d). The pattern is again nearly identical to what we observed in panels (b) and (c), and

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[34]See Maclean et al. (2020) for a review.

[35]These results are available upon request, and fall within the range of estimates in the existing literature.

[36]The coefficients are listed in table form in Table 3.

indicates a reduction of about 21.5 percent five years after the implementation of a MA PDMP.[37]

To probe the sensitivity of these results, we also employ a synthetic controls approach. As discussed in Section 3.1, we construct a synthetic version of each state consisting of a convex combination of other states which never adopted MA PDMPs over our sample period. The exact convex combination is chosen to mimic the treated state's outcome dynamics prior to treatment. We then compute the difference between each treated state's actual and synthetic counterpart and present the average difference in each period in Figure A4.[38] Similarly to Figure 2, we present the results for our aggregate measure in panel (a), followed by amphetamine, methylphenidate, and lisdexamfetamine separately in panels (b)-(d), respectively. In panels (b)-(d), the pre-period coefficients are (by construction) close to zero. In the post-period, we observe very similar dynamics to those from our primary regressions. Furthermore, the point estimates are quite similar using the two different methodologies, though the magnitudes of point estimates for the last post-period are larger in our synthetic control analysis than the corresponding regression estimates. For example, in panel (b) we estimate that five years after the adoption of a MA PDMP, adopting states amphetamine prescribing (measured as grams distributed per 100 population) is about -1.46, compared to -0.91 from the event study. Similarly in panels (c) and (d), the point estimates are larger than the regression estimates. Qualitatively, however, the synthetic control analysis confirms what we observe using the regression approach.[39]

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 Figure A5.[40] Beginning with panel (b), we observe a point estimate of -3.75 five years after adoption, relative to a mean of 5.49, a 68 percent decrease. This is somewhat larger than what we observe when using our main measure of

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[37]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.

[38]Coefficients are listed in table form in Table A3. Results for each individual state are available upon request.

[39]In panel (a), the coefficients are shifted up relative to the remaining panels. This is driven by poor synthetic matches for a few states. However, the dynamics look quite similar.

[40]We report the coefficients in table form in Table A4.

prescribing. Examining panel (c), however, we find results that are qualitatively different. Our last post-period coefficient is a positive 1.92, suggesting an increase in methylphenidate prescriptions. However, this five-year effect is statistically insignificant at the 5 percent significance level. In addition, examining the pattern of coefficients in the pre-period suggests that this may simply be the continuation of trends that existed prior to treatment, rather than an effect of MA PDMPs. For these reasons, we are hesitant to draw strong conclusions about this outcome variable. In panel (d), we see results that are qualitatively similar to those using the ARCOS measure. Our last coefficient is -1.05, indicating a 19 percent decrease relative to the mean of 5.4. This is nearly identical to what we observe in the ARCOS data. Since methylphenidate has different signed effects than amphetamine and lisdexamfetamine, pooling them together in panel (a) results in somewhat muted results. This highlights the importance of reporting each drug separately.

Lastly, we also consider synthetic control estimates using our prescribing measure from Medicaid. The results from this exercise are shown in Figure A6.[41] Qualitatively, these results mimic the regression results from Figure A5. For amphetamine and lisdexamfetamine (panels (b) and (d)), we find a decrease of about three prescriptions per 100 enrollees five years after policy implementation for each drug. This is similar in magnitude to what we observe in Figure A5 for amphetamine, although larger in magnitude for lisdexamfetamine. In panel (c), we again observe an increase in methylphenidate. Combining these drugs in panel (a), we see an overall reduction in stimulant prescribing of about 20.6 percent five years after MA PDMP implementation.

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,

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[41]We include the coefficients in table form in Table A5.

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 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 event study coefficients from equation 1 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.[42] In panel (a) we observe relatively flat pre-trends, followed by positive coefficients in the post-period. Panel (b) also shows positive coefficients in the post-period, although there is some evidence of pre-existing trends. The remainder of the panels, however, reveal relatively flat pre-trends, consistent with our identifying assumption. In all columns in Table 4, we observe a sudden increase in prescriptions in the first post-treatment period, and the coefficients are statistically significant in almost all columns. The effect sizes in the year of treatment range from a 9.5 percent increase (clonazepam) to a 20.2 percent increase (lorazepam). The effect sizes are stable over time, though the coefficients in the later post-periods are statistically indistinguishable from zero. The effect sizes five years after treatment range from a 17.9 percent decrease (diazepam) to a 29.6 percent increase (temazepam) in prescriptions per 100 enrollees. Overall, these sub-figures show that there was a clear increase in prescriptions following the implementation of a MA PDMP, contrary to what we observed for stimulants and what has commonly been found for opioids.

Next, we present the results from our synthetic controls approach in Figure A7.[43] This is important in light of the apparent pre-trends in panel (b) of the previous figure. Here, panel (a) shows that we are generally able to find synthetic controls which closely match the pre-period dynamics for each state. This panel reveals an increase of 5.61 total benzodiazepine prescriptions per 100 enrollees. Relative to a pre-period mean of 33.91, this indicates an increase of 16.5 percent. When we consider each type of benzodiazepine separately, the estimates are qualitatively similar and generally fall within the confidence intervals of the regression estimates. The only exceptions are lorazepam and temazepam, which show somewhat muted effects relative to the regression results.

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[42]Coefficients are shown in table form in Table 4.

[43]Estimates shown in table form in Table A6.

5.1 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). Likewise, Sun and Abraham (2021) show that event study difference-in-differences can suffer from a similar problem, in which treatment effect heterogeneity can induce apparent pre-trends. In this section, we examine the robustness of our results to their proposed estimator.[44]

We present the results for stimulants in Appendix Figure A8. The event studies shown in this figure are nearly identical to the standard event studies shown in Figure 2. This suggests that our findings of decreased stimulant prescribing are not an artifact of treatment effect heterogeneity. The consistent findings across this and the synthetic controls approach all 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 Figure A9. Interestingly, the effects are more muted using this specification relative to Figure 3. The post-period point estimates are generally positive, but the magnitudes are notably smaller. In conjunction with the synthetic controls results, our overall takeaway is that there is some evidence of an increase in benzodiazepine prescribing following an MA PDMP, although the results are somewhat sensitive to alternate specifications.[45]

Additional Controls In Appendix Figures A11–A13, we test the robustness of our results to adding controls for other co-occurring opioid-related policies. The solid red line indicates the baseline estimates and their 95 percent confidence intervals, and the dashed blue line presents the point estimates and associated 95 percent confidence intervals obtained by adding the following controls to the baseline model (equation 1): (i) an indicator for whether the state has a naloxone access law (NAL), (ii) an indicator for having a Good Samaritan overdose prevention

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[44]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.

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

law, and (iii) an indicator for a pain management clinic law.[46] We obtain nearly identical results when we add these controls, suggesting that our results are not driven by other opioid-related policies implemented around the time of MA PDMPs.

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).[47] Since MA PDMPs do not directly affect illicit methamphetamine production, this biases us against detecting any mortality changes.[48]

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

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

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[46]The dates of these laws are taken from Prescription Drug Abuse Policy System (PDAPS). For more details, see: https://pdaps.org/datasets/laws-regulating-administration-of-naloxone-1501695139, https://pdaps.org/datasets/good-samaritan-overdose-laws-1501695153, and https://pdaps.org/datasets/pain-management-clinic-laws, last accessed March 7, 2022.

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

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

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 some evidence of increased consumption of benzos, although these findings are more sensitive to different empirical specifications.

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 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
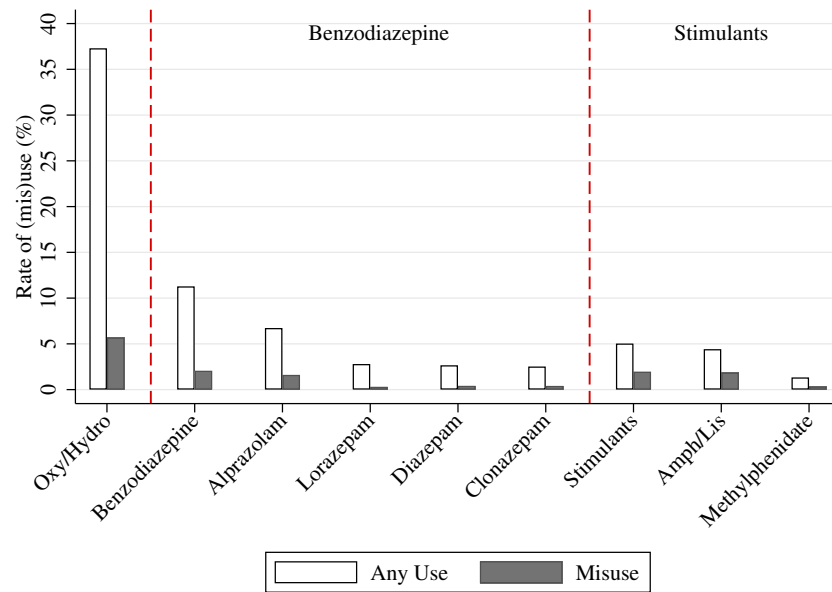
Notes: The figure presents the prevalence of use and misuse of prescription benzodiazepines and stimulants among 2015 and 2016 NSDUH respondents over age 12 (N=114,043).
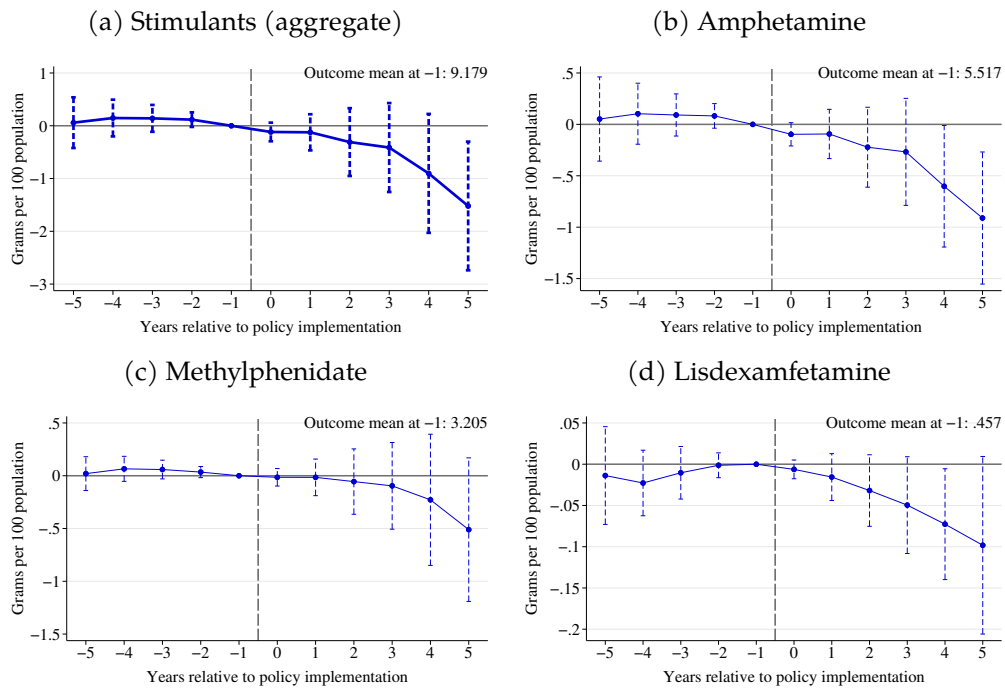
Figure 2: Effects of MA PDMPs on Stimulant Distribution (ARCOS Data)

(a) Stimulants (aggregate)

Outcome mean at −1: 9.179

(b) Amphetamine

Outcome mean at −1: 5.517

(c) Methylphenidate

Outcome mean at −1: 3.205

(d) Lisdexamfetamine

Outcome mean at −1: .457

Notes: These figures present 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 from estimation of equation (1). The year before the policy implementation is the omitted category. The regressions include state and year fixed effects. Although each regression includes a full set of indicators for event time periods -9 through 5, we only present estimates for event time periods -5 through 5 for brevity. 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)
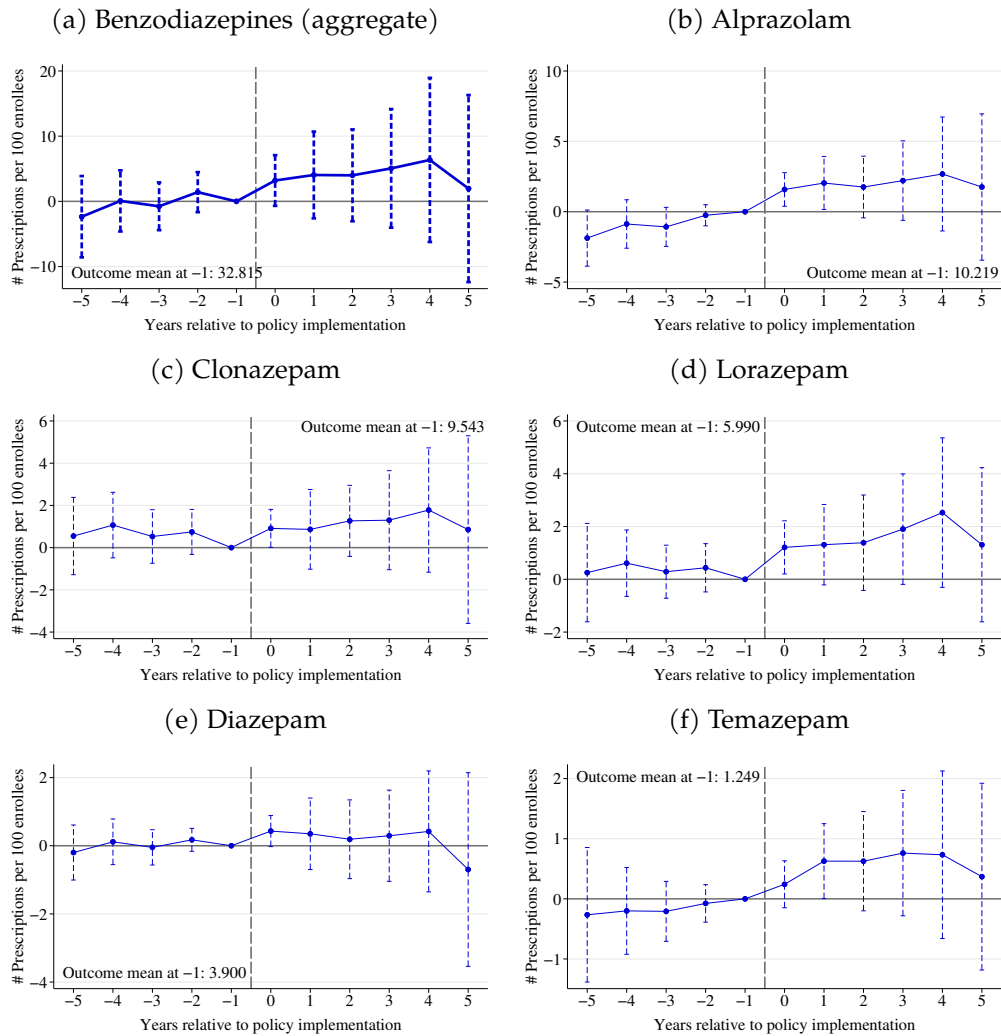
Notes: These figures present the coefficients and 95% confidence intervals on the interactions between the indicator for treated states and the indicators for each of the years before and after policy implementation obtained from estimation of equation (1). The year before the policy implementation is the omitted category. The regressions include state and year fixed effects. Although each regression includes a full set of indicators for event time periods -9 through 5, we only present estimates for event time periods -5 through 5 for brevity. 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

Outcome (mean, 2008–2017)	All States		Treated States		Control States	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (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

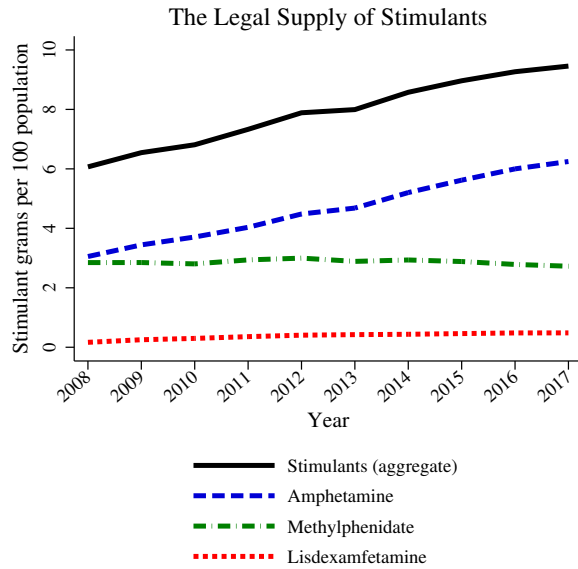|	Aggregate (1)	Amphetamine (2)	Methyl. (3)	Lisdexamf. (4)
Dependent variable: amphetamine equivalent stimulant grams per 100 population				
Immediate effect	-0.12	-0.10*	-0.01	-0.01
	(0.09)	(0.06)	(0.04)	(0.01)
1-year effect	-0.12	-0.09	-0.02	-0.02
	(0.17)	(0.12)	(0.09)	(0.01)
2-year effect	-0.31	-0.22	-0.06	-0.03
	(0.32)	(0.19)	(0.15)	(0.02)
3-year effect	-0.41	-0.27	-0.10	-0.05*
	(0.42)	(0.26)	(0.20)	(0.03)
4-year effect	-0.90	-0.60**	-0.23	-0.07**
	(0.56)	(0.29)	(0.31)	(0.03)
5-year effect	-1.52**	-0.91***	-0.51	-0.10*
	(0.61)	(0.32)	(0.34)	(0.05)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Time-varying covariates	Y	Y	Y	Y
Mean at -1	9.179	5.517	3.205	0.457
Observations	459	459	459	459
R^2	0.969	0.963	0.961	0.952

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from equation (1). Although each regression includes a full set of indicators for pre- and post-periods, we only report the coefficients on the post-periods 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. We include state and year fixed effects as well as time-varying covariates (age and race compositions) in each regression. 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

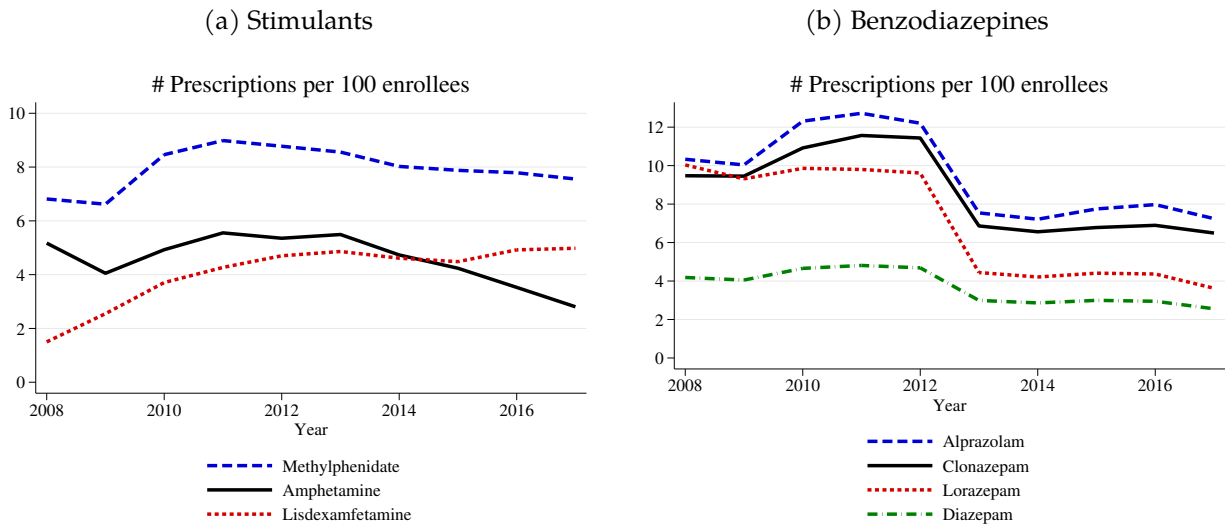|	Aggregate (1)	Alprazolam (2)	Clonazepam (3)	Lorazepam (4)	Diazepam (5)	Temazepam (6)
Dependent variable: Number of benzodiazepine prescriptions per 100 enrollees						
Immediate effect	3.20	1.58**	0.91**	1.21**	0.43*	0.24
	(1.93)	(0.59)	(0.45)	(0.50)	(0.23)	(0.19)
1-year effect	4.05	2.04**	0.86	1.31*	0.35	0.63**
	(3.30)	(0.93)	(0.94)	(0.76)	(0.52)	(0.31)
2-year effect	3.99	1.75	1.27	1.39	0.19	0.63
	(3.50)	(1.09)	(0.84)	(0.90)	(0.57)	(0.41)
3-year effect	5.06	2.21	1.30	1.90*	0.29	0.76
	(4.51)	(1.40)	(1.16)	(1.04)	(0.66)	(0.52)
4-year effect	6.35	2.68	1.78	2.53*	0.42	0.73
	(6.24)	(2.01)	(1.46)	(1.41)	(0.88)	(0.69)
5-year effect	1.96	1.75	0.86	1.31	-0.70	0.37
	(7.12)	(2.58)	(2.21)	(1.45)	(1.41)	(0.77)
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Time-varying covariates						
Mean at -1	32.815	10.219	9.543	5.990	3.900	1.249
Observations	431	443	450	449	448	433
R^2	0.848	0.833	0.838	0.868	0.826	0.815

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from equation (1). Although each regression includes a full set of indicators for pre- and post-periods, we report the coefficients on the post-periods above for brevity. 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.

Figure A1: Raw Trends in Stimulant Distribution

The Legal Supply of Stimulants

Stimulant grams per 100 population

Legend:
- Stimulants (aggregate)
- Amphetamine
- Methylphenidate
- Lisdexamfetamine

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 A2: Medicaid RX Time Series

(a) Stimulants

(b) Benzodiazepines

Prescriptions per 100 enrollees

Legend (a):
- Methylphenidate
- Amphetamine
- Lisdexamfetamine

Legend (b):
- Alprazolam
- Clonazepam
- Lorazepam
- Diazepam

Notes: These figures plot the raw trends in the number of prescriptions per 100 Medicaid enrollees.

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Figure A3: Trends in the Number of States with MA PDMPs

Our sample period: 2008–2017
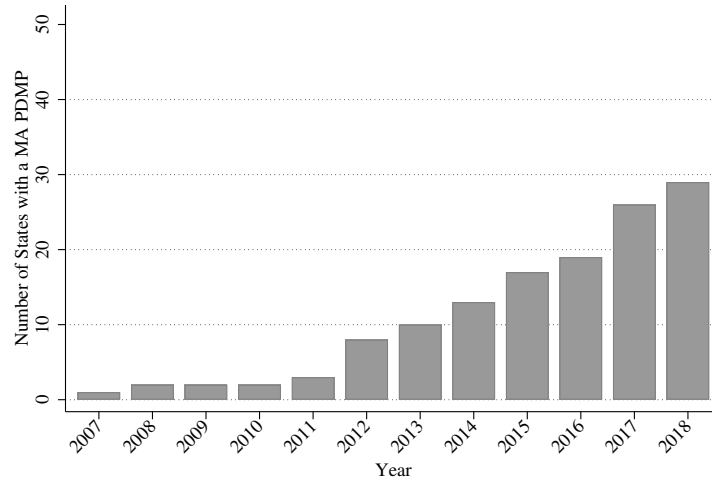
Notes: This figure shows how the number of states with a MA PDMP changes over time. The number of treated states are calculated using the effective dates of MA PDMPs reported in Table 1.

Figure A4: Synthetic Control Analysis, Effects of MA PDMPs on Stimulant Distribution (ARCOS Data)
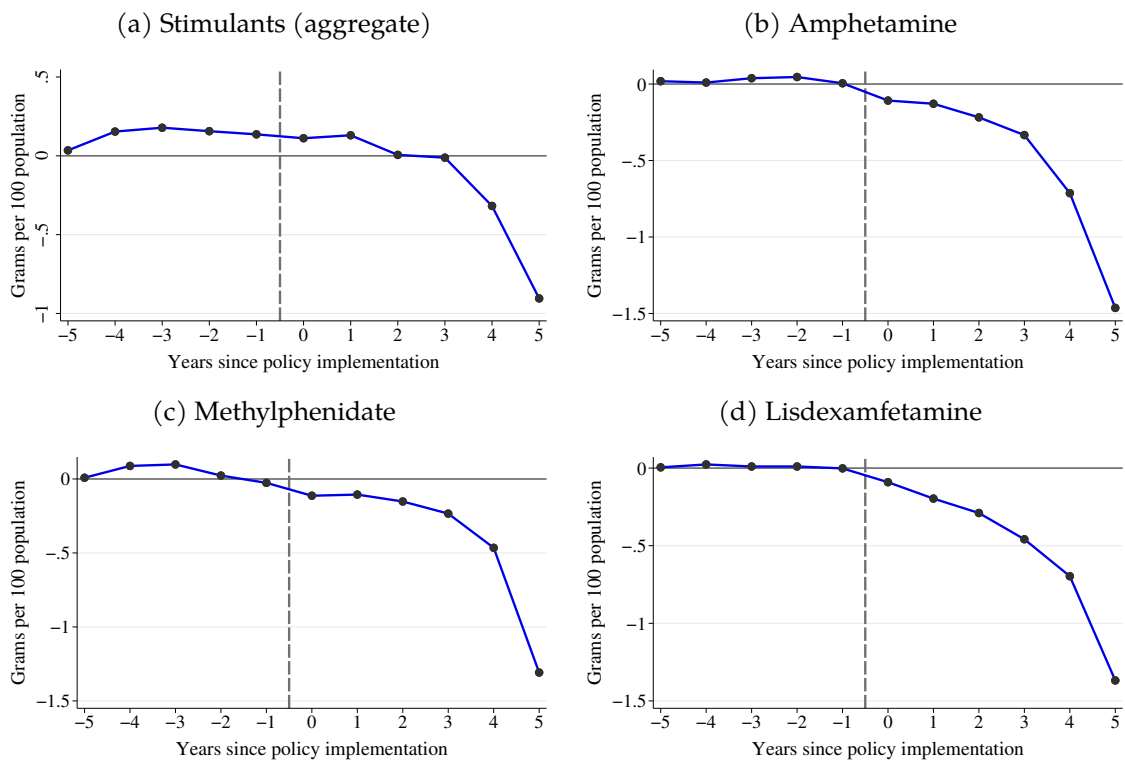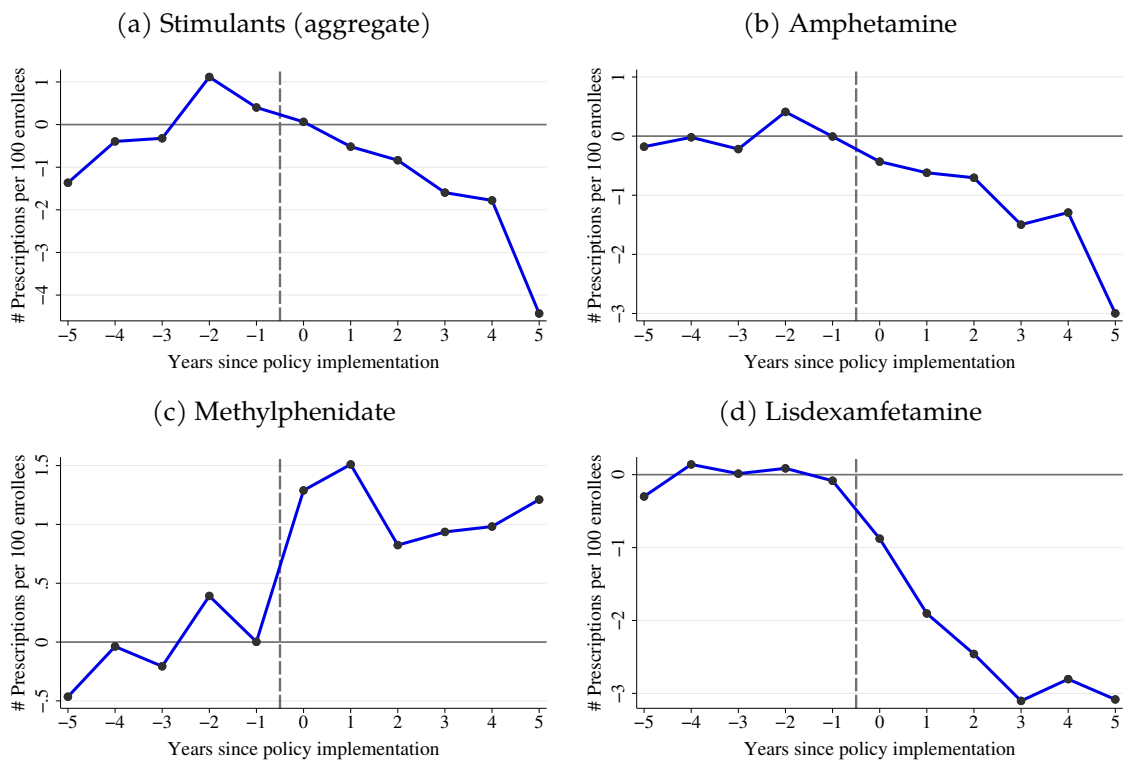

(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures show how the (weighted) average difference in the outcome between the treated and synthetic control groups changes over time.

Figure A5: Effects of MA PDMPs on Stimulant Prescribing (Medicaid Data)
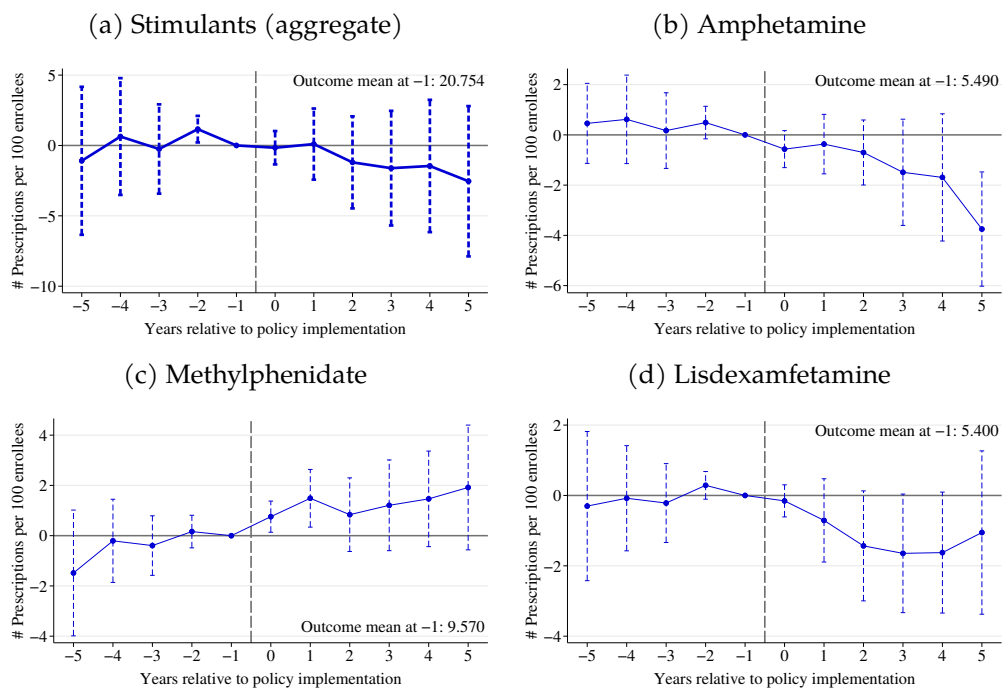
(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures present 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 from estimation of equation (1). 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 A6: Synthetic Control Analysis, Effects of MA PDMPs on Stimulant Prescribing (Medicaid Data)

(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures show how the (weighted) average difference in the outcome between the treated and synthetic control groups changes over time.

Figure A7: Synthetic Control Analysis, Effects of MA PDMPs on Benzodiazepine Prescribing (Medicaid Data)

(a) Benzodiazepine (aggregate)

(b) Alprazolam

(c) Clonazepam

(d) Lorazepam

(e) Diazepam

(f) Temazepam

Notes: These figures show how the (weighted) average difference in the outcome between the treated and synthetic control groups changes over time.

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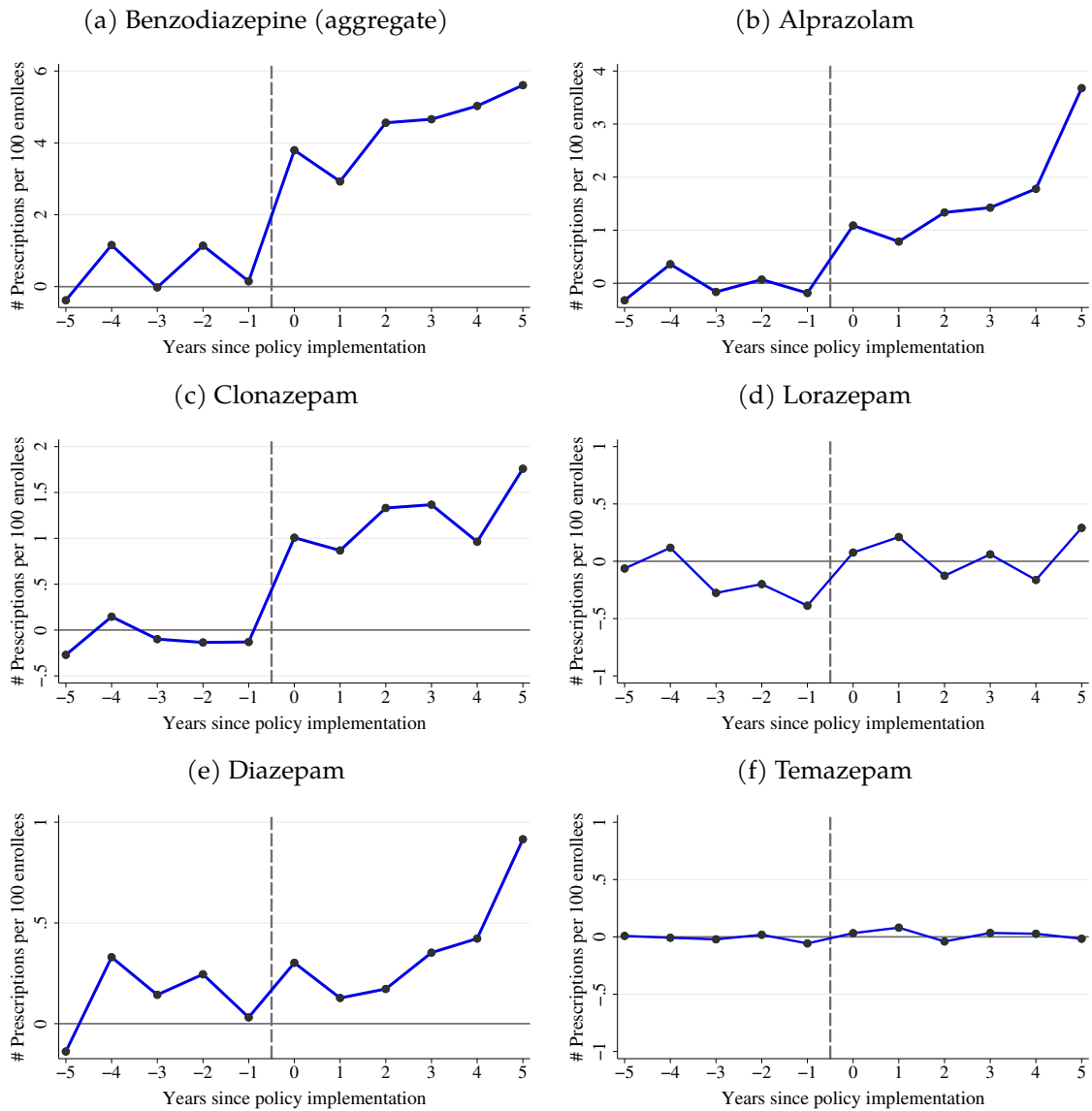
Figure A8: Effects on Stimulant Distribution (ARCOS Data): Sun and Abraham (2021) Estimates
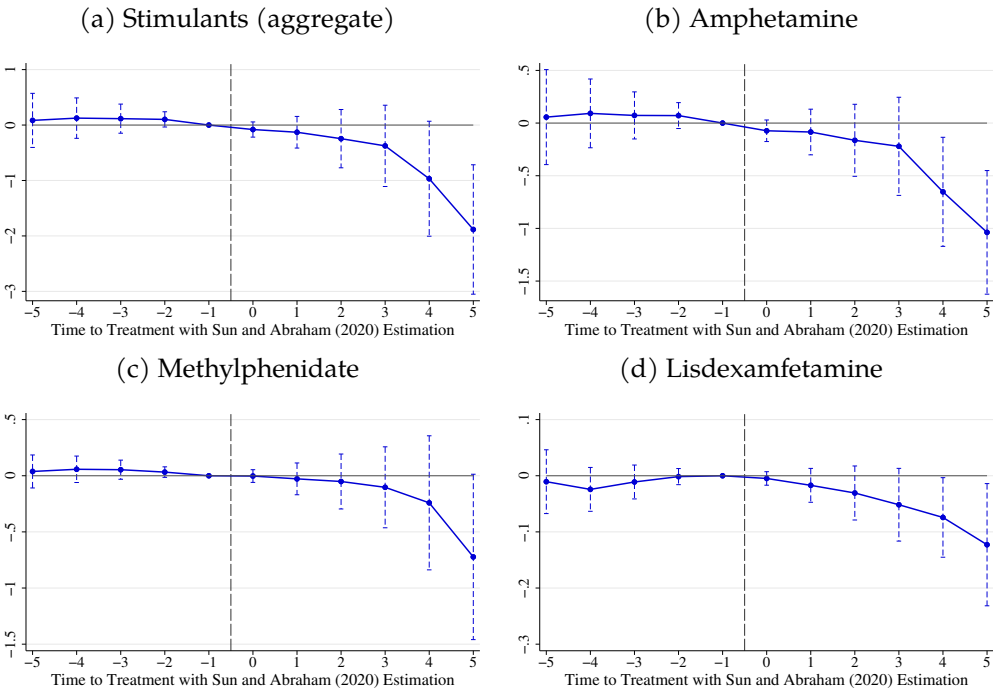
(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures present the coefficients from Sun and Abraham (2021) estimates for the stimulant distribution outcomes.

Figure A9: Effects on Benzodiazepine Prescribing: Sun and Abraham (2021) Estimates

(a) Benzodiazepine (aggregate)

(b) Alprazolam

(c) Clonazepam

(d) Lorazepam

(e) Diazepam

(f) Temazepam
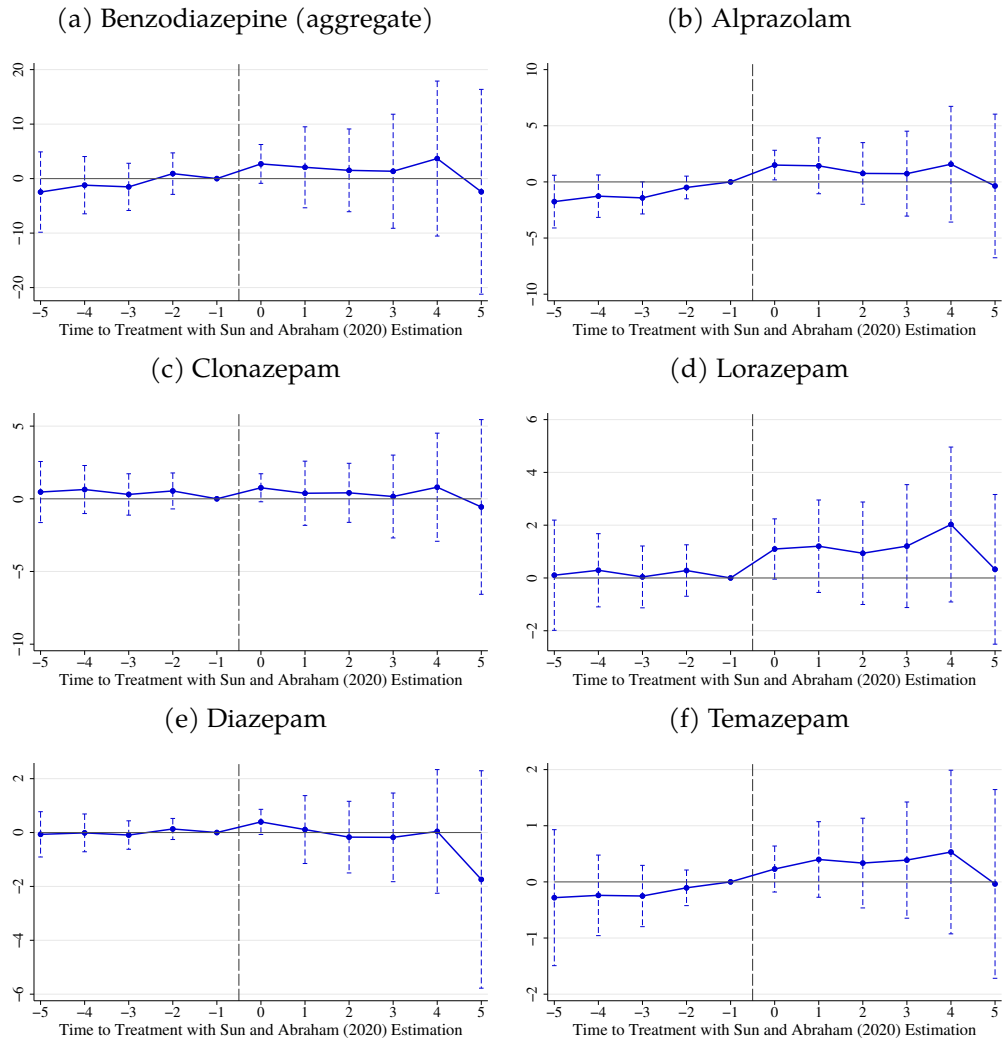
Notes: These figures present the coefficients from Sun and Abraham (2021) estimates for the benzodiazepine prescribing outcomes.

Figure A10: Effects on Stimulant Prescribing (Medicaid Data): Sun and Abraham (2021) Estimates

(a) Stimulants (aggregate)

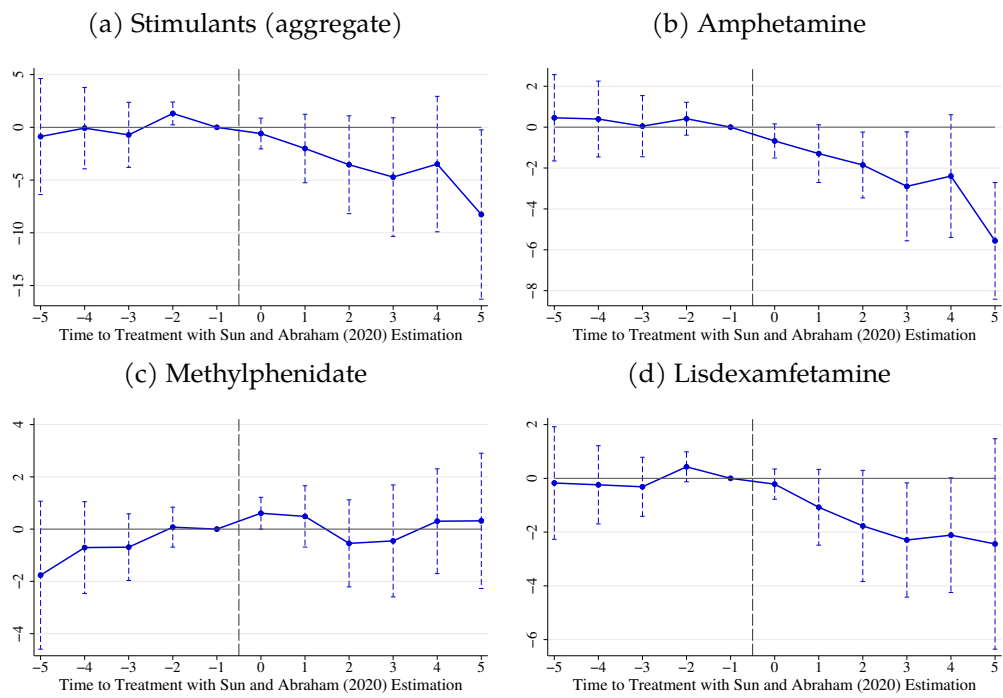(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

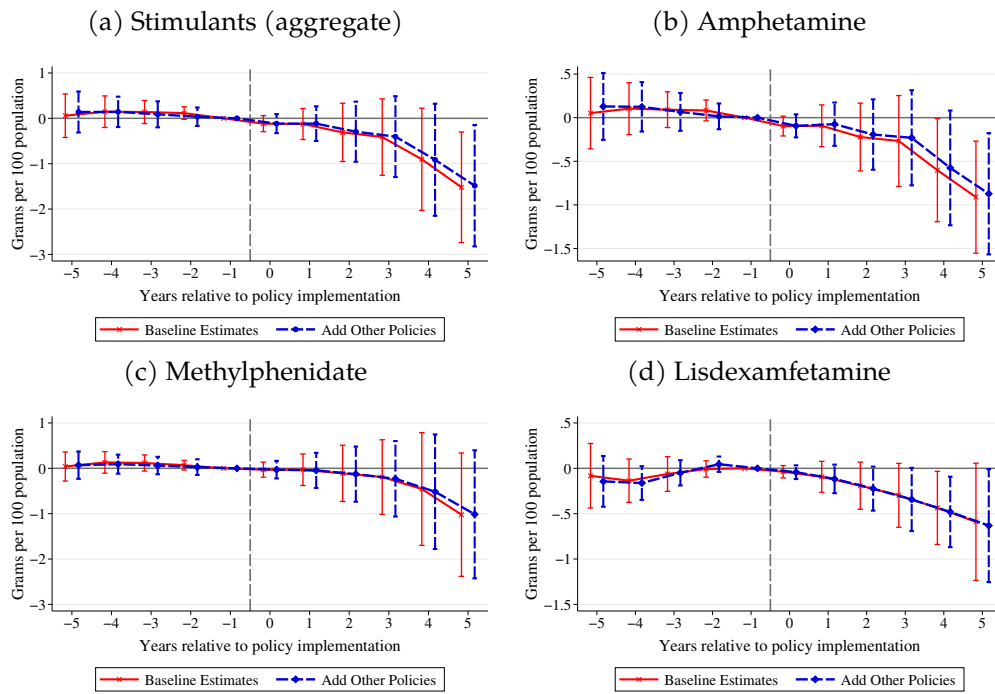*Notes*: These figures present the coefficients from Sun and Abraham (2021) estimates for the stimulant prescribing outcomes.

Figure A11: Robustness of the Stimulant Distribution Results (ARCOS Data)

(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures show the robustness of our results to adding co-occurring opioid-related policies to the baseline model, as described in Section 5.1. The dependent variables are amphetamine-equivalent stimulant grams per 100 population. The solid red line indicates the baseline estimates and their 95 percent confidence intervals, and the dashed blue line presents the point estimates and associated 95 percent confidence intervals obtained by adding to the baseline model (i) an indicator for whether the state has a naloxone access law (NAL), (ii) an indicator for having a Good Samaritan overdose prevention law, and (iii) an indicator for a pain management clinic law.

Figure A12: Robustness of the Benzodiazepine Prescribing Results
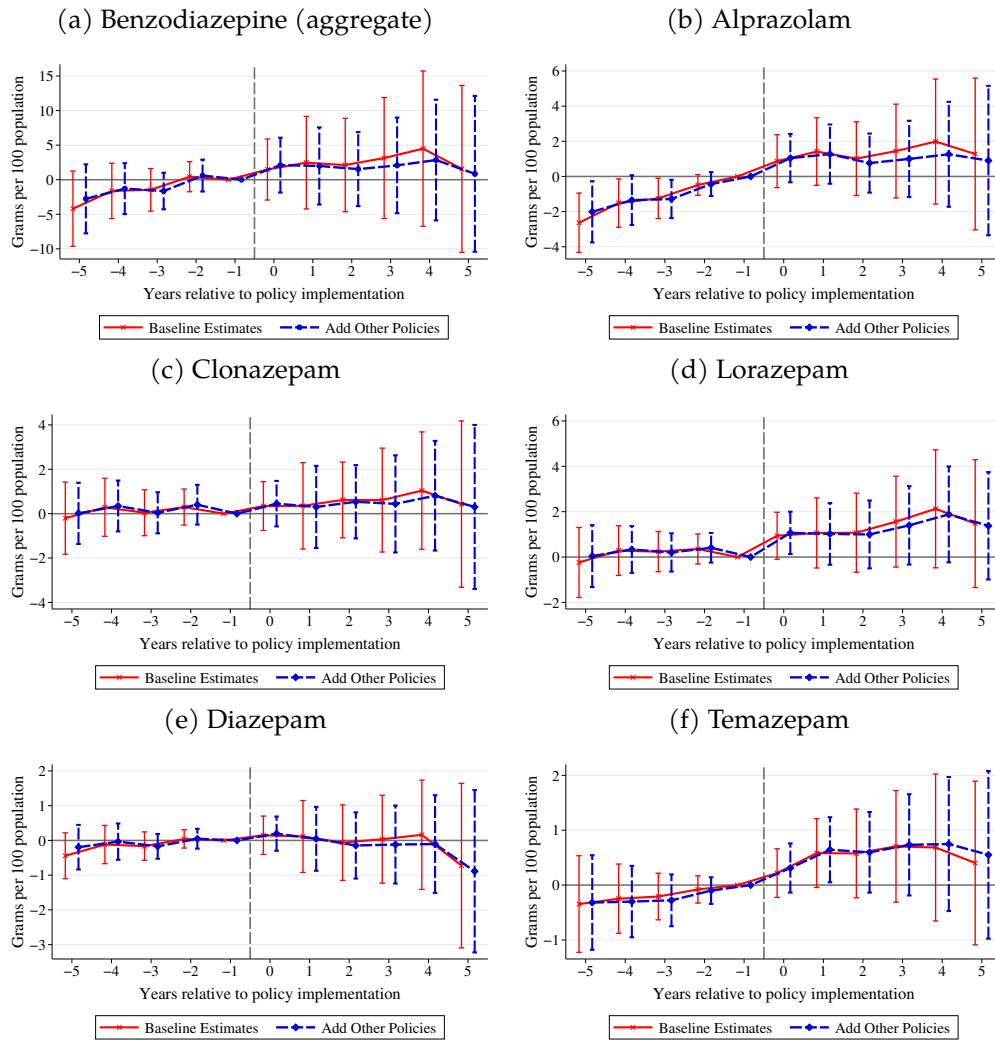
(a) Benzodiazepine (aggregate)

(b) Alprazolam

(c) Clonazepam

(d) Lorazepam

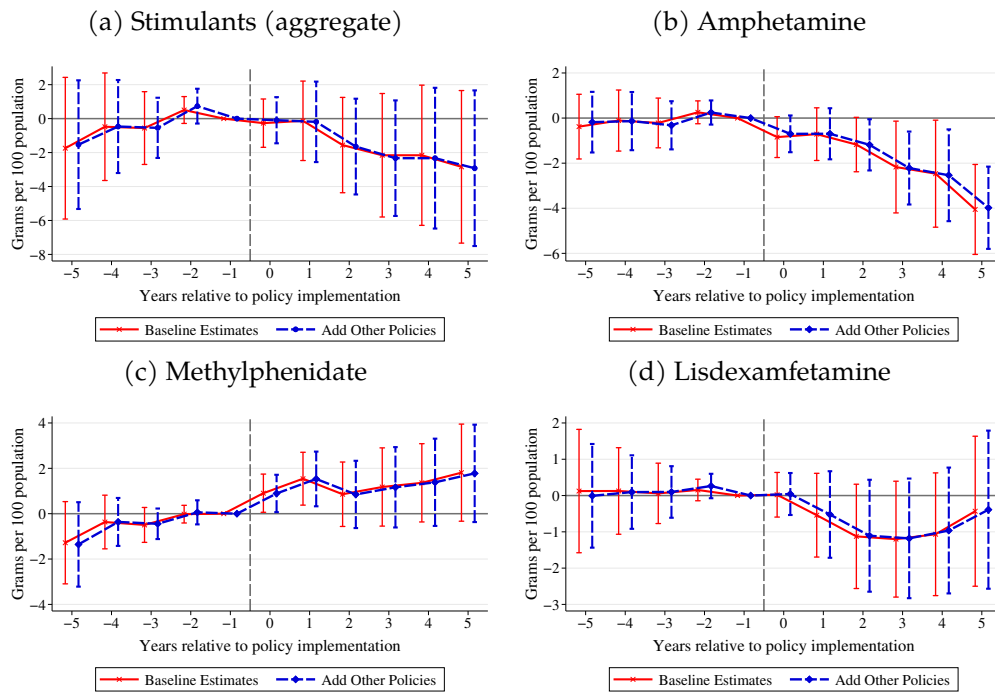
(e) Diazepam

(f) Temazepam

Notes: These figures show the robustness of our results to adding co-occurring opioid-related policies to the baseline model, as described in Section 5.1. The dependent variables are the number of benzodiazepine prescriptions per 100 Medicaid enrollees. The solid red line indicates the baseline estimates and their 95 percent confidence intervals, and the dashed blue line presents the point estimates and associated 95 percent confidence intervals obtained by adding to the baseline model (i) an indicator for whether the state has a naloxone access law (NAL), (ii) an indicator for having a Good Samaritan overdose prevention law, and (iii) an indicator for a pain management clinic law.

Figure A13: Robustness of the Stimulant Prescribing Results (Medicaid Data)

(a) Stimulants (aggregate)

(b) Amphetamine

(c) Methylphenidate

(d) Lisdexamfetamine

Notes: These figures show the robustness of our results to adding co-occurring opioid-related policies to the baseline model, as described in Section 5.1. The dependent variables are the number of stimulant prescriptions per 100 Medicaid enrollees. The solid red line indicates the baseline estimates and their 95 percent confidence intervals, and the dashed blue line presents the point estimates and associated 95 percent confidence intervals obtained by adding to the baseline model (i) an indicator for whether the state has a naloxone access law (NAL), (ii) an indicator for having a Good Samaritan overdose prevention law, and (iii) an indicator for a pain management clinic law.

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: Synthetic Control Analysis, Effects on Stimulant Distribution

	Aggregate (1)	Amphetamine (2)	Methyl. (3)	Lisdexamf. (4)
Synthetic Control Analysis				
Dependent variable: amphetamine equivalent stimulant grams per 100 population				
Immediate effect	0.112	-0.108	-0.057	-0.015
1-year effect	0.130	-0.129	-0.053	-0.033
2-year effect	0.006	-0.218	-0.076	-0.048
3-year effect	-0.012	-0.334	-0.117	-0.076
4-year effect	-0.318	-0.713	-0.233	-0.116
5-year effect	-0.904	-1.464	-0.654	-0.228
Outcome mean at -1	9.300	5.562	3.274	0.464

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from our synthetic control analysis (Appendix Figures A4).

Table A4: 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	-0.16	-0.57	0.76**	-0.15
	(0.59)	(0.37)	(0.31)	(0.23)
1-year effect	0.10	-0.37	1.49**	-0.71
	(1.25)	(0.59)	(0.57)	(0.59)
2-year effect	-1.20	-0.70	0.84	-1.43*
	(1.62)	(0.64)	(0.73)	(0.78)
3-year effect	-1.61	-1.49	1.21	-1.65*
	(2.02)	(1.05)	(0.90)	(0.84)
4-year effect	-1.46	-1.69	1.46	-1.62*
	(2.33)	(1.26)	(0.94)	(0.85)
5-year effect	-2.54	-3.75***	1.92	-1.05
	(2.65)	(1.13)	(1.23)	(1.15)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Time-varying covariates				
Mean at -1	20.754	5.490	9.570	5.400
Observations	425	441	449	430
R^2	0.846	0.755	0.770	0.831

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from equation (1). Although each regression includes a full set of indicators for pre- and post-periods, we report the coefficients on the post-periods above for brevity. 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 regressions include state and year fixed effects, as well as time-varying covariates (age and race compositions). 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.

Table A5: Synthetic Control Analysis, Effects on Stimulant Prescribing (Medicaid Data)

	Aggregate (1)	Amphetamine (2)	Methyl. (3)	Lisdexamf. (4)
Synthetic Control Analysis				
Dependent variable: Number of stimulant prescriptions per 100 enrollees				
Immediate effect	0.064	-0.431	1.289	-0.878
1-year effect	-0.517	-0.620	1.510	-1.904
2-year effect	-0.836	-0.704	0.824	-2.460
3-year effect	-1.596	-1.498	0.936	-3.103
4-year effect	-1.779	-1.293	0.982	-2.804
5-year effect	-4.431	-2.998	1.211	-3.083
Outcome mean at -1	21.460	5.960	9.810	5.560

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from our synthetic control analysis (Appendix Figure A6).

Table A6: Synthetic Control Analysis, Effects on Benzodiazepine Prescribing

	Aggregate (1)	Alprazolam (2)	Clonazepam (3)	Lorazepam (4)	Diazepam (5)	Temazepam (6)
Synthetic Control Analysis						
Dependent variable: Number of benzodiazepine prescriptions per 100 enrollees						
Immediate effect	3.796	1.088	1.007	0.076	0.302	0.032
1-year effect	2.929	0.786	0.868	0.211	0.127	0.080
2-year effect	4.561	1.335	1.331	-0.126	0.172	-0.040
3-year effect	4.659	1.425	1.367	0.060	0.353	0.034
4-year effect	5.028	1.779	0.963	-0.163	0.423	0.027
5-year effect	5.606	3.677	1.759	0.291	0.915	-0.016
Outcome mean at -1	33.912	10.482	9.849	6.122	4.020	1.257

Notes: This table shows the immediate effect, 1-year effect, 2-year effect, 3-year effect, 4-year effect, and 5-year effect from our synthetic control analysis (Appendix Figures A7).

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