Must-Access Prescription Drug Monitoring Programs and the Opioid Overdose Epidemic: The Unintended Consequences

Online Appendix

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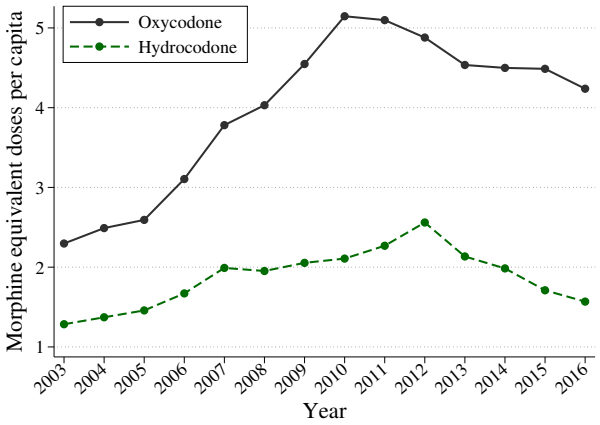
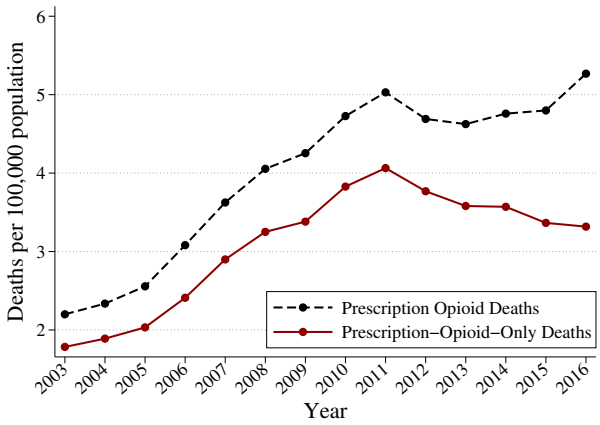
A Supplementary Figures and Tables

Figure A1: National Trends in the Legal Supply of Opioids and Prescription Opioid Death Rates

Panel A. The per capita legal supply of opioids

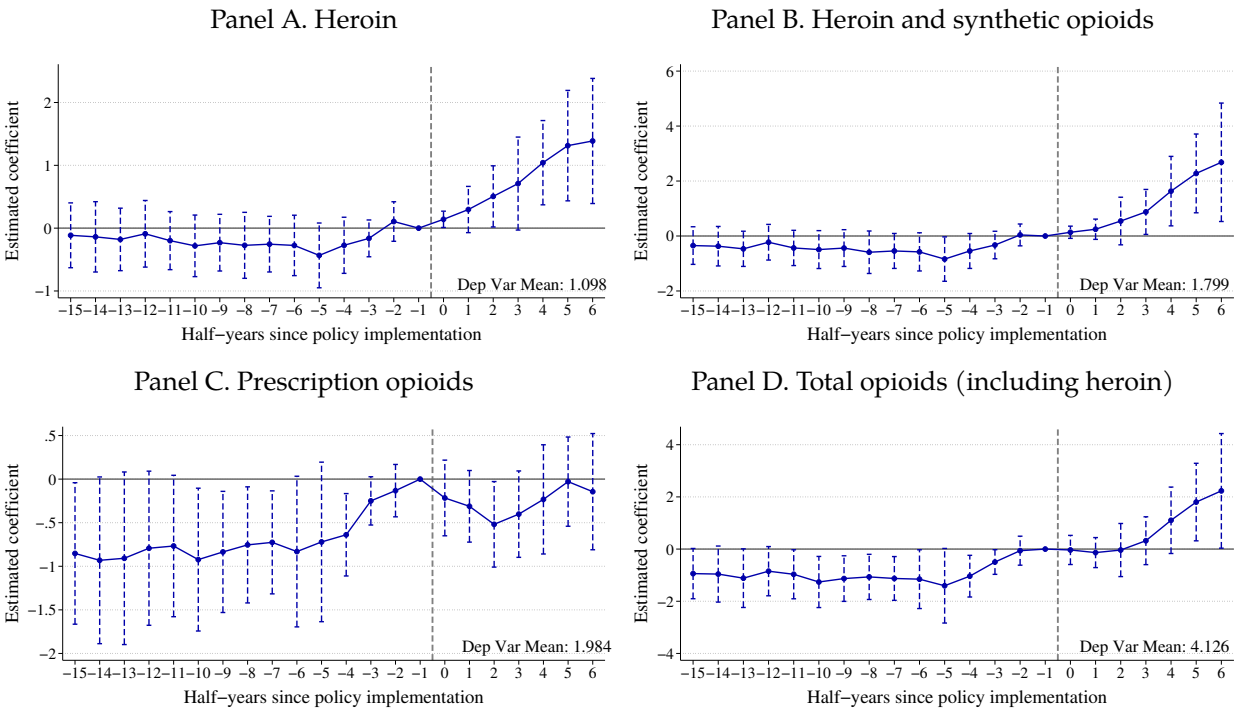


Panel B. Prescription opioid deaths per 100,000



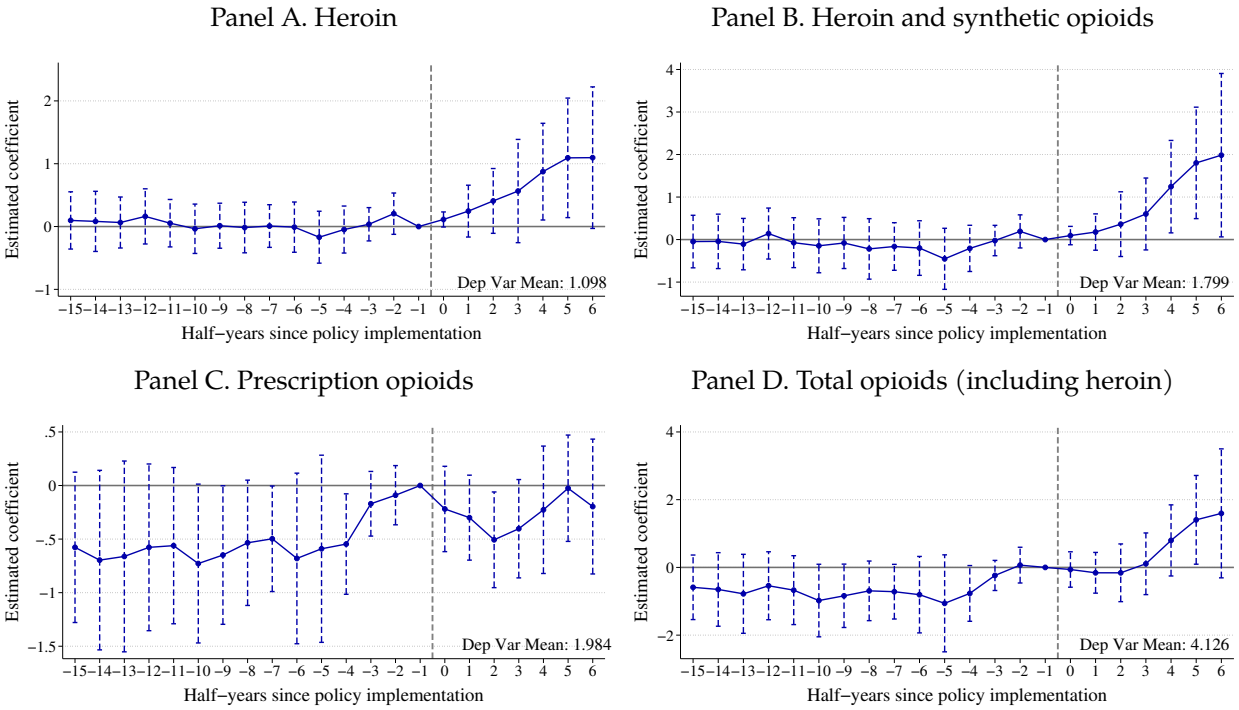
Notes: The figure plots the national trends in the per capita legal supply of opioids (Panel A) and Ruhm-corrected numbers of deaths from prescription opioids per 100,000 population (Panel B). The legal supply of oxycodone and hydrocodone in morphine equivalent doses obtained from the DEA's Automation of Reports and Consolidated Orders System (ARCOS). Ruhm-corrected numbers of deaths per 100,000 population are calculated using data from the National Vital Statistics System (NVSS). Drug overdose deaths are coded using ICD-10 underlying cause of death codes: X40–X44, X60–X64, X85, and Y10–Y14. Prescription opioid mortality rates, which use ICD-10 drug code T40.2, are identical to those in Figure 1. Prescription-opioid-only deaths indicate the deaths involved T40.2 but not T40.1, T40.3 or T40.4 at the time of death.

Figure A2: Baseline Results without Controls for the OxyContin Reformulation



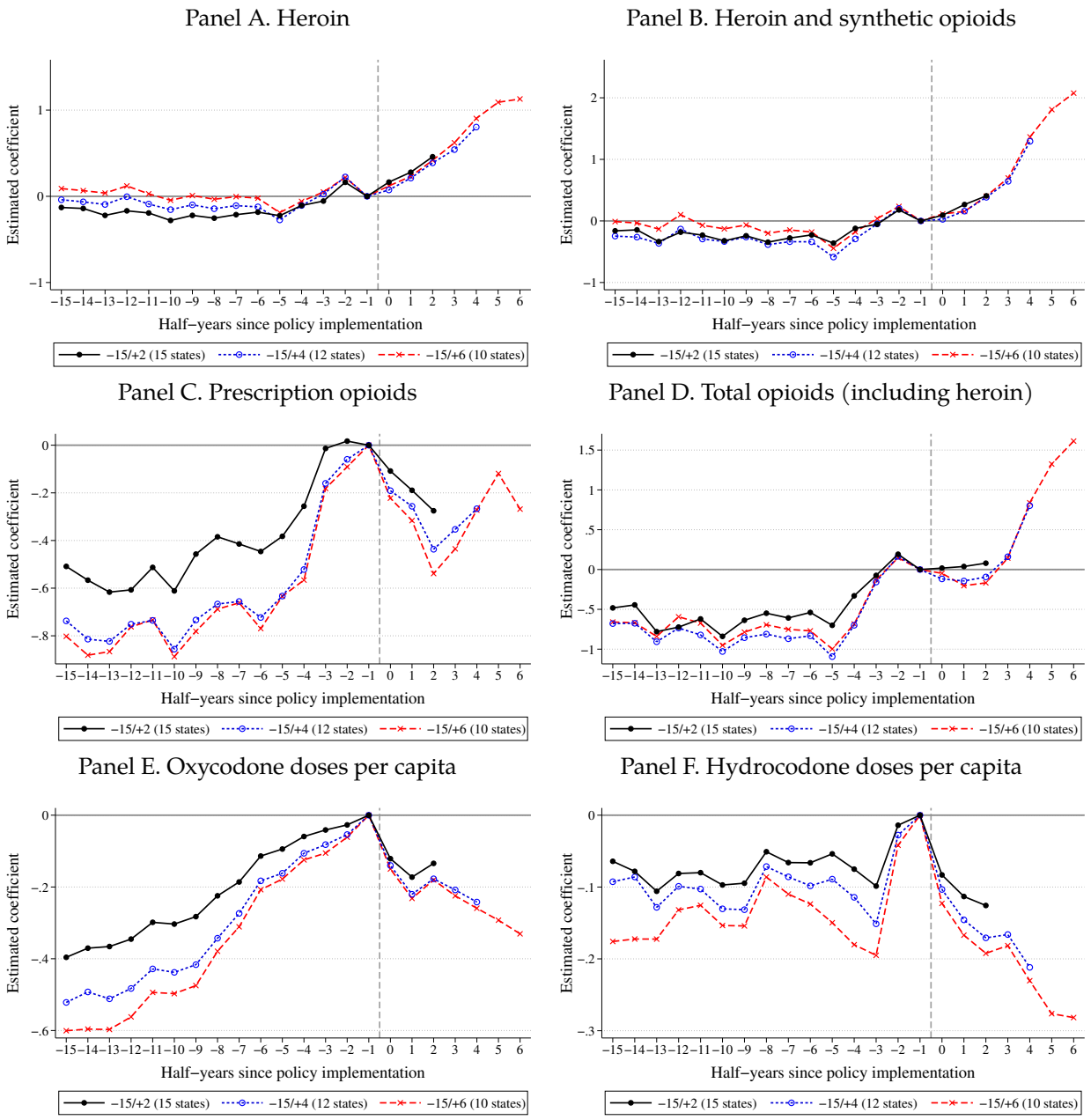
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) that I obtain when I do not control for the OxyContin reformulation. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D.

Figure A3: Baseline Results with the NSDUH Measure of OxyContin use



Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1). In all panels, the NSDUH measure of OxyContin use, instead of the ARCOS measure, is interacted with time fixed effects to account for exposure to the reformulation. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D.

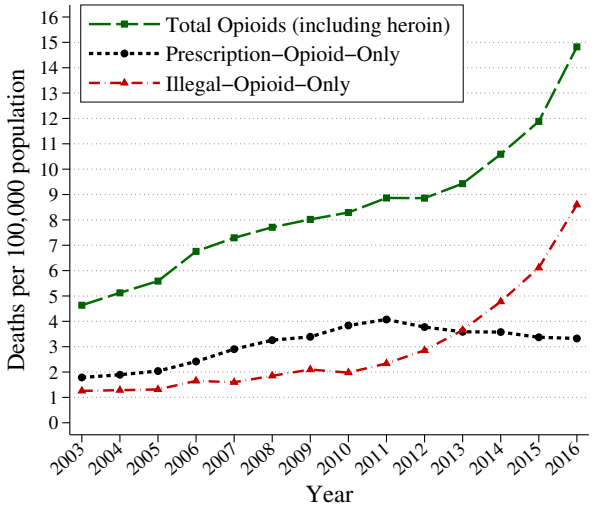
Figure A4: Baseline Results with Different Event Time Windows



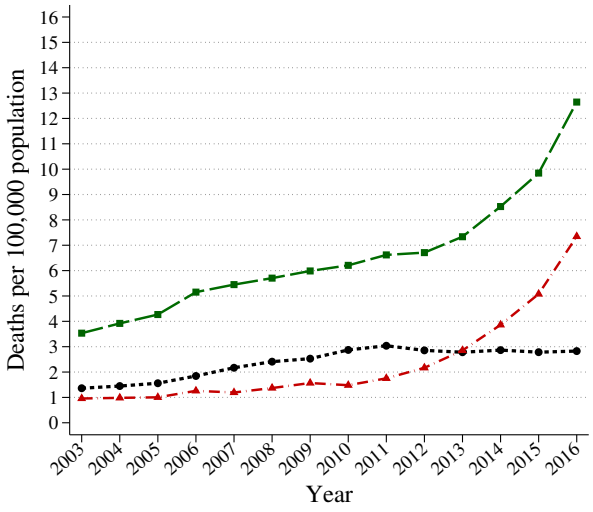
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) on three samples with different event time windows (separate regressions). Each sample includes treated states that were consistently observed during one of the following event time windows: -15/+6, -15/+4, or -15/+2. The control group is the same across samples. The dashed red line presents the baseline estimates, obtained using the sample that includes the ten treated states that are consistently observed during the broadest event time window (-15/+6). The short-dashed blue line corresponds to the 12 treated states that are observed during the -15/+4 event time window. The black solid line corresponds to the 15 treated states that are observed during the narrowest event time window (-15/+2). The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Florida is excluded from the control sample in all panels (see Appendix Section C). The controls are identical to those in Figure 3.

Figure A5: National Trends in the Exclusive Measures of Opioid Death Rates

Panel A. Ruhm-corrected drug overdose deaths

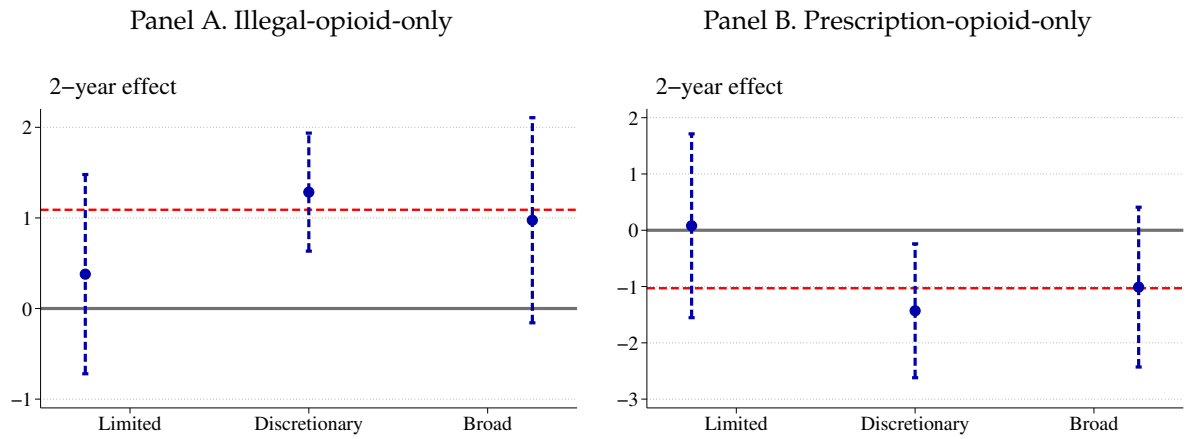


Panel B. Reported drug overdose deaths



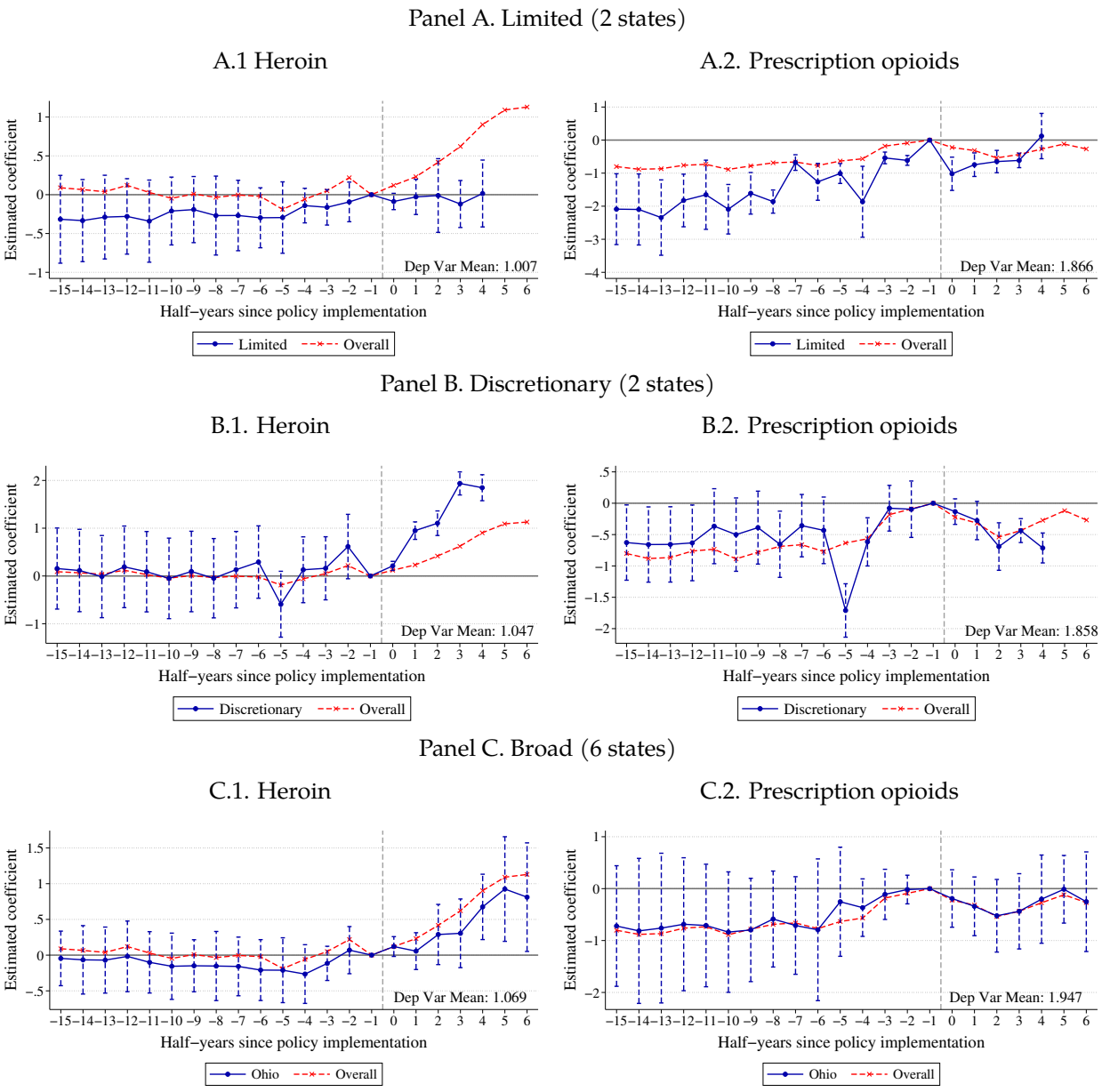
Notes: The figure plots the national trends in corrected and reported numbers of deaths per 100,000 population calculated using mortality data from the National Vital Statistics System (NVSS). Drug overdose deaths are coded using ICD-10 underlying cause of death codes: X40–X44, X60–X64, X85, and Y10–Y14. To identify drug involvement, the following four drug identification codes are used: heroin (T40.1), natural and semisynthetic opioids such as oxycodone and hydrocodone (T40.2), methadone (T40.3), and synthetic opioids excluding methadone, such as fentanyl (T40.4). I calculate total deaths from any opioid, including heroin, by combining T40.1–T40.4. Prescription-opioid-only deaths indicate the deaths involved T40.2 but not T40.1, T40.3, or T40.4 at the time of death. Illegal-opioid-only deaths indicate the deaths involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death. Reported mortality rates are based on mentions of the specified drugs on the death certificates. Corrected mortality rates are estimated by using the method suggested by Ruhm (2018), which uses information from death certificates that specified at least one drug category to impute drug involvement for cases in which none was specified.

Figure A6: Heterogeneous Treatment Effects on Exclusive Mortality Outcomes



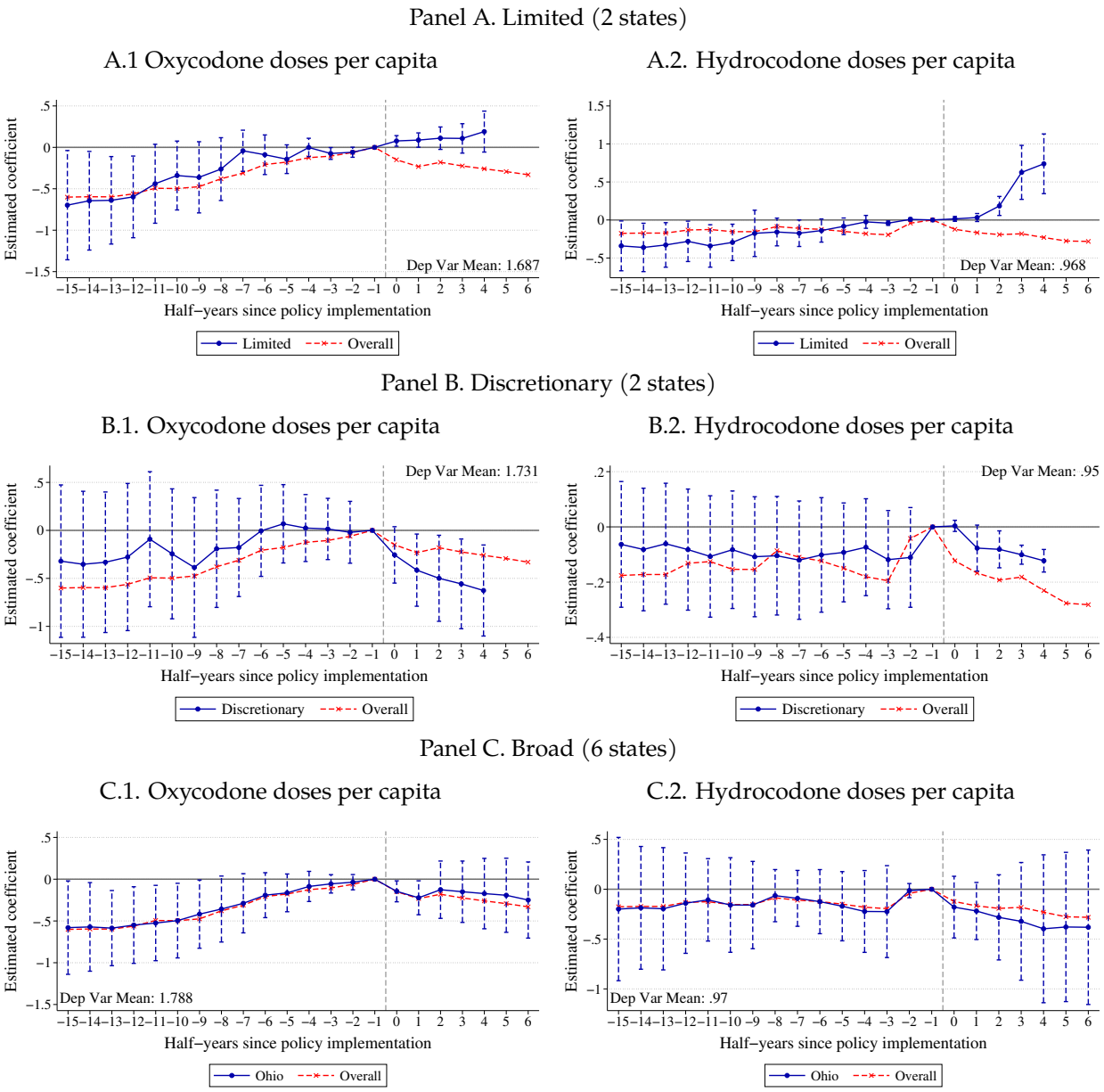
Notes: The figure shows the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). Each panel presents the estimates I obtain when I interact the indicators for all the post-periods (from 0 to +6) from the baseline specification (equation 1) with three indicators for limited, discretionary and broad laws. The (horizontal) dashed red line presents the overall estimate, for reference. In Panel A, the dependent variable is illegal-opioid-only deaths per 100,000, which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death. In Panel B, the dependent variable is prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4. In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. For all the outcomes, I report the trend break estimates summarizing the two-year effect ($\Delta 5_j = (\beta_{4j} * \mathbf{1}(\text{Law Type } j) - \beta_{-1}) - (\beta_{-1} - \beta_{-6})$). I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. In all panels, the analysis sample and controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

Figure A7: Separate Event Studies by Law Type—Effects on Opioid Death Rates



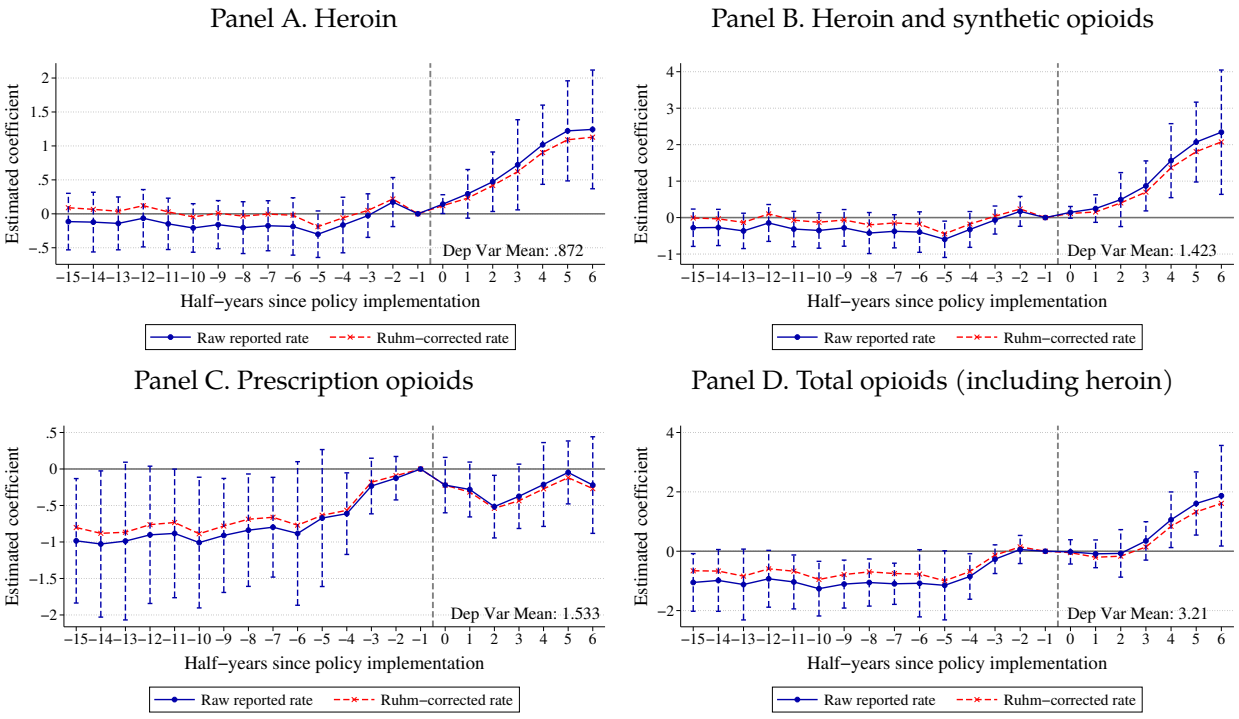
Notes: The figure presents the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I limit the treated group to each of the three law type. In all panels, the treated sample is balanced in relative periods from -15 to +6, and the distant relative periods outside the -15/+6 event time window are trimmed. In Panels A and B, I additionally trim event times +5 and +6, since three states strengthened their must-access laws during my sample period (see Section 5.6). The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. In all panels, the control sample is the baseline control sample. In the left column (Panels A.1, B.1, and C.1), the dependent variable is heroin deaths per 100,000 (drug code T40.1). In the right column (Panels A.2, B.2, and C.2), the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In all panels, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In Panels A and B, since there are only two treated states in each regression, caution in the interpretation of confidence intervals is needed. The controls are identical to those in Figure 3.

Figure A8: Separate Event Studies by Law Type—Effects on Opioid Supply



Notes: The figure presents the heterogeneous treatment effects across my three types of must-access laws: (i) limited laws that apply to certain ingredients or require access under limited circumstances (2 states), (ii) discretionary laws that rely on provider suspicion of abuse (2 states), and (iii) broad laws without such restrictions (6 states). The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I limit the treated group to each of the three law type. In all panels, the treated sample is balanced in relative periods from -15 to +6, and the distant relative periods outside the -15/+6 event time window are trimmed. In Panels A and B, I additionally trim event times +5 and +6, since three states strengthened their must-access laws during my sample period (see Section 5.6). The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. In all panels, the control sample is the baseline control sample. In the left column (Panels A.1, B.1, and C.1), the dependent variable is oxycodone (morphine equivalent) doses per capita. In the right column (Panels A.2, B.2, and C.2), the dependent variable is and hydrocodone (morphine equivalent) doses per capita. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In Panels A and B, since there are only two treated states in each regression, caution in the interpretation of confidence intervals is needed. The controls are identical to those in Figure 3.

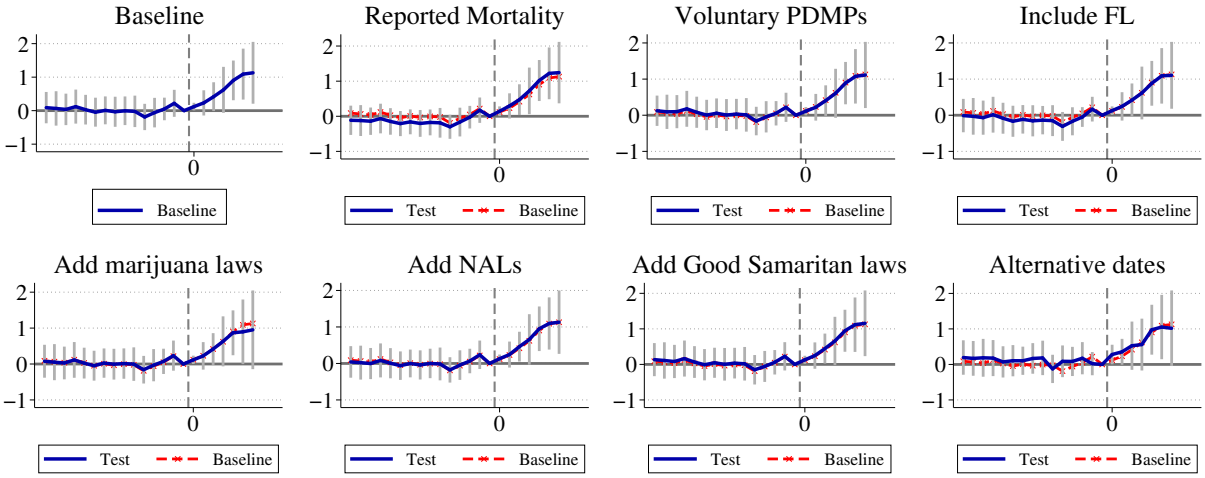
Figure A9: Effects of Must-Access PDMPs on Raw Reported Opioid Death Rates



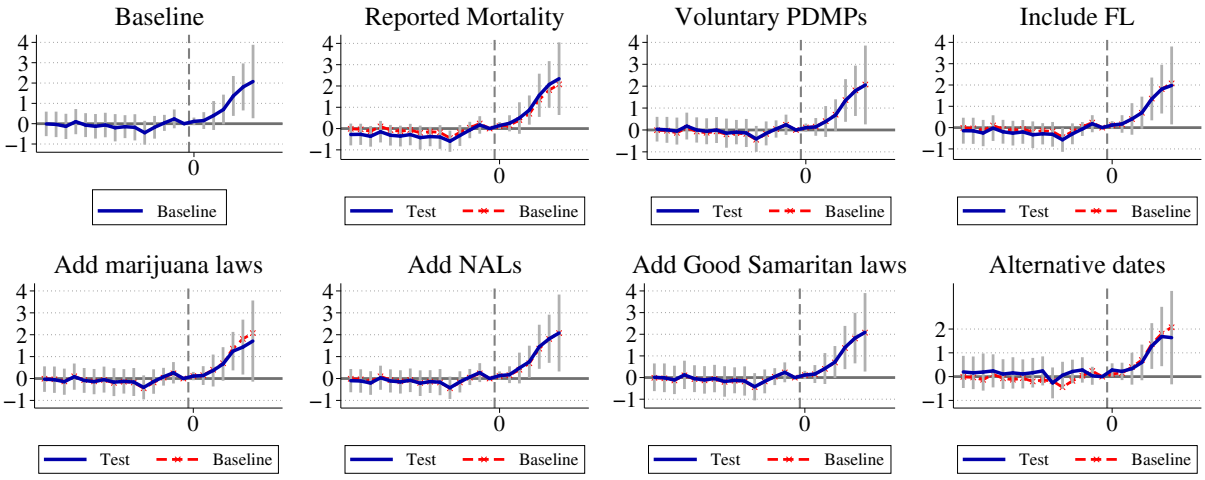
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) that I obtain when I use the raw reported death rates instead of the Ruhm-corrected death rates. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. In Panel A, the dependent variable is heroin deaths per 100,000 (drug code T40.1). In Panel B, the dependent variable is the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In Panel C, the dependent variable is prescription opioid deaths per 100,000 (drug code T40.2). In Panel D, the dependent variable is total deaths from any opioid, including heroin (drug codes T40.1–T40.4). The raw reported numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in all panels. Observations are weighted by state population. The sample and controls are identical to those in Figure 3.

Figure A10: Robustness of the Baseline Mortality Estimates to Alternative Explanations

Panel A. Heroin

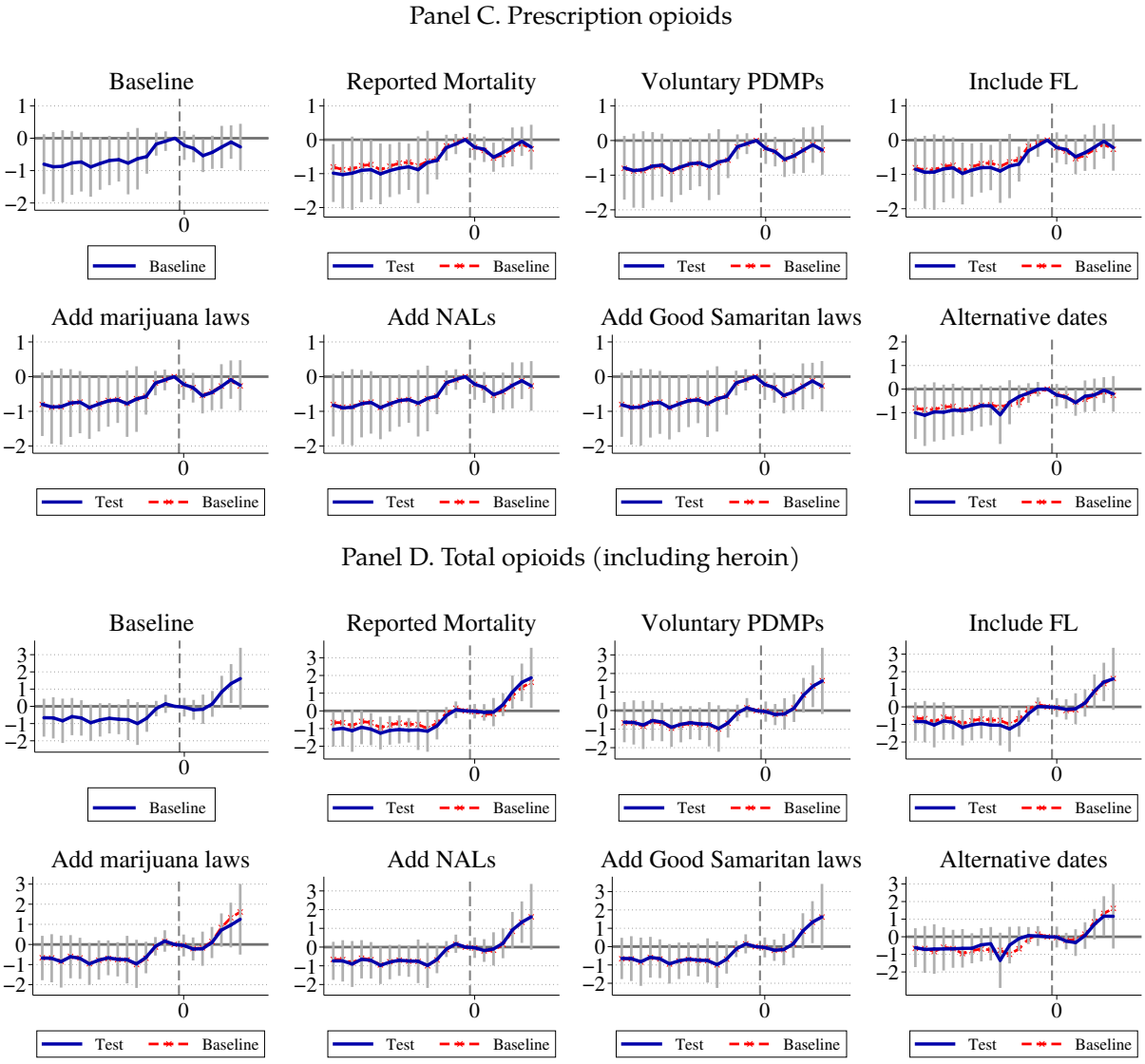


Panel B. Heroin and synthetic opioids



(continued)

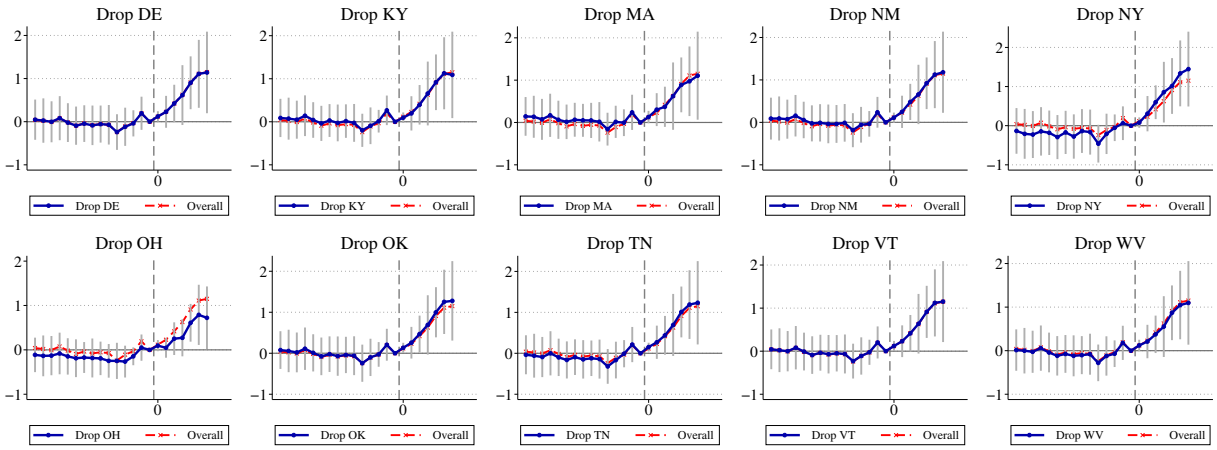
Figure A10: Robustness of the Mortality Estimates to Alternative Explanations (continued)



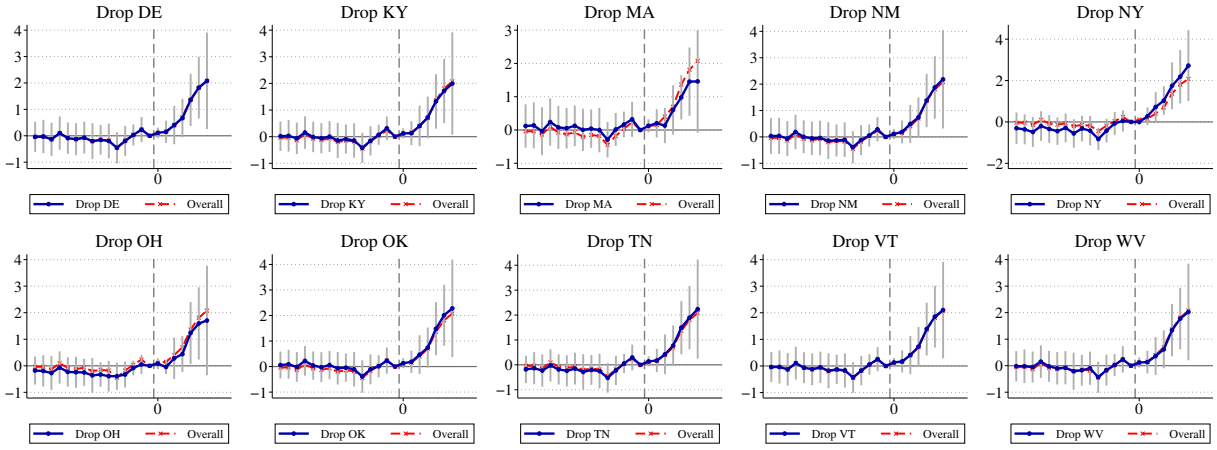
Notes: The figure shows the robustness of the baseline estimates to several sensitivity tests. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, the combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4) in Panel B, prescription opioid deaths per 100,000 (drug code T40.2) in Panel C, and total deaths from any opioid, including heroin (drug codes T40.1–T40.4) in Panel D. The estimates in Panel A are identical to those presented in Table 6. The estimates in Panels B, C, and D are identical to those presented in Appendix Table A4.

Figure A11: Robustness of the Mortality Estimates to Dropping One Treated State

Panel A. Heroin



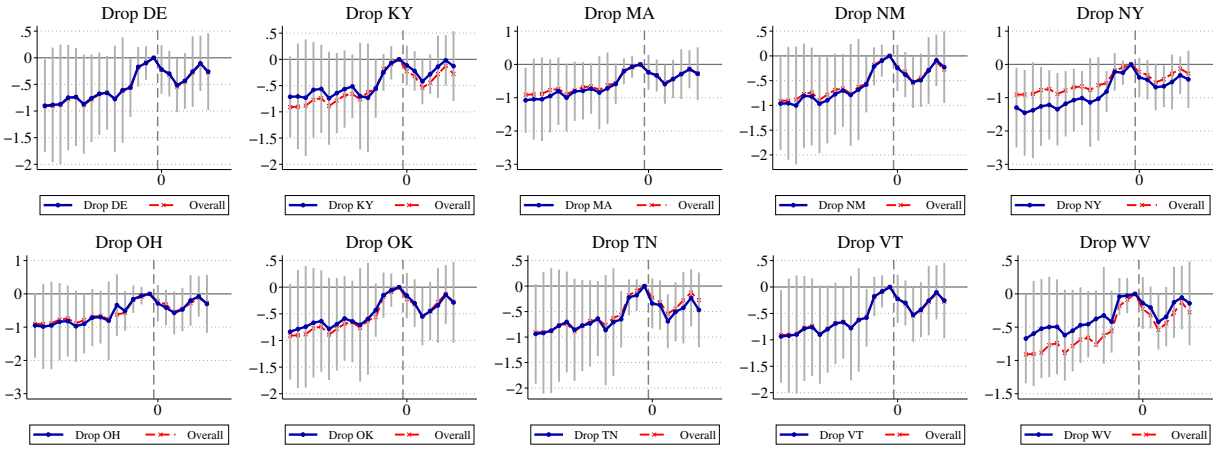
Panel B. Heroin and synthetic opioids



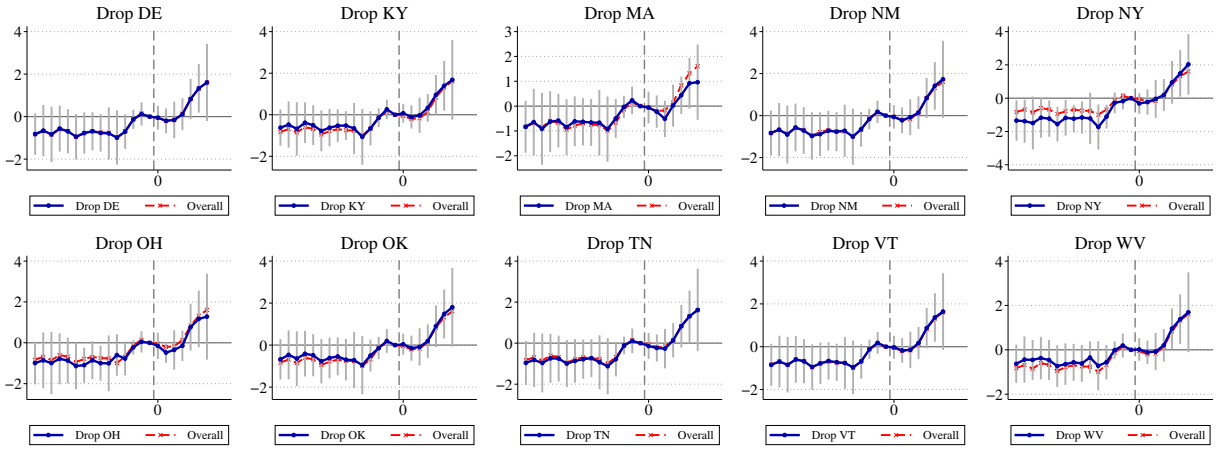
(continued)

Figure A11: Robustness of the Mortality Estimates to Dropping One Treated State (continued)

Panel C. Prescription opioids

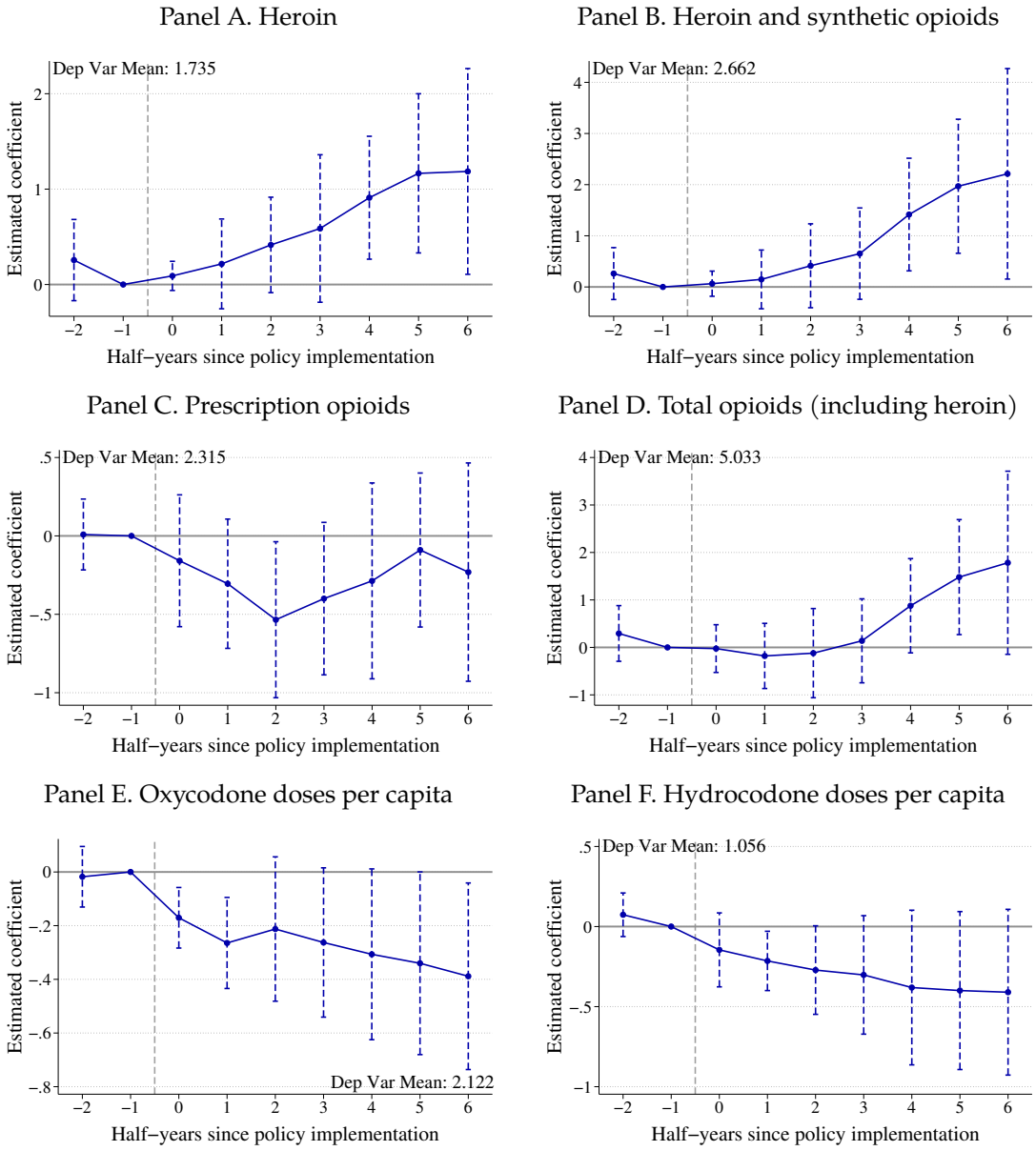


Panel D. Total opioids(including heroin)



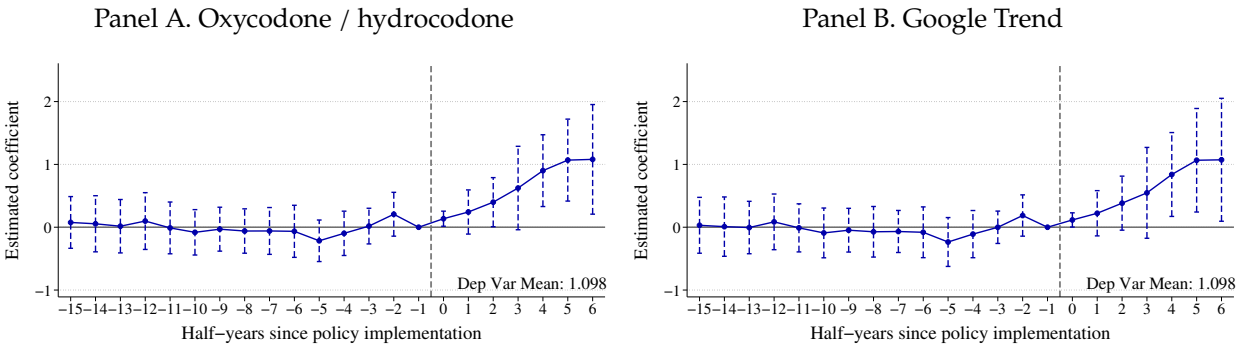
Notes: The figure shows the sensitivity of the mortality estimates to dropping one of the ten treated states. Each panel displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I drop one treated state. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The vertical dashed gray line indicates the implementation timing of a must-access PDMP. The dashed red line presents the baseline estimates indicating the overall effects among all the ten treated states. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, and total opioid-related death rate (T40.1–T40.4) in Panel D. Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in all panels. The control states are the 34 that did not implement must-access policies until 2016h2. Florida is excluded from the control sample (see Appendix Section C). Observations are weighted by state population. In all panels, the controls are identical to those in Figure 3.

Figure A12: Robustness of the Baseline Estimates to Dropping the Pre-Reformulation Period



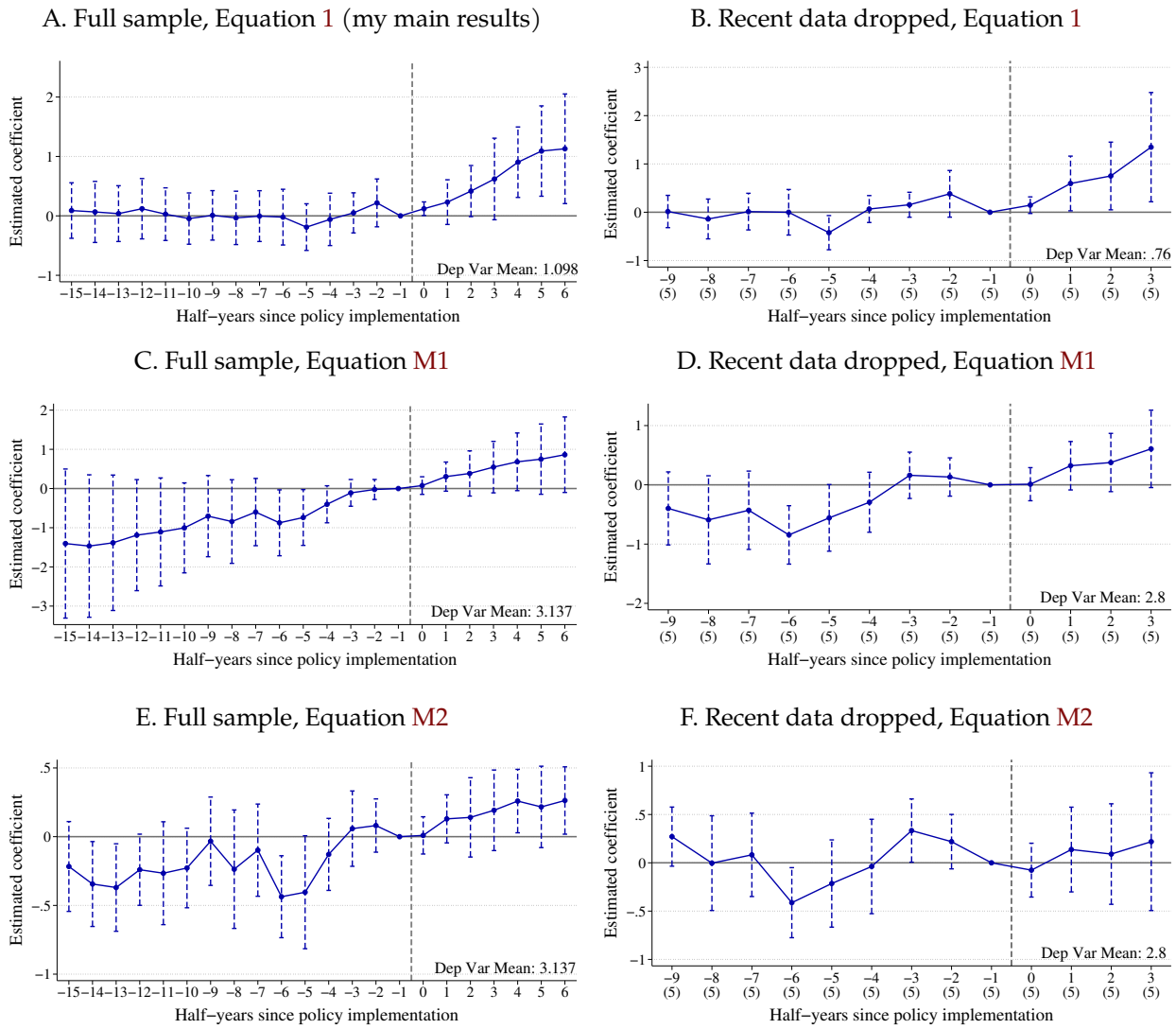
Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline specification (equation 1) obtained when I drop the pre-reformulation period (the reformulation was introduced in 2010h2). The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. In Panels A–D, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treatment states are the nine that implemented must-access PDMPs from 2011h2 to 2013h2, and the treated sample is balanced in relative periods from -2 to +6. The distant relative periods outside the -2/+6 event time window are trimmed. The control states are the 34 that did not implement must-access policies until 2016h2, and the control sample is balanced from 2010h2 to 2016h2. Florida is dropped (see Appendix Section C). Observations are weighted by state population. The controls are identical to those in Figure 3.

Figure A13: Alternative Measures of Pre-Reformulation OxyContin Use



Notes: The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I use two alternative measures of pre-reformulation OxyContin use (separate regressions): Panel A uses oxycodone/hydrocodone in morphine equivalent doses per capita, and Panel B uses the Google Trend measure obtained from Beheshti (2019). The dependent variable is heroin deaths per 100,000 (drug code T40.1). Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The controls are identical to those in Figure 3.

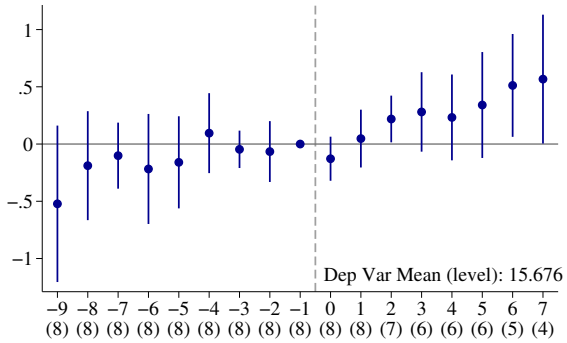
Figure A14: My Analysis Sample with Prior Literature Specification



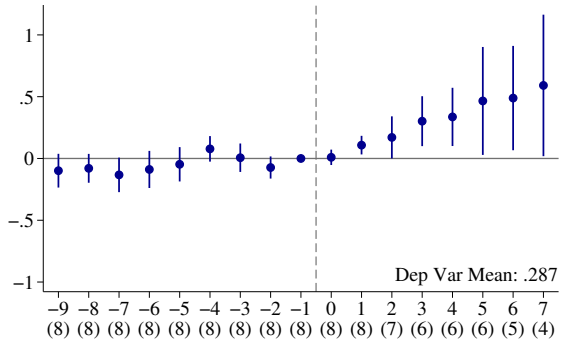
Notes: Each column corresponds to different sample periods. In the left column (Panels A, C, and E), I use my full sample (2003h1–2016h2), and in the right column (Panels B, D, and F), I drop the 2014h1–2016h2 period and reconstruct the sample so that the treated states are balanced in relative periods from -9 to +3. The sample from the right column includes the five treated states. The number of treated states observed in each time period is presented in the parentheses below that period. In all panels, the treated sample is balanced in relative periods, and the distant relative periods outside the given event time window are trimmed. Each row uses one of the three specifications: the top row (Panels A and B) uses my baseline specification (equation 1), the middle (Panels C and D) uses Meinhofer's (2018) preferred event study specification (equation M1), and the bottom (panels E and F) uses Meinhofer's (2018) alternative specification (equation M2). The regressions estimating my specification (equation 1) are weighted by population, while the regressions estimating Meinhofer's (equations M1 and M2) are unweighted. See Appendix Section B for a detailed description.

Figure A15: Meinhofer's (2018) Analysis Sample with My Specification

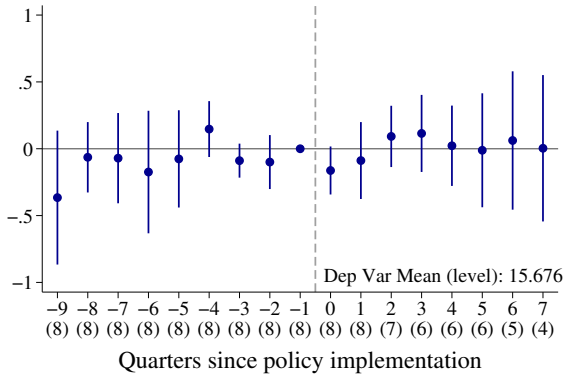
A. Meinhofer's sample, Equation M1
(Replication of Meinhofer (2018))



B. Meinhofer's sample, my specification

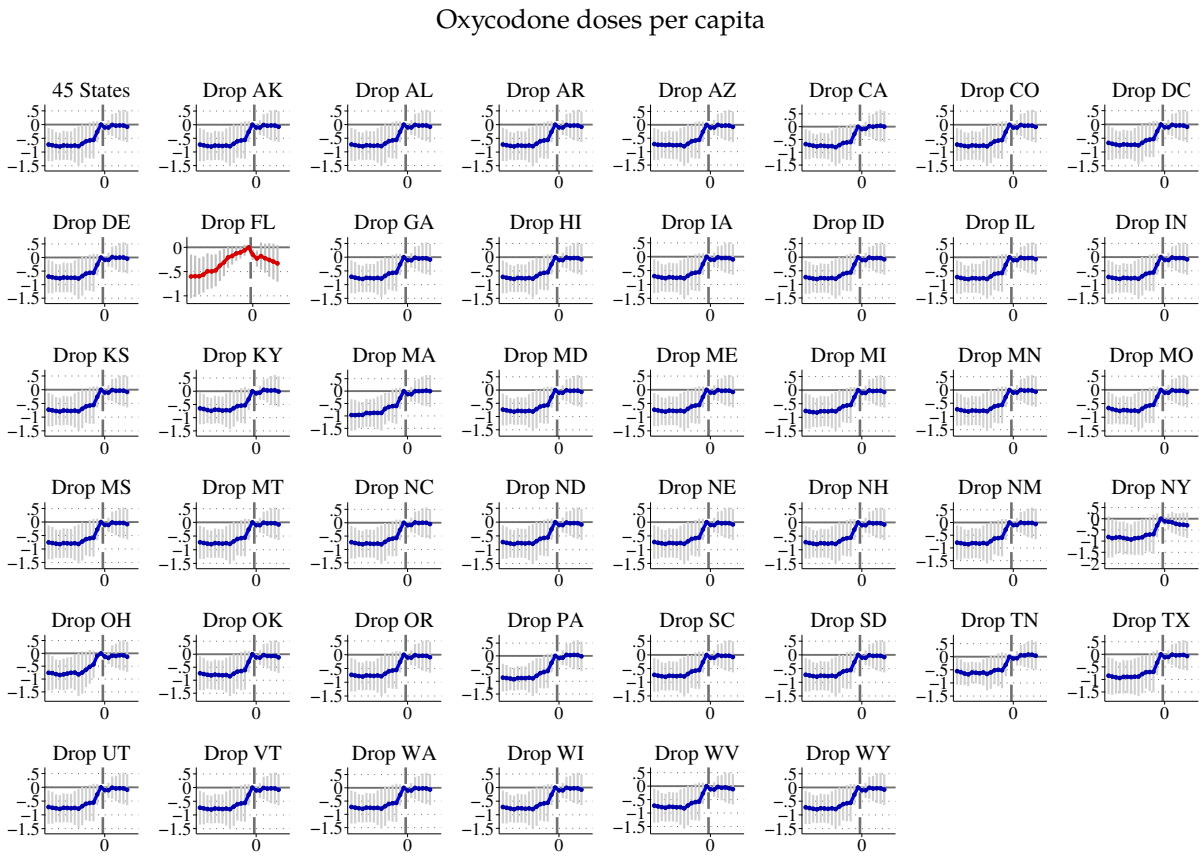


C. Meinhofer's sample, Equation M2



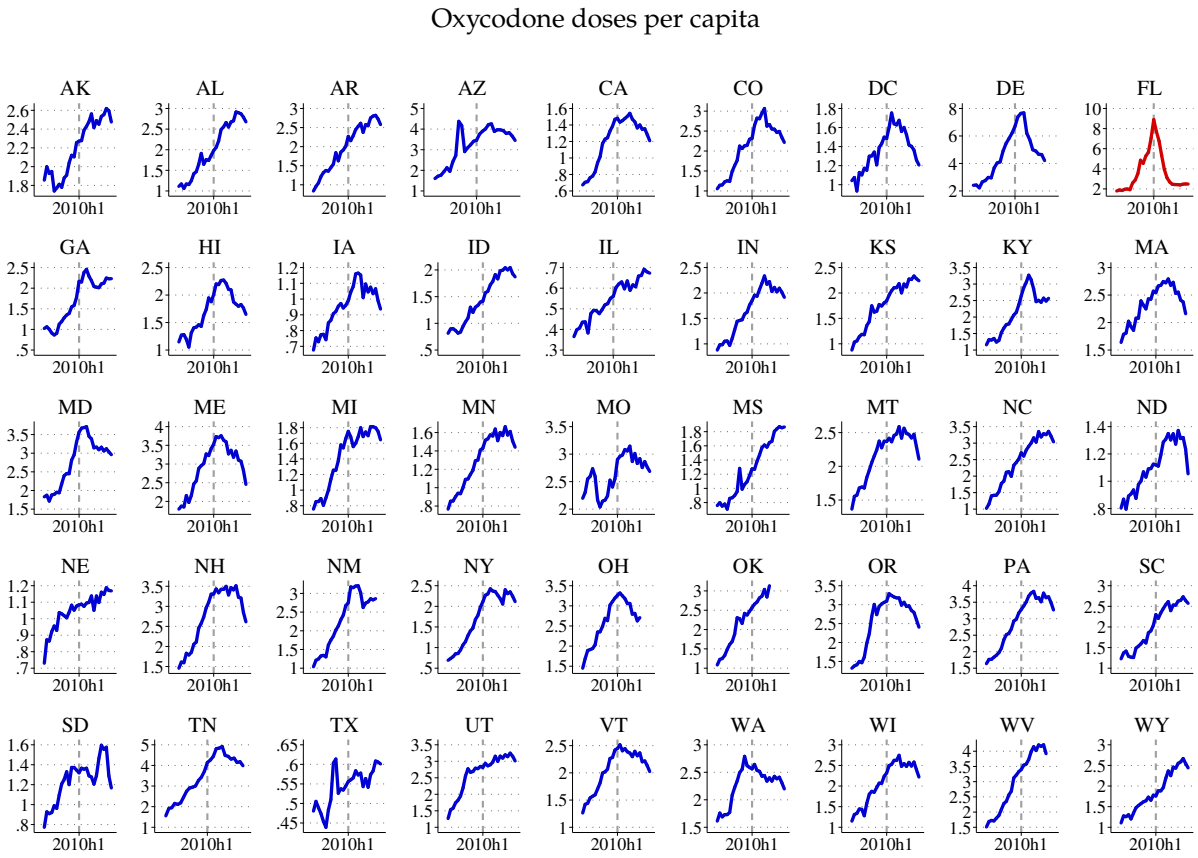
Notes: The figure shows how Meinhofer's (2018) heroin results are affected if I use my specification instead. Panel A displays the replication of Meinhofer's (2018) event study results for heroin mortality, and Panel B presents the estimates that I obtain when I use my baseline specification (equation 1) instead, while everything else, including the analysis sample from Panel A, remains unchanged. Panel C shows the results I obtain when I use Meinhofer's (2018) alternative specification (equations M2) instead. The regression estimating my specification (equation 1) is weighted by population, while the regression estimating Meinhofer's (equations M1 and M2) is unweighted. In all panels, the sample is unbalanced in relative (quarter) periods from -9 to +7. The number of treated states observed in each event time period is presented in the parentheses below that period. The distant relative periods that are outside the -9/+7 event time window are dropped. The control sample is balanced from 2000q1 to 2013q4. See Appendix Section B for a detailed description.

Figure A16: Sensitivity of Oxycodone Results to Dropping Florida



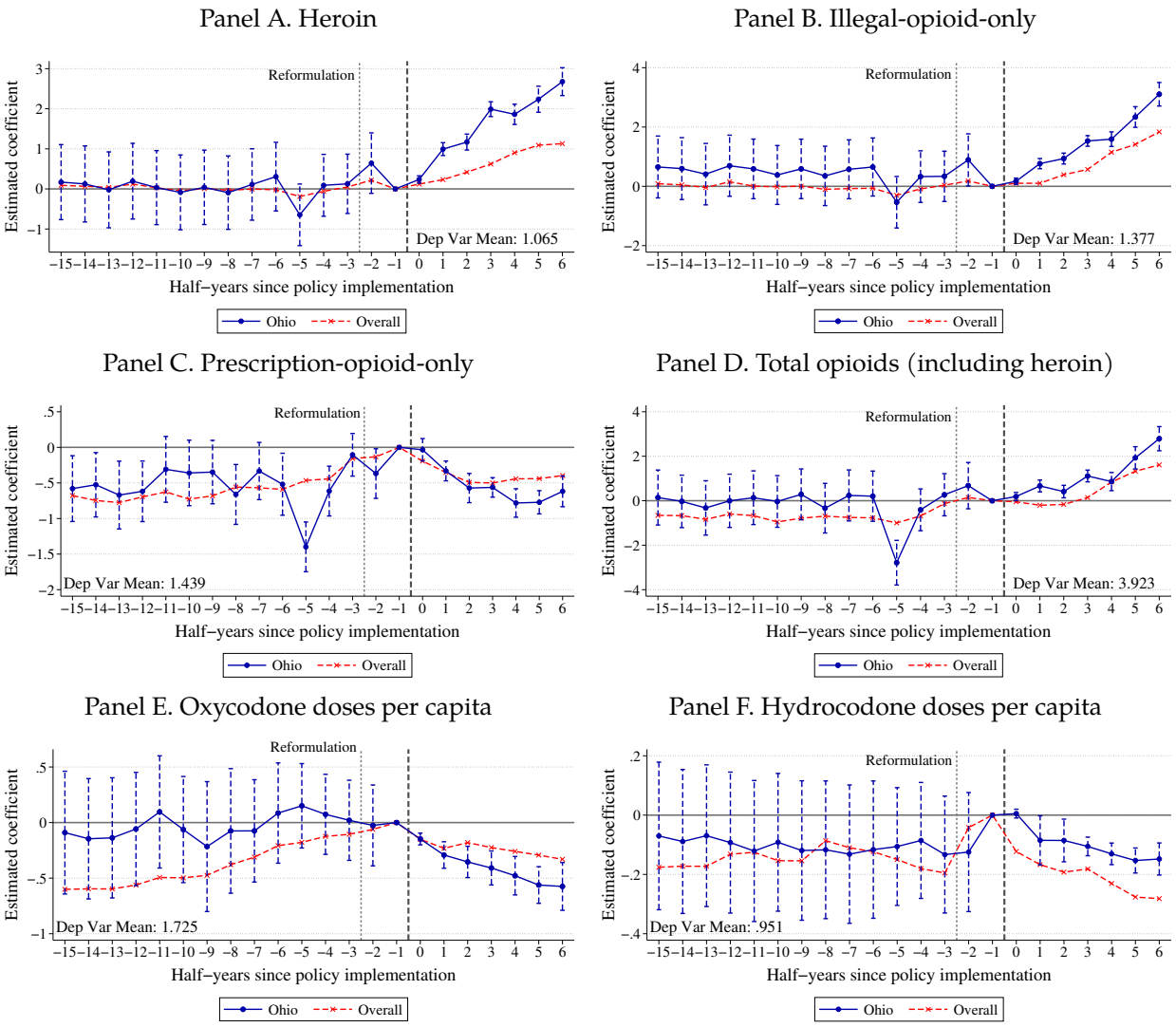
Notes: The top left panel displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I include Florida in my analysis sample. The subsequent panels show the sensitivity of the top left panel's estimates to removing one of the 45 states. In the sample from the top left panel, the treated states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 35 that did not implement must-access policies until 2016h2. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. The dependent variable is oxycodone (morphine equivalent) doses per capita. See Appendix Section C for a detailed description.

Figure A17: Trends in Per Capita Legal Supply of Oxycodone by State



Notes: Each panel displays the trends in a state's legal supply of oxycodone (morphine equivalent) doses per capita in the half-year period. The dashed gray line indicates 2010h1. See Appendix Section C for a detailed description.

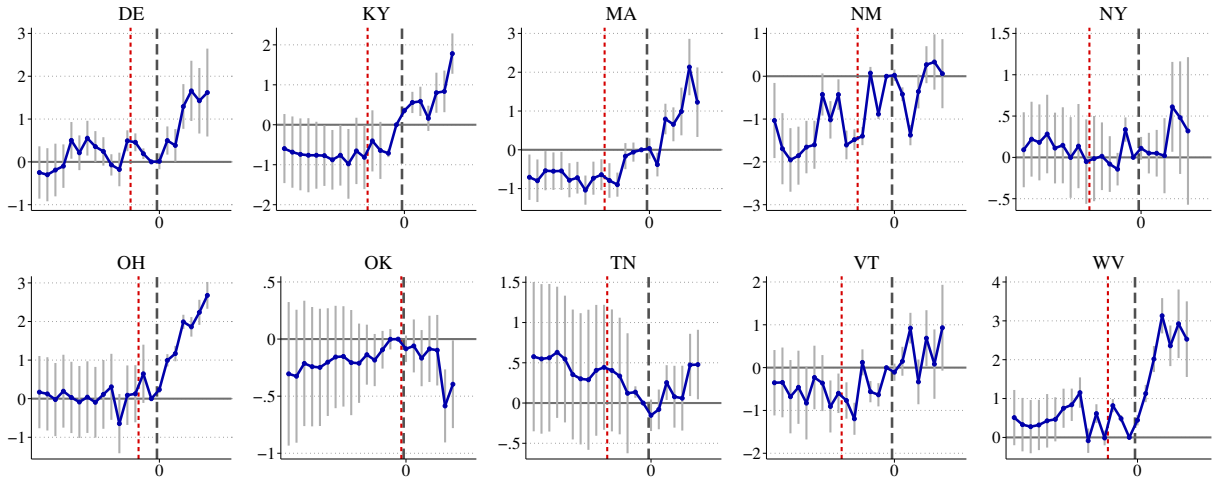
Figure A18: Effects of the Must-Access PDMP within Ohio



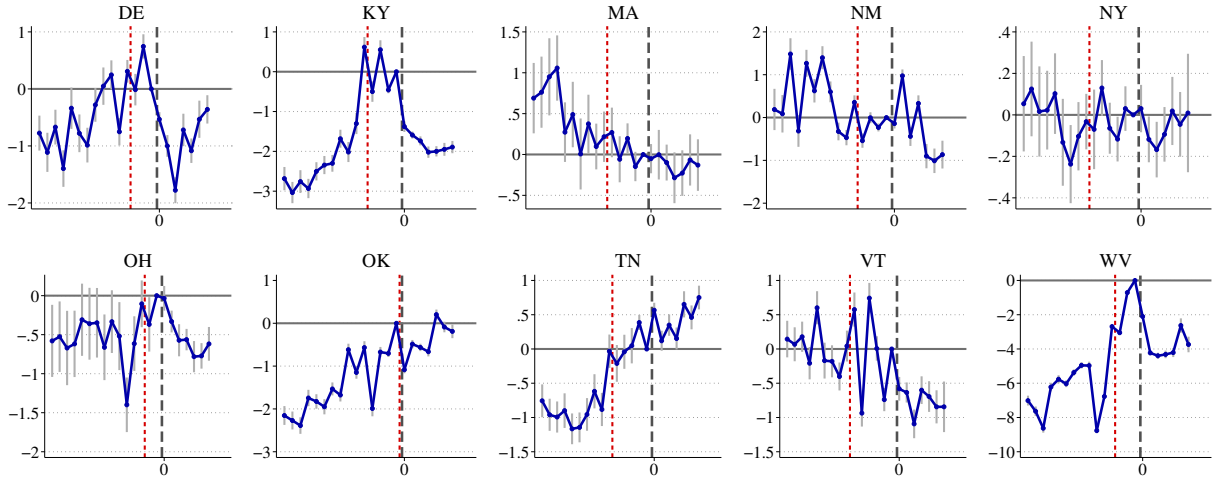
Notes: The figure presents the effect of the must-access PDMP within Ohio. The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I drop all the treated states except for Ohio. The last pre-period is omitted. The dashed red line indicates the overall effects of must-access PDMPs among the ten treated states. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed gray line indicates 2010h2, when the OxyContin reformulation was introduced (between event times -3 and -2). In all panels, the control sample is the baseline control sample. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal-opioid-only deaths per 100,000, which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death in Panel B, prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4 in panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. Observations are weighted by state population. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Since there is only one treated state in each regression, caution in the interpretation of confidence intervals is needed. The controls are identical to those in Figure 3. Fixed effects for state and half-year are always included.

Figure A19: Effects of the Must-Access PDMP within a Single State

Panel A. Heroin



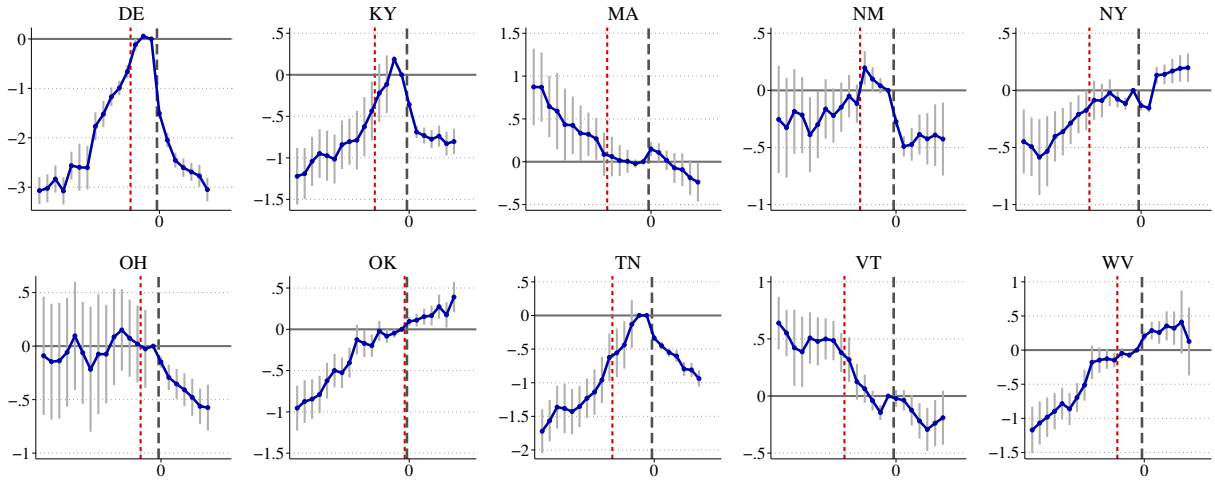
Panel B. Prescription-opioid-only



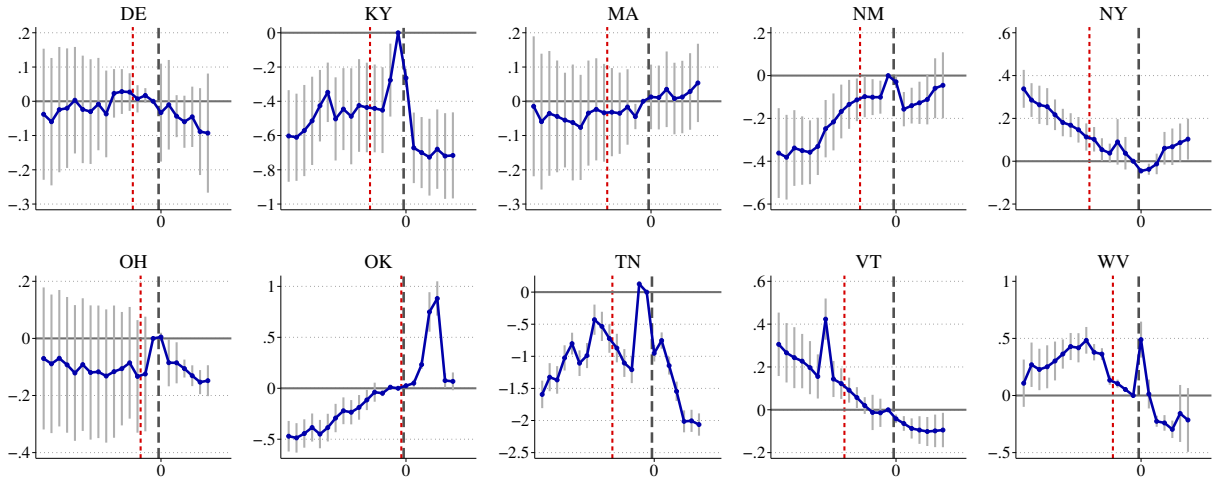
(continued)

Figure A19: Effects of the Must-Access PDMP within a Single State (continued)

Panel C. Oxycodone doses per capita

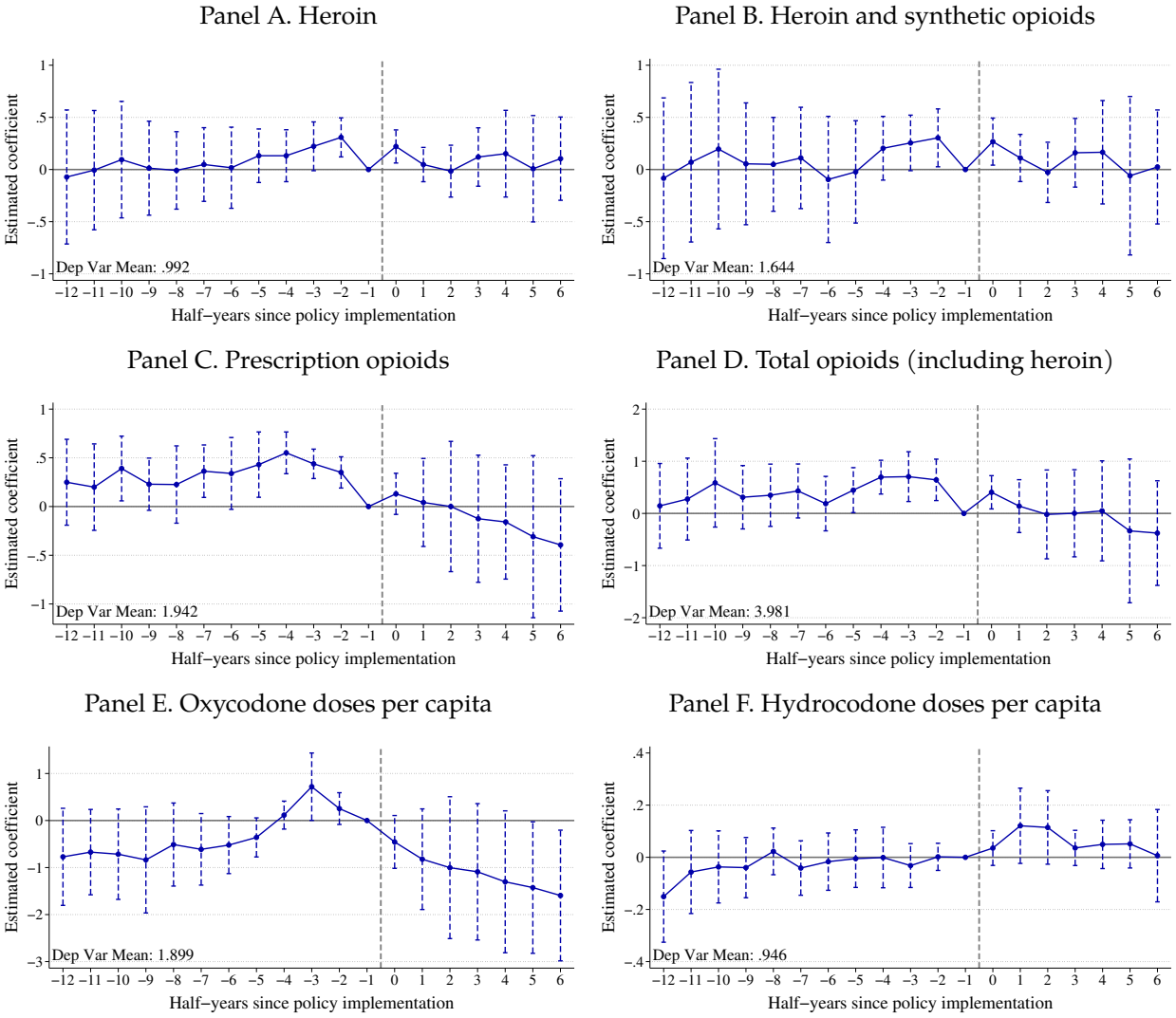


Panel D. Hydrocodone doses per capita



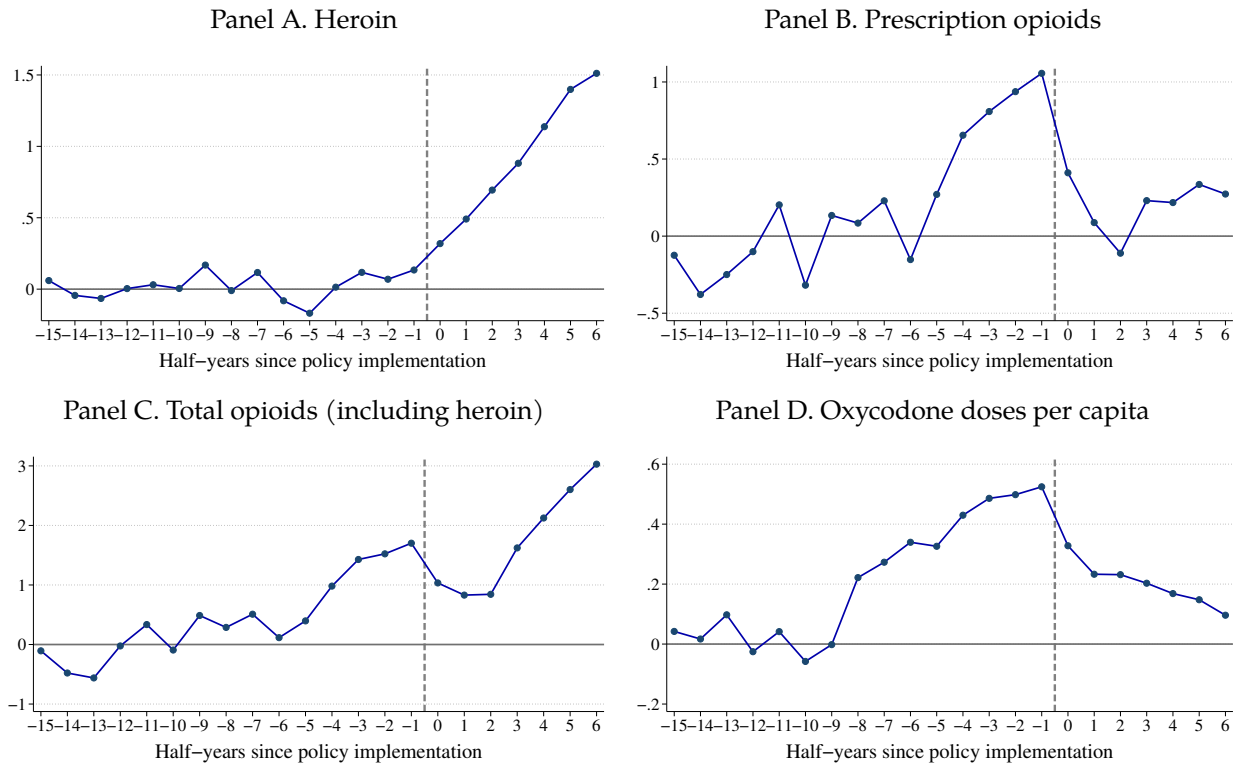
Notes: The figure shows the effect of must-access PDMP within a single treated state. The figure displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences model (equation 1) obtained when I drop all the treated states except for one. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription-opioid-only deaths, which involved T40.2 but not T40.1, T40.3, or T40.4 in panel B, oxycodone (morphine equivalent) doses per capita in Panel C, and hydrocodone (morphine equivalent) doses per capita in Panel D. Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used in Panels A–B. The control states are the 34 that did not implement must-access policies until 2016h2. Florida is dropped (see Appendix Section C). Observations are weighted by state population. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Since there is only one treated state in each regression, caution in the interpretation of confidence intervals is needed. The controls are identical to those in Figure 3.

Figure A20: Effects of Pill Mill Laws Among States without Must-Access PDMPs



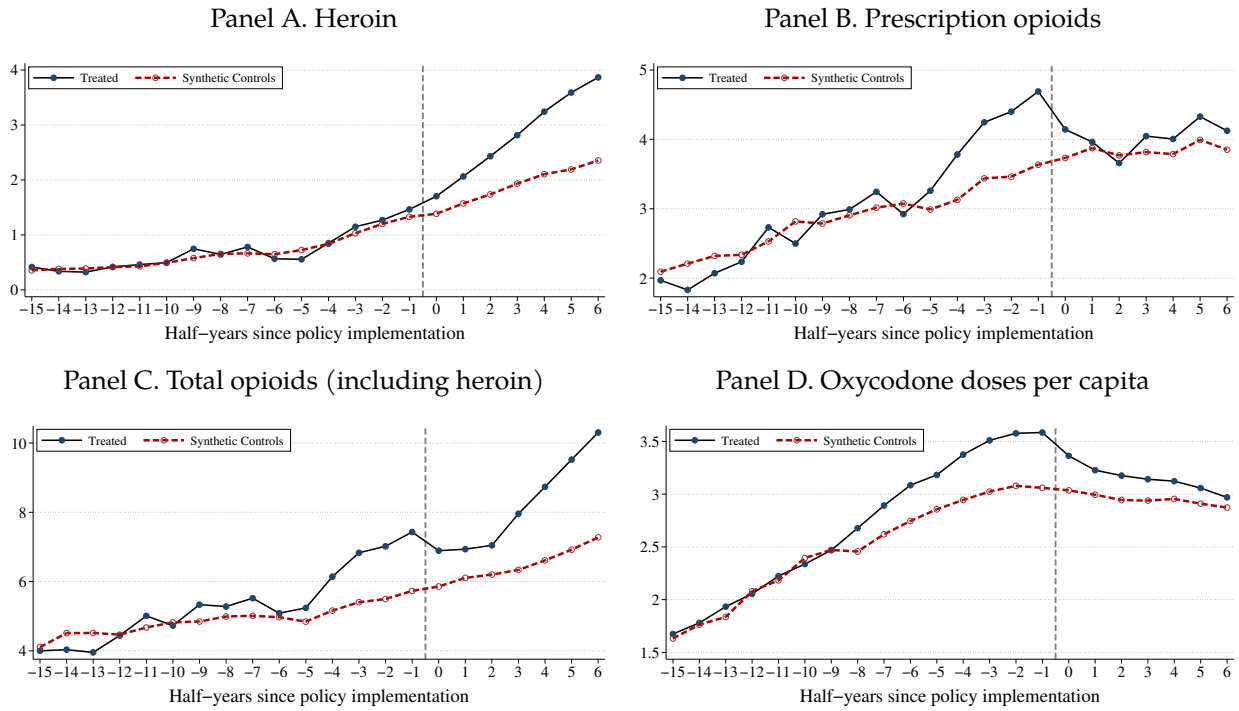
Notes: The figure shows an independent effect of pain clinic laws among the 35 states that did not implement a must-access PDMP until 2016h2. Among these 35 states, Florida, Mississippi, and Texas implemented pain clinic regulations between 2009h1 and 2011h2. In the regressions, I control for a full set of indicators for pre- and post-periods around the enactment of pill mill laws, the full set of state- and time-varying covariates that I use in the baseline analysis, the ARCOS measure of OxyContin misuse interacted with the time fixed effects, an indicator for whether the state had a voluntary-access PDMP, and the fixed effects for state and half-year. The figure displays the coefficients on the indicators for pre- and post-periods around the enactment of pill mill laws. The last pre-period is omitted. For each period, I present the point estimate and its 95 percent confidence interval that is calculated using standard errors clustered at the state level. Observations are weighted by state population. The treated sample is balanced in relative periods from -12 to +6. The distant relative periods outside the -12/+6 event time window are trimmed. The control states are balanced from 2003h1 to 2016h2. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in Panel A, illegal opioid death rate (T40.1, T40.4) in Panel B, prescription opioid death rate (T40.2) in Panel C, total opioid-related death rate (T40.1–T40.4) in Panel D, oxycodone (morphine equivalent) doses per capita in Panel E, and hydrocodone (morphine equivalent) doses per capita in Panel F. In Panels A–D, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used.

Figure A21: Outcome Gap Between the Treated and Synthetic Control Groups



Notes: The figure displays how the (unweighted) average of the outcome gaps between the treated and synthetic control groups changes over time. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in Panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita in Panel D. In Panels A–C, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treated states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the ten synthetic controls (see Appendix Table A5). Both the treated and synthetic control samples are balanced in relative periods from -15 to +6. See Appendix Section F.3 for a detailed description.

Figure A22: Synthetic Control Analysis—Differential Trends



Notes: The figure displays the trends in the outcomes separately for the treated and synthetic control groups. The solid black line presents how the (unweighted) average outcomes change over time in the treated states, and the dashed red blue line displays the trends for the control group. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in Panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita in Panel D. In Panels A–C, Ruhm-corrected numbers of deaths calculated using data from the National Vital Statistics System (NVSS) are used. In all panels, the treated states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the ten synthetic controls (see Appendix Table A5). Both the treated and synthetic control samples are balanced in relative periods from -15 to +6. See Appendix Section F.3 for a detailed description.

Table A1: Effects of Must-Access PDMPs on Opioid Overdose Deaths—Summary Effect

	Overdose Deaths per 100,000					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Heroin deaths and illegal opioid deaths per 100,000						
	Heroin (T40.1)			Heroin and Synthetic Opioids (T40.1, T40.4)		
Average effect	1.17**	1.00***	0.60***	1.97***	1.76***	0.99**
	(0.44)	(0.32)	(0.21)	(0.70)	(0.59)	(0.41)
R^2	0.771	0.814	0.862	0.716	0.754	0.827
Mean of dependent variable	1.151			1.884		
Panel B. Prescription opioid deaths and total opioid-related deaths per 100,000						
	Prescription Opioids (T40.2)			Total Opioids (including heroin) (T40.1–T40.4)		
Average effect	0.58***	0.44**	0.28	2.13***	1.84***	1.04**
	(0.21)	(0.17)	(0.20)	(0.69)	(0.59)	(0.42)
R^2	0.811	0.842	0.852	0.756	0.797	0.849
Mean of dependent variable	1.971			4.186		
Panel C. Illegal-opioid-only deaths and prescription-opioid-only deaths per 100,000						
	Illegal-Opioid-Only (T40.1, T40.4 but not T40.2 or T40.3)			Prescription-Opioid-Only (T40.2 but not T40.1, T40.3, or T40.4)		
Average effect	1.61**	1.44***	0.80**	0.27*	0.17	0.13
	(0.61)	(0.52)	(0.36)	(0.15)	(0.12)	(0.14)
R^2	0.710	0.749	0.820	0.815	0.843	0.846
Mean of dependent variable	1.5119			1.5123		
Ruhm (2018) correction	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X	X
Time-varying covariates		X	X		X	X
Pill mill laws		X	X		X	X
OxyContin reformulation			X			X
Number of treatment states	16	16	16	16	16	16
Number of control states	34	34	34	34	34	34
Observations	1,400	1,400	1,400	1,400	1,400	1,400

Notes: The table reports the estimated coefficients obtained when I replace a full set of indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) with a single indicator for the entire post-period. In each column, I include different sets of controls. Controls are identical to those in columns 1, 2, and 4 of Table 3. I use the full sample of the balanced panel of state-half-year from 2003h1 to 2016h2, and Florida is dropped (see Appendix Section C). The treatment states are the 16 that implemented must-access PDMPs until 2016h2. The control states are the 34 that did not implement must-access policies until 2016h2, excluding Florida. The dependent variable is heroin deaths per 100,000 (drug code T40.1) in columns 1–3 of Panel A, combined deaths from heroin and synthetic opioids per 100,000 (T40.1, T40.4) in columns 4–6 of Panel A, prescription opioid deaths per 100,000 (T40.2) in columns 1–3 of Panel B, total deaths from any opioid, including heroin, per 100,000 (T40.1–T40.4) in columns 4–6 of Panel B, illegal-opioid-only deaths per 100,000 (which involved T40.1 or T40.4 but not T40.2 or T40.3 at the time of death) in columns 1–3 of Panel C, and prescription-opioid-only deaths per 100,000 (T40.2 but not T40.1, T40.3, or T40.4) in columns 4–6 of Panel C. Ruhm-corrected mortality rates are used in all regressions. Observations are weighted by state population. Fixed effects for states and half-years are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A2: Robustness of Heroin Estimates—Voluntary-Access PDMPs

	Heroin Deaths per 100,000 (T40.1)				
	(1)	(2)	(3)	(4)	(5)
	Baseline	Add voluntary-access PDMPs			
		Enactment date			User access
		Horwitz	PDAPS	NAMSDL	Horwitz
1-year effect (β_2)	0.42*	0.40*	0.41*	0.42*	0.40*
	(0.21)	(0.21)	(0.21)	(0.21)	(0.22)
2-year effect (β_4)	0.90***	0.89***	0.90***	0.90***	0.88***
	(0.29)	(0.29)	(0.29)	(0.29)	(0.30)
3-year effect (β_6)	1.13**	1.11**	1.12**	1.13**	1.10**
	(0.46)	(0.45)	(0.46)	(0.46)	(0.46)
Voluntary-access PDMPs		-0.19*	-0.18*	-0.07	-0.17*
		(0.11)	(0.10)	(0.09)	(0.09)
Ruhm (2018) Correction	X	X	X	X	X
State fixed effects	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X
Time-varying covariates	X	X	X	X	X
OxyContin reformulation	X	X	X	X	X
Voluntary-access PDMPs		X	X	X	X
Number of treatment states	10	10	10	10	10
Number of control states	34	34	34	34	34
Observations	1,172	1,172	1,172	1,172	1,172
Mean of dependent variable	1.098	1.098	1.098	1.098	1.098
R^2	0.845	0.846	0.846	0.845	0.847

Notes: The table shows the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6) from the baseline specification (equation 1). Although each regression includes a full set of indicators for the pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. In all columns, the dependent variable is Ruhm-corrected numbers of heroin deaths per 100,000 (drug code T40.1), which are calculated using data from the National Vital Statistics System (NVSS). In column 1, I repeat my baseline estimates from column 4 of Table 3 Panel A. In all columns, I control for fixed effects for states and half-years, the ARCOS measure of pre-reformulation OxyContin misuse interacted with the time fixed effects, and the time-varying covariates, which are identical to those in column 4 of Table 3. In columns 2–5, I additionally control for voluntary-access PDMPs. Each column uses start dates of voluntary-PDMPs from a separate source: columns 2–4 use the enactment dates suggested by Horwitz et al. (2018), the PDAPS, and the NAMSDL, respectively; column 5 uses the dates PDMP data became accessible to any authorized user, suggested by Horwitz et al. (2018). Observations are weighted by state population. The treatment states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 34 that did not implement must-access policies until 2016h2. Florida is (see Appendix Section C). In all columns, the sample and controls are identical to those in column 4 of Table 3. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A3: Robustness of Heroin Estimates to Removing a Single State

	Heroin Deaths per 100,000 (T40.1)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	Drop DE	Drop KY	Drop MA	Drop NM	Drop NY	Drop OH	Drop OK	Drop TN	Drop VT	Drop WV
1-year effect (β_2)	0.42*	0.42*	0.40*	0.37	0.48**	0.60***	0.25	0.47**	0.44*	0.41*	0.36*
	(0.21)	(0.22)	(0.23)	(0.24)	(0.22)	(0.22)	(0.18)	(0.23)	(0.24)	(0.21)	(0.21)
2-year effect (β_4)	0.90***	0.90***	0.91***	0.88***	0.91***	1.01***	0.61***	0.99***	1.00***	0.91***	0.85***
	(0.29)	(0.30)	(0.32)	(0.32)	(0.30)	(0.35)	(0.21)	(0.30)	(0.30)	(0.30)	(0.30)
3-year effect (β_6)	1.13**	1.12**	1.07**	1.08**	1.16**	1.43***	0.71**	1.26**	1.21**	1.13**	1.08**
	(0.46)	(0.46)	(0.49)	(0.51)	(0.47)	(0.47)	(0.35)	(0.48)	(0.50)	(0.46)	(0.47)
Ruhm (2018) correction	X	X	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X	X	X
Half-year fixed effects	X	X	X	X	X	X	X	X	X	X	X
Time-varying covariates	X	X	X	X	X	X	X	X	X	X	X
OxyContin reformulation	X	X	X	X	X	X	X	X	X	X	X
Number of treatment states	10	9	9	9	9	9	9	9	9	9	9
Number of control states	34	34	34	34	34	34	34	34	34	34	34
Observations	1,172	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150
Dep var mean	1.098	1.097	1.091	1.085	1.092	1.080	1.060	1.109	1.106	1.098	1.096
R^2	0.845	0.845	0.843	0.840	0.845	0.841	0.831	0.845	0.850	0.845	0.845

Notes: The table shows the sensitivity of the baseline estimates for heroin mortality to removing a single treatment state. The table reports the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6) from the baseline specification (equation 1). Although each regression includes a full set of indicators for the pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. In all columns, the dependent variable is Ruhm-corrected numbers of heroin deaths per 100,000 (drug code T40.1), which are calculated using data from the National Vital Statistics System (NVSS). Observations are weighted by state population. In column 1, the treatment states are the ten that implemented must-access PDMPs from 2010h2 to 2013h2. In each of columns 2–11, I remove one of these ten states from the analysis sample. In all columns, the treated sample is balanced in relative periods from -15 to +6, and the distant relative periods outside the -15/+6 window are trimmed. In all columns, the control states are the 34 that did not implement must-access policies until 2016h2, and the baseline control sample is balanced from 2003h1 to 2016h2. Florida is dropped (see Appendix Section C). In all columns, the controls are identical to those in column 4 of Table 3. Fixed effects for state and half-year are always included. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels respectively.

Table A4: Robustness of Other Estimates

	Overdose Deaths per 100,000							
	(1) Baseline	(2) Reported mortality	(3) Voluntary PDMPs	(4) Include FL	(5) Marijuana laws	(6) Add NALs	(7) Good Sam laws	(8) Alternative dates
Panel A. Heroin and synthetic opioid deaths per 100,000 (T40.1, T40.4)								
1-year effect (β_2)	0.40	0.49	0.39	0.41	0.37	0.48	0.42	0.33
	(0.35)	(0.37)	(0.35)	(0.37)	(0.36)	(0.35)	(0.35)	(0.33)
2-year effect (β_4)	1.36***	1.56***	1.35***	1.35***	1.25***	1.45***	1.39***	1.29**
	(0.49)	(0.50)	(0.48)	(0.50)	(0.44)	(0.50)	(0.50)	(0.47)
3-year effect (β_6)	2.08**	2.34***	2.06**	1.98**	1.71*	2.08**	2.09**	1.64*
	(0.90)	(0.85)	(0.89)	(0.91)	(0.92)	(0.87)	(0.90)	(0.97)
Mean of dependent variable	1.799	1.423	1.799	1.7994	1.799	1.799	1.799	1.792
R^2	0.807	0.797	0.808	0.812	0.816	0.812	0.807	0.813
Panel B. Prescription opioid deaths per 100,000 (T40.2)								
1-year effect (β_2)	-0.54**	-0.52**	-0.54**	-0.48*	-0.55**	-0.52*	-0.55**	-0.57**
	(0.25)	(0.21)	(0.25)	(0.25)	(0.25)	(0.26)	(0.26)	(0.28)
2-year effect (β_4)	-0.27	-0.21	-0.28	-0.21	-0.29	-0.25	-0.28	-0.25
	(0.32)	(0.28)	(0.33)	(0.32)	(0.32)	(0.33)	(0.33)	(0.34)
3-year effect (β_6)	-0.27	-0.22	-0.27	-0.22	-0.25	-0.27	-0.27	-0.21
	(0.35)	(0.33)	(0.35)	(0.33)	(0.36)	(0.35)	(0.36)	(0.37)
Mean of dependent variable	1.984	1.533	1.984	2.039	1.984	1.984	1.984	2.016
R^2	0.861	0.843	0.862	0.860	0.863	0.862	0.861	0.867
Panel C. Total opioid-related deaths per 100,000 (T40.1–T40.4)								
1-year effect (β_2)	-0.17	-0.07	-0.18	-0.11	-0.21	-0.10	-0.16	-0.33
	(0.42)	(0.40)	(0.42)	(0.44)	(0.42)	(0.44)	(0.43)	(0.38)
2-year effect (β_4)	0.84*	1.06**	0.82*	0.90*	0.71*	0.91*	0.85*	0.71
	(0.46)	(0.46)	(0.46)	(0.47)	(0.41)	(0.48)	(0.48)	(0.43)
3-year effect (β_6)	1.61*	1.87**	1.60*	1.59*	1.25	1.62*	1.62*	1.16
	(0.89)	(0.84)	(0.89)	(0.88)	(0.87)	(0.87)	(0.89)	(0.90)
Mean of dependent variable	4.126	3.210	4.126	4.205	4.126	4.126	4.126	4.140
R^2	0.842	0.837	0.842	0.841	0.846	0.844	0.842	0.851
Ruhm (2018) correction	X		X	X	X	X	X	X
Number of treatment states	10	10	10	10	10	10	10	8
Number of control states	34	34	34	35	34	34	34	31
Observations	1,172	1,172	1,172	1,200	1,172	1,172	1,172	1,044

Notes: Notes: The table tests the robustness of my baseline heroin mortality estimates to alternative explanations. The table shows the 1-year effect (β_2), 2-year effect (β_4), and 3-year effect (β_6), obtained from the baseline specification (equation 1). Although each regression includes a full set of indicators for pre- and post-periods, I report the three coefficients above for brevity. The last pre-period is omitted. Observations are weighted by state population. In all columns in panel A, the dependent variable is combined deaths from heroin and synthetic opioids per 100,000 (drug codes T40.1, T40.4). In all columns in panel B, the dependent variable is prescription opioid deaths per 100,000 (drug codes T40.2–T40.3). In all columns in panel C, the dependent variable is total deaths from any opioid, including heroin (drug codes T40.1–T40.4). In column 1 of Panel A, I repeat my baseline estimates from column 4 of Table 3 Panel B. In column 1 of Panels B and C, I repeat my preferred estimates from column 4 of Table 5. In column 2, I use the raw reported numbers of deaths, and the other columns use the Ruhm-corrected numbers of deaths. Both the corrected and reported numbers of deaths are calculated using data from the National Vital Statistics System (NVSS). In column 3, I control for an indicator for whether a state had a voluntary-access PDMP. In column 4, I include Florida in the analysis sample. Florida is dropped from the control group in the other columns (see Appendix Section C). In columns 5–7, I include several other co-occurring opioid-related policies one by one: in column 5, I add time-varying indicators for whether medical marijuana dispensing is legal in the state and for whether recreational marijuana dispensing is legal in the state; in column 6, I add an indicator for whether the state had naloxone access laws (NALs), and in column 7, I add an indicator for whether the state had Good Samaritan overdose prevention laws. In column 8, I use alternative start dates of must-access PDMPs listed in the third column of Table 1, and in this estimation, the treated states are the 8 that implemented must-access PDMPs from 2010h2 to 2013h2, and the control states are the 31 that did not implement must-access policies until 2016h2, excluding Florida. In all columns, the distant event periods outside the -15/+6 window are trimmed. In all columns, I control for fixed effects for state and half-year, an indicator for whether a state had a pill mill law, the ARCOS measure of OxyContin misuse interacted with the half-year fixed effects, and the time-varying covariates that are identical to those in column 4 of Table 3. Standard errors clustered at the state level are in parentheses. ***, **, * denotes statistical significance at 1%, 5%, and 10% levels, respectively.

Table A5: Synthetic Control States

Treated State	Synthetic Control State					
Panel A. Heroin deaths per 100,000 (T40.1)						
Delaware	30.1% UT,	20.5% AZ,	17.7% ID,	10.9% DC,	7% KS,	13.9% Other
Kentucky	54% WI,	23.5% AK,	16.6% AZ,	4.9% DC,	1% MN,	
Massachusetts	48.6% AK,	19.6% DC,	13.6% NH,	8.9% MD,	4% ME,	5.2% Other
New Mexico	47.4% MO,	28.3% PA,	24.3% UT			
New York	17.7% WA,	16.3% NH,	14.1% NC,	13.7% WY,	13.5% MD,	24.7% Other
Ohio	65.9% MO,	23.8% UT,	6.1% MD,	4.1% DC		
Oklahoma	68.7% ND,	13.3% NH,	7.1% NE,	4.7% NC,	3.7% AZ,	2.5% Other
Tennessee	34.4% MS,	34.2% ND,	12.1% AZ,	7.5% AL,	5.5% KS,	6.3% Other
Vermont	54.1% MN,	31% AK,	8% IL,	6.9% WY		
West Virginia	24.5% KS,	20.6% MO,	19.8% MT,	18.6% HI,	11.3% OR,	5.2% UT
Panel B. Prescription opioid deaths per 100,000 (T40.2)						
Delaware	48.5% WY,	38.1% IN,	11.7% AK,	1.6% UT		
Kentucky	74.8% UT,	25.2% WY				
Massachusetts	79.2% TX,	14.7% ND,	4.6% DC,	1.5% CO		
New Mexico	97.6% UT,	2.4% AK				
New York	25.5% IA,	22.6% IL,	21.1% MD,	17.5% HI,	8.7% DC,	4.5% Other
Ohio	34.3% AZ,	29.1% NH,	16.5% WY,	15.1% HI,	4.9% AR,	0.1% SD
Oklahoma	55.7% UT,	28.6% AK,	15% WY,	0.7% NH		
Tennessee	35.9% WY,	31.9% UT,	27.1% AZ,	5.1% PA		
Vermont	35% ID,	23.2% UT,	19.1% ND,	12.3% DC,	6.1% TX,	4.4% WY
West Virginia	100% UT					
Panel C. Total opioid-related deaths per 100,000 (T40.1–T40.4)						
Delaware	48.8% MO,	22.1% WY,	12.2% CO,	10.1% AK,	5.3% AL,	1.4% NH
Kentucky	75.9% UT,	24.1% MO				
Massachusetts	61.1% IL,	19.1% CO,	8.1% HI,	6.4% UT,	2.3% KS,	3% Other
New Mexico	100% UT					
New York	29.7% IA,	22.6% KS,	16.9% DC,	13.6% MD,	12.9% HI,	4.5% Other
Ohio	89.6% AZ,	4.9% MO,	2.5% UT,	1.8% WY,	1.1% MD	
Oklahoma	57.8% UT,	26.4% AK,	15.8% NH,			
Tennessee	47.3% MO,	23.2% UT,	10.7% PA,	7.2% WI,	6.5% ME,	5% Other
Vermont	54.8% IL,	37.5% UT,	5.2% WY,	2.5% DC		
West Virginia	100% UT					
Panel D. Oxycodone doses per capita						
Delaware	100% AZ					
Kentucky	54.1% GA,	27.9% PA,	18% MD			
Massachusetts	23.8% AK,	19.6% UT,	13.3% MO,	12.9% NE,	11.2% WA,	19.2% Other
New Mexico	42.3% MD,	40.8% CO,	17% GA			
New York	66.7% GA,	26% IN,	7.3% NH			
Ohio	38.2% ME,	26.8% NH,	18.6% WA,	8.6% MO,	7.7% PA	
Oklahoma	27.2% UT,	25.6% AR,	21.3% MI,	12.1% OR,	9% AZ,	4.8% NH
Tennessee	50.2% ME,	43.4% AZ,	6.4% PA			
Vermont	29.2% WA,	20.3% MD,	16% NE,	12.4% MI,	10.5% NH,	11.6% Other
West Virginia	37.9% NH,	28.2% MD,	20.9% PA,	13% ME		

Notes: This table shows how synthetic control states included in the sample from Appendix Figures A21 and A22 are constructed. Each synthetic control state is calculated as a linear combination of the subset of my 34 control states (Florida is excluded from the control sample; see Appendix Section C). Values are independently rounded, and if there are more than six synthetic states, remaining states are combined into an "other" category. The outcome is heroin deaths per 100,000 (drug code T40.1) in Panel A, prescription opioid death rate (T40.2) in Panel B, total opioid-related death rate (T40.1–T40.4) in Panel C, oxycodone (morphine equivalent) doses per capita in Panel D.

B Comparisons with the Prior Literature

In this section, I compare my study with the prior literature on two dimensions—data period and model specification. Because Meinhofer (2018) is most closely related to my paper, I focus on comparing my study with hers. For simplicity, Meinhofer (2018) is often referred to as Meinhofer in this section. In Appendix Figure A14, I show how my heroin mortality results change if I drop recent data or use the approach suggested by Meinhofer with my analysis sample. Similarly, in Appendix Figure A15, I show how my replication of Meinhofer's event study results for heroin mortality change if I use my model specification instead of hers, while keeping everything else unchanged. Overall, my exercises suggest two things. First, using a longer period, which allows for including additional post-periods and several more implementations of must-access PDMPs, is key to identifying the overall spillover effects of must-access PDMPs. Second, the heroin estimates from my model specification provide stronger evidence of the spillover effect of must-access PDMPs compared with those from Meinhofer's event study specifications. My model specification better accounts for the pre-period differences in heroin mortality between the treated and control groups, and my heroin estimates are more robust to alternative specifications. These differences generated by the specification choices are more pronounced when I use a longer period.

B.1 Data Period

In Appendix Figure A14, I show the consequences of employing more recent data. Each column of Appendix Figure A14 corresponds to different sample periods. In the left column (Panels A, C, and E), I use my full sample (2003h1–2016h2), and in the right column (Panels B, D, and F), I drop the 2014h1–2016h2 period and reconstruct the sample so that the treated states are balanced in relative periods. The sample from the right column includes the five treated states that are consistently observed from nine half-years prior to implementation and three half-years after implementation. The number of treated states observed in each event time period is presented in the parentheses below that period.[1]

The first row (Panels A and B) shows how my baseline estimates for heroin mortality (Panel A) change if I drop the recent data period. The two panels display the coefficients on the indicator

[1]While my full sample in the left column excludes Nevada, which implemented the earliest must-access PDMP in the nation, to include more pre-periods, the sample from the right column does not exclude Nevada because it has five treated states only and the estimates are not robust to excluding one of them. The difference in the number of pre-periods across the two samples is attributable to the fact that Nevada has nine pre-periods in my sample.

for pre- and post-periods from the baseline specification (equation 1) on the full sample (Panel A) and on the sample without the recent data (Panel B). As seen in Panel B, even if I drop the period after 2013, I still find suggestive evidence of the spillover effects on heroin mortality. However, compared with the estimates in Panel A, the estimates in Panel B have much larger effect sizes, and all the coefficients in Panel B become close to zero and insignificant if I drop Ohio, one of the five treated states. Overall, Panels A and B suggest that using a longer data period is crucial to obtaining robust estimates that reflect the overall spillover effects on heroin mortality.

B.2 Model Specification

In this paper, I provide causal evidence that must-access PDMPs have increased heroin mortality, and my estimates are robust to controlling for several other co-occurring state and national opioid-related policies, including the 2010 OxyContin reformulation. Above, I show that using more recent data is crucial to identifying these effects of must-access policies. However, not only a longer data period but also my model specification contributes to my findings. Because Meinhofer (2018) is most closely related to my paper, I focus on comparing my econometric model with hers.

Below, I present my baseline specification (equation 1, provided below for convenience as equation A1) and the event study specifications used in Meinhofer (equations M1 and M2). Equation M1 is Meinhofer's (2018) preferred event study specification. Note that the notations in equations M1 and M2 differ from those in the original equations presented in Meinhofer (2018) (I use my preferred notations for an easier comparison of specifications), although they are fundamentally the same. To distinguish between year in equations M1 and M2 and half-year in equation 1, I change the subscript indicating half-year from t to h, only in this section. The model specifications used in the two studies are as below.

Equation (1) from my paper:

$$y_{sh} = \alpha_s + \alpha_h + \sum_{k \neq -1} \beta_k \mathbf{1}(Policy_{sk}) + X_{sh}\delta + oxy_s \cdot \omega_h + \varepsilon_{sh}, \tag{A1}$$

Equation (4) from Meinhofer (2018):

$$ln(Y_{stq} + 1) = \alpha_s + \alpha_t + \alpha_q + \sum_{k \neq -1} \beta_k \mathbf{1}(Policy_{sk}) + \gamma ln(P_{stq}) + \theta_s \cdot t + \varepsilon_{stq}, \tag{M1}$$

Equation (3) from Meinhofer (2018):

$$ln(Y_{stq}+1) = \alpha_s + \alpha_t + \alpha_q + \sum_{k\neq -1} \beta_k \mathbf{1}(Policy_{sk}) + \gamma ln(P_{stq}) + \varepsilon_{stq}, \tag{M2}$$

where $ln(Y_{stq}+1)$ is the log of overdose deaths in state s over qth quarter of year t, and Meinhofer (2018) adds 1 to all outcomes to avoid losing observations with count zero. P_{stq} is state population, and in equation A1, $y_{sh} = Y_{sh}/P_{sh} * 100,000$ is overdose deaths per 100,000 in state s over half-year h. α_s are state fixed effects, and α_t, α_h, and α_q are fixed effects for year, half-year, and quarter (seasonality), respectively. $\mathbf{1}(Policy_{sk})$ is 1 if a given state enacted a must-access PDMP k periods ago, and $k \geq 0$ denotes a post-period. $oxy_s \cdot \omega_h$ in equation A1 indicates the measure of pre-reformulation OxyContin use interacted with the half-year fixed effects. $\theta_s \cdot t$ in equation M2 is state-specific (year-level) trends. ε_{stq} is the error term. Note that the regressions in my study are weighted by state population, while those in Meinhofer are not; to be consistent, throughout this section, the regressions estimating my specification (equation 1) are weighted by population, while the regressions using Meinhofer's (equations M1 and M2) are unweighted. Although not reported, I estimate similar policy effects of must-access PDMPs on heroin mortality if I use my specification without weights.

In Appendix Figure A14, each row estimates one of the three specifications: the top row (Panels A and B) estimates my baseline specification (equation 1), the middle (Panels C and D) estimates Meinhofer's preferred event study specification (equation M1), and the bottom (Panels E and F) estimates Meinhofer's alternative event study specification (equation M2). Panel C shows that my results from the full sample (Panel A) are substantially affected if I use Meinhofer's preferred specification, while everything else that includes the full sample remains unchanged. If I use the same specification as in Panel C and drop the period after 2013, I obtain the results shown in Panel D, which are expected to be similar to Meinhofer's heroin results (Meinhofer uses data through 2013). In fact, Panel D has a results pattern similar to my replication of Meinhofer (see Panel A of Figure A15), although balancedness, data frequency, and the time window length are different across the two figures (the replication of Meinhofer is explained in detail below). All the panels in Figure A14 use half-year frequency data, and thus Panels C–F employ equations M1 and M2, which are based on quarter frequency, by including half-year fixed effects instead of fixed effects for year and quarter (seasonality).

Compared with Panels A and B, which are based on my specification, Panels C and D show that

the estimated policy effects are statistically less significant, and there is evidence of a pre-trend prior to policy implementation. In particular, with the more recent data in Panel C, using equation M1 leads to a clear upward pre-trend in the entire pre-period. Finally, Panels E and F display the results obtained using equation M2, which drops state-specific time trends from equation M1. Panels E and F suggest that the estimates presented in Panels C and D, which are based on equation M1, are sensitive to dropping state-specific time trends. The sizes of the coefficients are much smaller in Panels E and F than in Panels C and D, and Panel F suggests no effect of the policy on heroin-related deaths. Compared with the estimates presented in Panels C–E, the heroin estimates based on my specification are more stable across regressions with different sets of controls, as seen in Table 3 Panel A.

B.3 Replication of Meinhofer (2018)

As discussed above, Figure A14 suggests that the estimates from my specification, as compared with those from Meinhofer's preferred specification, provide stronger evidence of the spillover effects on heroin mortality. However, one may have a concern about whether other factors drive these findings, such as legal coding, Ruhm (2018) correction, or data frequency. To address this concern, I perform an exercise similar to that in Appendix Figure A14 but using the replication of Meinhofer (2018). I first replicate Meinhofer's event study results for heroin-related deaths and then test how these estimates change if I use my specification (equation 1) instead, while keeping everything else unchanged.

Panel A of Appendix Figure A15 shows my replication of Meinhofer's event study results for heroin-related deaths, and Panel B presents the estimates obtained when I use my baseline specification (equation 1) instead, while everything else, including the analysis sample from Panel A, remains unchanged. Panel C shows the estimates I obtain when I use Meinhofer's alternative specification equation M2 instead. Although I cannot directly compare the estimates from my baseline model with those from Meinhofer's log-transformed model, the estimates from Panels A and B have similar trends in both the pre- and post-periods. However, my specification better accounts for the pre-period differences in herion mortality across the treated and control groups, and the estimates in Panel A are sensitive to dropping time trends, as seen in Panel C.

These findings are consistent with those from Figure A14.[2] In summary, Appendix Figures A14 and A15 suggest that the estimates from my specification (equation 1) provide stronger and more robust evidence of the spillover effect on heroin deaths than equation M1 and that the differences generated by the specification choices are more pronounced when I include additional years of data.

C Dropping Florida

In the 2000s, increasing numbers of pill mills caused a dramatic rise in the opioid supply in Florida. As a result, Florida was at the center of the nation's opioid epidemic in the late 2000s and was an extreme outlier both in levels of and trends in opioid supply (Meinhofer 2016). In response, Florida passed several laws in 2010 and 2011 that strictly regulated pain clinics. These aggressive regulations led to a huge drop in the opioid supply in Florida. Appendix Figure A17 displays the trends in each state's per capita legal supply of oxycodone. This figure shows that Florida is an outlier that experienced both a sharp increase and decrease in oxycodone supply within a decade. There was a large spike in Florida's per capita legal supply of oxycodone throughout the 2000s until the pill mill peak in 2010, and then the oxycodone supply began to decrease sharply as a result of aggressive regulations in 2010 and 2011.

I exclude Florida from my control group for all analyses in this study. The key assumption of my difference-in-differences model is that, in the absence of must-access PDMPs, the trends in the outcomes would have been the same across the treated and control groups. However, given that Florida experienced dramatic policy changes around the time of my treatment states' implementation of the must-access PDMPs, including Florida may potentially violate the parallel assumption. In fact, I find that the results for oxycodone doses per capita are sensitive to whether I include Florida in my control group or not, as shown in Appendix Figure A16. The first panel of the top row of Appendix Figure A16 presents the oxycodone estimates from my baseline specification (equation 1) obtained using a sample that includes my ten treatment states and all of the 35 states that did not implemented a must-access PDMP until 2016h2, including Florida. As illustrated in Appendix Figure A16, the estimates from this sample are sensitive to removing Florida. When I drop Florida, I observe a sudden decrease in oxycodone supply following policy

[2]Note that the sample used in Panels B and D of Figure A14 is balanced in relative (half-year) periods from -9 to +3, while the sample used in Figure A15 is unbalanced in relative (quarter) periods from -9 to +7 (the corresponding half-year event time window is -4.5/+3.5). Although the event time windows and the number of treated states included are different in the two figures, the results patterns in the overlapped relative periods are similar across the figures.

implementation, and a negative trend in oxycodone supply is found in the entire post-period. However, if I include Florida in my analysis, all coefficients for the post-periods are close to zero, as seen in Appendix Figure A16. Although not reported, after dropping Florida, my oxycodone results are robust to removing one of the treated or control states. In contrast to the sensitivity of my oxycodone results to including Florida, my mortality results are robust to whether Florida is included, as shown in Section 6 (see columns 1 and 4 of Table 6 and those of Appendix Table A4).

D Must-Access PDMP in Ohio

In this section, I explore the policy effect in Ohio in particular and propose three possible explanations for the strong effect of Ohio's must-access PDMP, which relied on provider suspicion. In Section 6, I test the robustness of the baseline heroin mortality estimates to removing one treated state (see Appendix Table A3, the corresponding regression coefficients are presented in Panel A of Appendix Figure A11). As shown in Appendix Table A3, regardless of which treated state is dropped, the estimates are statistically significant and qualitatively similar to the baseline estimates. However, when I drop Ohio, the magnitudes of the heroin mortality estimates are slightly attenuated, although the coefficients for the two- and three-year effects remain statistically significant (see column 7 of Appendix Table A3).

In this section, I first explore the effect of must-access policy within Ohio and show that Ohio's policy had stronger effects on my outcomes than the policies in the other treated states. I then propose three possible explanations for the strong impact of Ohio's initial must-access PDMP on the heroin death rate—a sharp increase in PDMP utilization, the existence of a complementary law, and high accessibility of heroin. As mentioned in Section 5.6, Ohio implemented its initial must-access PDMP in 2011h2 and then strengthened its must-access law in 2015h2 (at event time +8). However, the strengthened law cannot explain why my heroin estimates are affected when I remove Ohio because the most distant post-period in my analysis is event time +6 (3 years after implementation) and Ohio strengthened its law at event time +8 (4 years after the initial implementation). Therefore, in this section, I focus on discussing why Ohio's initial must-access PDMP, which relied on provider suspicion, had stronger effects on the outcomes than the must-access policies in the other treated states. Throughout this section, I use the phrase *Ohio's initial must-access PDMP* synonymously with *Ohio's must-access PDMP*. Also, note that Ohio has the second-largest population among my ten treated states, and my regressions are weighted by

state population. The strong impact of Ohio's (initial) must-access policy and Ohio's large population explain why the coefficients become smaller when I remove Ohio.

D.1 Effects of Must-Access PDMP within Ohio

I first investigate how the must-access PDMP impacted the mortality outcomes within Ohio. In Appendix Figure A18, I present the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except Ohio.[3] The estimates in blue show the impact of Ohio's must-access PDMP, and the point estimates in red are my baseline estimates, indicating the overall effects of the policy among the ten treated states, including Ohio. The vertical dashed gray line (between event times -1 and 0) indicates the implementation timing of a must-access PDMP. The vertical short-dashed gray line (between event times -3 and -2) indicates 2010h2, when the OxyContin reformulation was introduced.

Appendix Figure A18 suggests that Ohio's must-access PDMP had stronger impacts on most outcomes compared with the overall effects among the ten treated states. Following implementation of the must-access PDMP policy, heroin and illegal opioid mortality sharply increased (Panels A and B), and these increases coincided with sudden decreases in prescription opioid mortality (Panel C) and the legal supply of opioids (Panels E and F). Note that a sharp decrease in prescription opioid mortality at event time -5 is due to a temporary drop in Ohio's prescription opioid deaths in 2009h1. Overall, in Appendix Figure A18, I observe a clear and strong substitution pattern between legal and illegal opioid mortality following Ohio's policy implementation and I find no apparent pre-trends in the outcomes, evidence of that supports the parallel trends assumption. As mentioned above, Ohio has the second-largest population among my ten treated states, and my regressions are weighted by state population.[4] The strong impact of Ohio's must-access policy and Ohio's large population explain why my heroin mortality estimates become smaller when I remove Ohio. Below, I propose three possible explanations that may account for the strong effects of the must-access PDMP in Ohio.

[3] An alternative way to estimate the impact of Ohio's PDMP is to use the full sample and interact an indicator for Ohio with the full set of indicators for pre- and post-periods. However, the results I observe when limiting my treatment group to Ohio are similar to those I obtain when including the interactions.

[4] This is another reason why removing Ohio has a relatively larger effect on the estimates than dropping one of the other treated states. Although not reported in the revised paper, I find similar policy effects regardless of whether I weight observations by state population, and removing Ohio has a smaller effect on the estimates with the unweighted regressions.

D.2 PDMP Utilization

The first plausible explanation for the strong impact of Ohio's must-access PDMP is that Ohio's policy was associated with a dramatic increase in PDMP utilization. In 2011h2, Ohio enacted its initial must-access law, which required prescribers to review a patient's prescription history at the beginning of treatment and annually after that, if they had reason to believe that treatment with controlled substances in Schedules II–V would exceed 12 continuous weeks (Urahn 2016). Even though this initial must-access law primarily relied on provider suspicion, the utilization of the PDMP increased dramatically, from 911,000 reports requested in 2010, to 1.8 million in 2011, 5.4 million in 2012, 7.4 million in 2013, 10.8 million in 2014, and 16.5 million in 2015 (the 2016 Ohio Automated Rx Reporting System (OARRS) Annual Report[5]).

Other actions may also have increased PDMP participation in Ohio. In 2012h2, the state published guidelines for prescribing opioids in emergency departments, and in 2013h2 guidelines were established for the long-term prescription of opioids, both initiatives may have encouraged the utilization of the PDMP and reduced opioid prescriptions. For example, the Governor's Cabinet Opiate Action Team (2014) encouraged providers to "consider checking Ohio Automated Rx Reporting System (OARRS) for all patients who will receive an opiate," demonstrating that Ohio encouraged providers to use the PDMP in situations beyond those prompted by their suspicion.

In 2015h2, Ohio strengthened the must-access law by adding further requirements for the utilization of the PDMP. Based on the updated laws, prescribers must request a PDMP report on a patient under certain circumstances, even without provider suspicion. Following implementation of the updated mandate in 2015, PDMP utilization increased again, from 1.2 million queries in April to 1.4 million queries in September, reflecting a 17% increase (PDMP Center of Excellence 2016).

Transition from Ohio's initial must-access PDMP in 2011h2 to the updated program in 2015h2 occurred gradually, rather than a single implementation date marking a sudden increase in PDMP utilization (Urahn 2016). As a result of Ohio's consistent efforts, utilization of Ohio's PDMP system increased dramatically, and opioid prescriptions decreased sharply. Following the implementation of Ohio's initial must-access PDMP in 2011, the rate of individuals who see five or more prescribers and five or more pharmacies in a three month period to obtain controlled substances (commonly referred to as doctor shopping) decreased by over half, by the last quarter

[5] https://www.ohiopmp.gov/documents/Annual%20Report%20(2016).pdf (last accessed May 2020)

of 2013 (PDMP Center of Excellence 2014). According to the 2016 OARRS Annual Report, for the period 2012 to 2016, the total doses of opioids dispensed to Ohio patients decreased by 162 million doses (or 20.4%), while the number of opioid prescriptions issued to Ohio patients decreased by 2.5 million (or 20%); during that same period, the state experienced a 78.2% decrease in the number of individuals who see multiple prescribers to obtain controlled substances illicitly. Ohio is one of the potential models for states looking to mandate PDMP use (PDMP Center of Excellence 2016). The dramatic increase in PDMP utilization, which resulted from implementing the initial must-access PDMP, publishing guidelines on opioid prescriptions, and a sharp decrease in opioid prescriptions as a result of increasing PDMP utilization, can explain why Ohio's mandate had dramatic effects on heroin and other opioid mortality even if it relied on provider suspicion.

D.3 Complementary Law—Pill Mill Law

Another initiative that may account for the strong effects of the must-access PDMP in Ohio is the enactment of a complementary law. Around the time of policy implementation of the must-access PDMP, three states (Kentucky, Ohio, and Tennessee) also enacted pill mill laws, which impose strict regulations on pain clinics to prevent them from issuing opioid prescriptions without medical indication. Buchmueller and Carey (2018) view pain clinic laws as complements to must-access laws, as they target a slightly different channel of misuse than PDMP policies. Must-access laws target a large fraction of providers, while pain clinic laws directly regulate the behavior of the small share of providers, who prescribe high volumes of opioids without medical indication. Ohio and Kentucky, which implemented the must-access PDMP one or two half-years after implementing the pill mill law, experienced large decreases in opioid prescriptions following the mandate (PDMP Center of Excellence 2016). This is consistent with my findings from Appendix Figure A19. Appendix Figure A19 presents the effect of the must-access PDMP within each state by plotting the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except one. The vertical dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The vertical short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. Appendix Figure A19 suggests that for most outcomes, must-access PDMPs had larger impacts in Ohio and Kentucky, compared with the policies in other treated states. The pill mill law, considered a complementary law to must-access policies, may have contributed to the strong

impact of Ohio's must-access PDMP.

However, this also raises a concern that pill mill laws may not be complements to must-access laws but a confounder that drives variation in my mortality outcomes. To address this concern, I test whether pain clinic laws have an independent effect when passed in the absence of a must-access law, following the approach used by Buchmueller and Carey (2018). If pill mill laws alone have little or no impact on my outcomes, they are not likely to drive my results. In Appendix Figure A20, I test for an independent effect of pain clinic laws using data on the 35 states, that did not implement a must-access PDMP until 2016h2. Among these 35 states, Florida, Mississippi, and Texas implemented pain clinic regulations between 2009h1 and 2011h2. I consider the event time window -12/+6, during which these three states are consistently observed. The distant relative periods outside this event time window are trimmed. For this test, I control for a full set of indicators for pre- and post-periods around the enactment of pill mill laws, the full set of state- and time-varying covariates that I use in the baseline analysis, the ARCOS measure of OxyContin use interacted with the time fixed effects, an indicator for whether the state had a voluntary-access PDMP, and the fixed effects for state and half-year. Appendix Figure A20 suggests that, in the absence of a must-access law, pill mill laws in these three states had no effect on all the outcomes except oxycodone supply. The negative effects on oxycodone supply observed in Panel E are primarily driven by Florida, which experienced a dramatic change in its oxycodone supply (see Appendix Section C); I find no effect of pill mill laws on oxycodone supply if I drop Florida. I conclude that this policy is not likely to affect mortality outcomes when providers are not also required to access the PDMP database. My findings are consistent with Brighthaupt et al. (2019), who find pill mill laws had no effect on prescription opioid, heroin, or synthetic opioid overdose deaths in Ohio and Tennessee. It is possible that a pill mill law alone has no substantial effects, but it may work to strengthen the effects of the must-access PDMP if implemented together.

D.4 Accessibility of Heroin

Finally, high accessibility of heroin in Ohio is also likely to contribute to the strong association between must-access PDMP and heroin mortality. The nation's major heroin routes, I-70 and I-75, pass through Ohio, allowing users easy access to heroin and illegal fentanyl. It is not surprising that a supply-side drug policy has a stronger spillover effects in the area where people can easily find affordable substitutes.

E Additional Heterogeneity Analysis

In Appendix Figure A19, I investigate the effect of the must-access PDMP within each treated state by plotting the estimates from the baseline specification (equation 1) that I obtain by dropping all the treated states except one. The (vertical) dashed gray line indicates the implementation timing of a must-access PDMP (between event times -1 and 0). The (vertical) short-dashed red line indicates 2010h2, when the OxyContin reformulation was introduced. Although the estimates are noisy because the treated sample includes one state only, the results presented in Appendix Figure A19 allows for a better understanding of the differential effects of must-access policies across states. Since there is only one treated state in each regression, caution in the interpretation of confidence intervals is needed.

F Additional Robustness Analysis

F.1 Consequences of Excluding Six Treated States

To estimate the policies' medium-run effects, I focus on the treated states that were consistently observed during an event time window that runs from -15 to +6. As a result, among the 16 states that implemented a must-access PDMP during my sample period, the six states that were not observed at some point during the -15/+6 window are excluded.[6] To address the consequences of excluding these six states, I test how my estimates change when I include more treated states in the analysis sample. Note that I construct the sample so that the treated states are balanced in relative periods, so the event time window decreases as the number of treated states included increases. Appendix Figure A4 presents the coefficients obtained when I estimate the baseline specification (equation 1) on three different samples. The dashed red line presents my baseline estimates, obtained using the sample that includes the ten treated states that are consistently observed during the -15/+6 event time window. The short-dashed blue line corresponds to the 12 treated states that are consistently observed during the -15/+4 event time window, and the black solid line corresponds to the 15 treated states that are consistently observed during the -15/+2

[6] My sample period is from 2003h1 to 2016h2, and in this period, the 16 states implemented must-access PDMPs from 2007h2 to 2016h2.

window.[7]

In Appendix Figure A4, the lines connecting the estimates from each of these three samples closely track one another for all outcomes. Given that the estimated short-term effects are similar across the three samples, I prefer to use the sample that allows me to look at the longer-term effects, which is my analysis sample. By looking at the longer-term effects, I can better understand how heroin mortality and other mortality outcomes change over time and compare how the longer-run impacts differ from the short-run impacts. In Appendix Table A1, I also report the summary effect of must-access laws among all the 16 treatment states, which I obtain by replacing the full set of indicators for pre- and post-periods with a single indicator for the entire post-period.

F.2 Analysis with the Post-Reformulation Data Period

Appendix Figure A12 presents the estimates from my baseline specification (equation 1) obtained when I drop the pre-reformulation time period and reconstruct the sample so that the treated sample is balanced in related periods. I consider an event time window that runs from -2 to +6, during which the nine treated states were consistently observed. The distant periods outside the -2/+6 are trimmed. As presented in Appendix Figure A12, I see that estimates are very similar to my baseline estimates. In addition, the consequences of controlling for the reformulation are similar to those observed in my baseline analysis: although not reported, if I drop the controls for the reformulation (the interaction of the measure of pre-reformulation OxyContin use and the time fixed effects) from the regressions, the estimated effects on heroin mortality and illegal opioid mortality become larger, which is consistent with my findings from the baseline analysis (see Figure 4). In summary, Appendix Figure A12 suggests that my estimates are robust to dropping the pre-reformulation period and that accounting for the reformulation is important for obtaining more accurate estimates.

[7] Although there are the 16 states that implemented must-access PDMPs until 2016h2, the sample with the -15/+2 window has 15 states. This is because Nevada, which implemented the must-access PDMP for the first time in the nation, only has the nine pre-periods that are observed during my sample period. Although not reported, including Nevada in my analysis sample by limiting the number of pre-periods to nine does not change my main results. I prefer to include more pre-periods by dropping Nevada because I can observe the trends in heroin mortality for a longer period while not affecting the estimates significantly.

F.3 Synthetic Control Analysis

As described in Section 5, I find that must-access PDMPs have increased the heroin death rate and that this increase coincided with a sudden decrease in prescription opioid mortality. These findings implicitly assume that any pre-treatment differences between the groups can be explained by my econometric model (equation 1). However, a concern that some of the unaccounted for pre-period differences between the two groups may be responsible for my results motivates me to conduct a synthetic control analysis as a robustness analysis. I construct a comparable synthetic control state for each treated state based on pre-period data in such a way that the synthetic control state outcome trends are similar to those of the treated state prior to policy implementation. If the baseline results are comparable to those from the synthetic control analysis, my results are not likely to be driven by unaccounted for pre-treatment differences between the groups.

For each of my ten treated states, I construct a synthetic control state from the 34 control states that never implemented a must-access policy,[8] matching on the value of the outcome variable in each of the 15 pre-treatment periods.[9,10] Each synthetic control state is composed of a weighted average of observations from the subset of the 34 control states. A set of synthetic controls are constructed for each of the following outcomes: heroin mortality, prescription opioid mortality, total opioid-related mortality, and oxycodone doses per capita. Table A5 shows the makeup of the synthetic states for each outcome.

Using observations from the treated and synthetic control groups, I create a sample so that the treated and synthetic control samples are strongly balanced in relative periods, from -15 to +6. Using this sample, I calculate the outcome gap between each treated state and its synthetic control state for each event time. Appendix Figure A21 plots how the (unweighted) average of these gaps changes over time. In addition, Appendix Figure A22 depicts the outcome trends for the treated and synthetic control groups separately. The solid black line reflects how the (unweighted) average outcomes change over time in the treated states, and the dashed red line reflects the trends for the control group.

As shown in Appendix Figures A21 and A22, my synthetic control analysis suggests a larger

[8] Florida is excluded in the analysis (see Appendix Section C).

[9] A synthetic control analysis has been more widely conducted for a single treated unit or multiple units with the same treatment timing, but recent papers extend this method for the case of multiple units with differential timing of treatment (e.g., Kleven (2019); Acemoglu et al. (2016)).

[10] I use the Stata command `synth` to construct synthetic controls. See https://fmwww.bc.edu/RePEc/bocode/s/synth.html for a description of the `synth` command.

policy effect size, but the results pattern is very similar to that observed in my main analysis (see Figure 3). Although I still observe upward pre-trends in prescription opioid mortality and oxycodone consumption,[11] the sudden decreases in these outcomes in the first post-period provide suggestive evidence for the substitution.

F.4 Alternative Measures of Pre-Reformulation OxyContin Use

Appendix Figure A13 displays the coefficients on the indicators for pre- and post-periods from the baseline difference-in-differences specification (equation 1) obtained when I use two alternative measures of pre-reformulation OxyContin use (separate regressions): Panel A uses oxycodone / hydrocodone in (morphine equivalent) doses per capita, and Panel B uses the Google Trend measure suggested by Beheshti (2019). My heroin mortality results are robust to using each of these alternative measures.

[11] The reason for this is that a few treated states experienced a sharp increase in prescription opioid mortality and in the legal supply of oxycodone in the pre-period.

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