How Does Opioid Prevalence Affect Surgery Decisions?

David Beheshti* Seth Neller†

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Abstract

This paper studies how the prevalence of opioids affects joint physician-patient decisions over medical procedures. Following Alpert et al. (2022), we utilize variation in opioid exposure due to state policies that affected OxyContin's marketing and market entry. Our results suggest that higher availability of opioids led to a substantial (21%) increase in the number of elective surgical discharges, such as knee replacements, hip replacements, and back surgeries. We also consider effects for non-elective surgical discharges—procedures where we expect a much smaller response to the availability of opioids—and find a statistically insignificant increase of 1%. Finally, we investigate medical discharges—procedures where no response is expected—and find no detectable effect. This increase in elective procedures is consistent with a model of physician behavior that incorporates patient pain and post-surgical well-being into surgical decisions and where decreases in the "hassle" of prescribing pain-reducing medication pushes marginal patients to undergo surgeries that they might not otherwise elect. Our results highlight an important tradeoff: while liberal opioid prescribing has led to widespread misuse and abuse, the availability of opioids may allow some patients to undergo quality-of-life improving surgeries that would otherwise be too painful.

Keywords: Opioids, Surgeries, Triplicate Laws, Physician Behavior

JEL Codes: I10, I11

*University of Texas at San Antonio; david.beheshti@utsa.edu

†University of Tennesse at Knoxville; seth.neller@gmail.com

1 Introduction

The proliferation of prescription opioids in the 1990s and 2000s had disastrous consequences in the United States. Between 1990 and 2019, more than 250 thousand individuals died from a prescription opioid overdose.[1] Beyond mortality, the opioid crisis has had far-reaching societal effects, contributing to declines in labor force participation (Powell, 2021), increased crime rates (Maclean et al., 2022), and reductions in child welfare (Buckles et al., 2023; Meinhofer and Angleró-Díaz, 2019).

Despite the well-documented harms of the opioid crisis, opioids remain highly effective for managing acute and post-surgical pain. This presents an important tradeoff: while prescription opioids have led to widespread misuse and dependence, they also enable physicians to perform quality-of-life enhancing surgeries that might otherwise be considered too painful for patients. In this paper, we consider how access to opioids influences physicians' decisions regarding surgical procedures. Our model suggests that if physicians incorporate patients' welfare into their decision-making, increased access to opioids should encourage them to perform more marginal surgeries—procedures that, in the absence of effective post-surgical pain management, would not be worthwhile.

To empirically test our model, we leverage a quasi-experimental design based on the introduction of OxyContin in 1996 and its differential marketing across states. Prior to 1996, some states had triplicate prescription laws, which required physicians to use special forms when prescribing certain controlled substances, including opioids. Purdue Pharma, the manufacturer of OxyContin, largely avoided marketing the drug in triplicate states, leading to significantly lower growth in opioid availability in these regions. This variation in exposure to OxyContin provides a natural experiment that allows us to estimate the causal impact of opioid availability on surgical decision-making.

Our findings indicate that increased opioid prevalence led to a rise in elective surgeries—procedures where physicians and patients have greater flexibility in deciding whether to operate—while having no effect on the number of non-elective surgeries or total medical discharges. This suggests that physicians responded to greater opioid availability by expanding the scope of surgeries they were willing to perform, particularly for cases where pain management was a key concern. Importantly, we find no evidence that increased opioid access changed the volume of non-elective surgeries or medical discharges, suggesting that the observed effects are driven by shifts in decision-making surrounding marginal surgeries rather

[1] Authors calculation using CDC WONDER Online Database available here: https://wonder.cdc.gov/. Prescription opioid deaths are those with ICD-10 codes T40.2 (Other opioids) or T40.3 (Methadone).

than broader changes in patient or physician behavior.

Our study contributes to the literature on the economic and health effects of opioid availability by providing new evidence on how opioids influence medical decision-making. While much of the existing research has focused on the negative externalities of the opioid crisis—such as addiction, labor market declines, and social costs—our findings highlight an underexplored channel through which opioid access affects healthcare provision. Our study also adds to the literature on determinants of physician behavior and how altruism affects treatment decisions (Chandra et al., 2011). From a policy perspective, our results suggest that opioid restrictions may have unintended consequences for surgical care, potentially reducing access to procedures for patients who would benefit from them.

2 Model

In this section, we outline a model of physician decision making in order to illustrate how changes in opioid prevalence can affect surgery decisions.[2] The key insight of the model is that, if physicians take their patients' pain into account and opioids reduce pain, then easier access to opioids can lead to marginal patients undergoing additional surgeries. However, patients far from the margin are not affected by opioid availability.

Consider a physician who chooses treatment intensity x to maximize his or her utility. Overall utility U is a function of utility benefits derived from income $m(x)$, costs of effort $c(x)$, and net benefit to the patient. Let $b(x)$ represent the benefit to the patient and $\theta \cdot x$ denote physical pain of the patient.[3] We assume that both $m(x)$ and $b(x)$ are increasing and concave, so that marginal increases in treatment intensity lead to increases in both physician income and patient benefits, although at a declining rates. We assume that $c(x)$ is increasing and convex. The physicians optimization problem is therefore to maximize

$$U(x) = m(x) - c(x) + \alpha[b(x) - \theta \cdot x], \tag{1}$$

where α indicates the level of the physician's altruism, i.e., how much the weight the physician puts on the patient's utility when making treatment decisions. Following Iizuka (2007), we assume that $\alpha > 0$.

[2]We model this problem from the perspective of the physician. However, with a few slight modifications one could replace the patient as the decision maker, or consider a joint patient-physician decision making process. Our goal, however, is to use the simplest framework that demonstrates the underlying mechanisms by which changes in opioid prevalence affect surgery decisions.

[3]In principle, this term could include any costs to the patient such as monetary costs, inconvenience, or physical pain, among others

The first order condition yields

$$m'(x) + \alpha b'(x) = \alpha \cdot \theta + c'(x)$$

which simply equates the physicians direct marginal benefit in terms of income plus indirect utility experienced on behalf of the patient to the physicians direct costs of effort and indirect costs experienced on behalf of the patient. We define $\Gamma(x)$ to be a function such that

$$\Gamma(x) = \frac{\partial U}{\partial x} = 0 \tag{2}$$

Totally differentiating 2 to uncover the effect of pain θ on treatment intensity x yields

$$\frac{dx}{d\theta} = -\frac{\frac{\partial \Gamma(x)}{\partial \theta}}{\frac{\partial \Gamma(x)}{\partial x}} = -\frac{-\alpha}{m''(x) + \alpha b''(x) - c''(x)}$$

The assumptions that $\alpha > 0$, $m''(x) < 0$, $b''(x) < 0$, and $c''(x) > 0$ then imply that $\frac{dx}{d\theta} < 0$. In words, optimal treatment intensity is decreasing in pain. If opioids reduce pain, then this model predicts that increased opioid availability would lead to increases in treatment intensity. This could include surgeries replacing more conservative treatment options, for example. Figure 1 illustrates this positive correlation in the raw data.

3 Data

Data on state-year medical and surgical discharges from 1992 to 2015 come from the Dartmouth Atlas of Healthcare.[4] Derived from Medicare claims, these data provide rates of medical and surgical discharges per 1,000 beneficiaries. Surgical discharges are classified as elective (e.g., back, hip, or knee surgeries) or non-elective, covering six urgent procedures (e.g., coronary bypass, aortic aneurysm repair).

We also use opioid prescription rates and shipments from the CDC and the DEA's ARCOS. CDC data, derived from IQVIA Xponent, cover about 94% of US retail prescriptions from 2006 to 2015. DEA ARCOS reports hydrocodone and oxycodone shipments (2000–2015). We supplement these with total and opioid-specific overdose death rates from CDC WONDER (1992–2015).

Following Alpert et al. (2022), we categorize states with triplicate laws before OxyCon-

[4] Available here: `https://data.dartmouthatlas.org/surgical-discharges/` and `https://data.dartmouthatlas.org/medical-discharges/`.

tin's release (see Figure A1).

Table 1 presents summary statistics by triplicate status and before/after OxyContin's 1996 introduction. Columns (1)–(2) cover triplicate states; (3)–(4) cover nontriplicate states. The first and third columns are for 1992–1995, and the second and fourth for 1996–2015. Consistent with Alpert et al. (2022), total and opioid-specific drug death rates rose faster in nontriplicate states.

4 Empirical Strategy

The main hurdle to estimating the causal effect of opioid availability on surgical decions is that opioids are commonly prescribed to patients after surgery. As a result, regressing the rate of surgical discharges on the rate of opioid prescriptions suffers from a classic reverse causality problem. In order to address this concern, we exploit plausibly exogenous variation in opioid prevalence based on pre-existing state laws which altered the extent to which states were exposed to the introduction of OxyContin. This identification strategy was pioneered by Alpert et al. (2022), who showed that states that were more exposed to the introduction of OxyContin experienced substantially more drug overdose deaths in the following decades.

Intuitively, our estimation strategy compares states that did not have triplicate laws prior to the release of OxyContin in 1996–and therefore had more exposure to opioid marketing– to states that had triplicate laws prior to 1996, before and after the release of OxyContin. Formally, we consider models of the form:

$$y_{st} = \alpha_s + \gamma_t + \sum_{k \neq 1995} \beta_k \cdot \mathbb{1}(triplicate_s) \cdot \mathbb{1}(t = k) + \epsilon_{st} \tag{3}$$

where y_{st} is the outcome in state s and year t, α_s are state fixed effects, and γ_t are year fixed effects. The state fixed effects control for any time-invariant differences in across states, such as underlying differences in patient health that are not changing over time. The year fixed effects control for any secular trends common to all states. The β_k's are the coefficients of interest, and represent the difference in outcomes between triplicate and non-triplicate states in each period k, conditional on the included fixed effects relative to 1995. We calculate confidence intervals using a clustered wild bootstrap at the state level to account for the small number of treated clusters.

The key identifying assumption of this model is that, absent the introduction on OxyContin in 1996, outcomes in states with triplicate laws would have trended similarly to states without triplicate laws. We provide supporting evidence of this assumption by plotting the β_k coefficients in each year. These coefficients reveal similar trends in triplicate and non-triplicate states prior to the introduction of OxyContin, which is consistent with our identifying assumption.

In order to summarize our event study regressions, we also consider difference-in-differences models of the form

$$\begin{aligned} y_{st} = \alpha_s + \gamma_t + &\delta_{early} \cdot \mathbb{1}(1996 \leq t \leq 2009) \cdot \mathbb{1}(triplicate_s) + \\ &\delta_{late} \cdot \mathbb{1}(2010 \leq t \leq 2015) \cdot \mathbb{1}(triplicate_s) + \epsilon_{st} \end{aligned} \tag{4}$$

where all terms are defined as in equation 3, but rather than interacting each individual year with the triplicate indicator, we break our sample into three periods: (1) pre-period (1992-1995), (2) early (1996-2009), and (3) late (2010-2015).[5] We then use the δ_{late} coefficient as a summary measure of the long-run impact of the introduction of OxyContin.

5 Results

First Stage The plausibility of our results hinges on the claim that the introduction of OxyContin led to higher opioid availability in nontriplicate states relative to triplicate states. Evidence supporting this claim has been carefully documented in prior literature (Alpert et al., 2022; Buckles et al., 2023; Powell, 2021). Rather than replicating this literature in detail, we present one piece of particularly transparent evidence in Figure 2. This figure shows annual shipments of oxycodone (the active ingredient in OxyContin) and hydrocodone, the two most commonly prescribed opioids, separately for triplicate and nontriplicate states. This figure clearly shows the relative growth in oxycodone in nontriplicate states relative to triplicate states, while there is almost no corresponding difference for hydrocodone. This supports the claim that opioids were more prevalent in nontriplicate states. The lack of a notable difference for hydrocodone is indicative that the higher overall opioid consumption in nontriplicate states was driven by the introduction of OxyContin.

Main Results We present our key event study in Figure 3. This figure plots the coefficients from equation 3 as blue circles along with the 95-percent confidence intervals as a shaded region. The outcome variable is the rate of elective surgical discharges per 1,000 Medicare

[5] We allow for three periods rather than a simple before and after in order to account for the dynamics that we observe in our event studies. The "late" period was chosen based on the date when prescribing differences between triplicate and nontriplicate states stabilized, as shown in Figure 2.

beneficiaries. Prior to the introduction of OxyContin in 1996, the coefficients are small in magnitude and not significantly different from zero. This indicates that elective surgical discharge rates were on similar trajectories in triplicate and nontriplicate states, lending plausibility to the parallel trends assumption. In each year following the introduction of OxyContin, the coefficients are negative and generally grow in magnitude. By 2007, each coefficient is statistically significant at either the 5 or 10 percent level. Our estimate of δ_{late}, the long-run effect of the introduction of OxyContin, is -1.88 and is statistically significant at the 1 percent level. This indicates that in the years 2010 to 2015, triplicate states had 1.88 fewer elective surgeries per 1,000 Medicare beneficiaries than nontriplicate states, relative to 1995. This is an approximately 21 percent decline relative to the pre-period mean.[6]

We summarize these results, along with those for non-elective surgeries and non-surgical discharges in Table 2. Odd-numbered columns present the baseline estimates from equation 4, while even-numbered columns add controls for demographics, education, disability, socioeconomic variables, and urbanicity. Because of their non-discretionary nature, we expect much smaller, if any responses for non-elective surgeries. Likewise, we expect to see no responses for medical discharges. In both cases, the coefficients are small in magnitude (about 1.0 and 1.7 percent of their respective means) and statistically insignificant. The fact that we only see effects for elective surgeries supports the conclusion of our model, that opioid prevalence only affects decisions surrounding marginal decisions.

We break down elective surgeries into specific types in Table 3. Our coefficients indicate relative long-term increases in hip replacements, knee replacements, and back surgeries of between 8.4 and 24.1 percent, depending on the specification, in nontriplicate relative to triplicate states.

6 Discussion

This paper investigates how the proliferation of opioids affected physician-decision making over elective surgical procedures. We find that states more exposed to aggressive marketing of opioid painkillers following the introduction of OxyContin saw increases in elective surgical discharges, but no changes in non-elective or medical discharges. While the negative consequences of the opioid crisis have been well-documented, this paper highlights one potential positive effect of more lenient opioid prescribing: increases in elective surgeries which have generally been found to increase quality of life (e.g., Konopka et al., 2018; Tosteson et al.,

[6]In Appendix Figure A3, we find almost identical coefficients using a synthetic difference-in-differences estimator (Arkhangelsky et al., 2021).

2008; Ethgen et al., 2004). This finding highlights the importance of policymakers considering positive aspects of appropriate pain-management when considering further restrictions on opioid painkillers.

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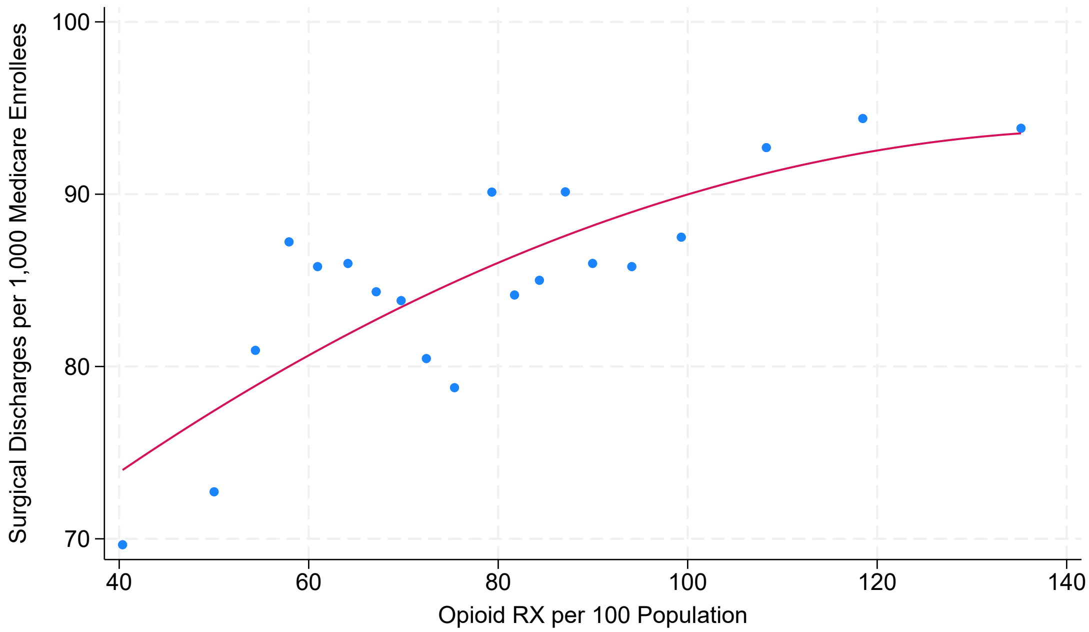
Tables and Figures

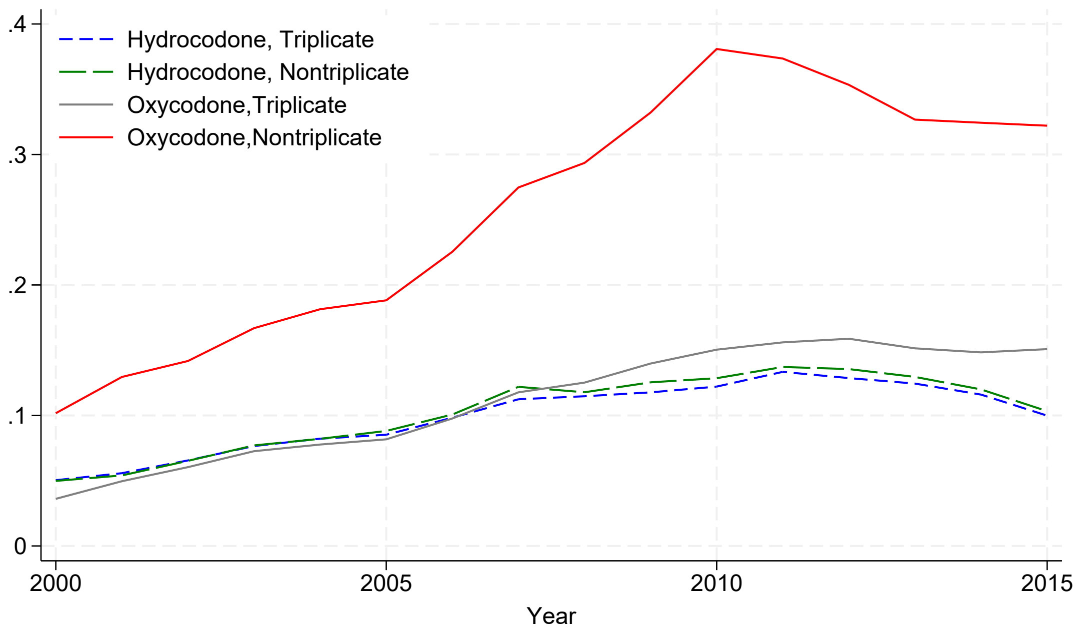
Figure 1: Binned Scatter Plot of Opioid Prescription Rate Against Surgical Discharge Rate



Note: This figure shows a binned scatter plot of the rate of surgical discharges per 1,000 Medicare beneficiaries against the opioid prescription rate per 100 population. This figure pools data from the years 2006-2015, which is the period when both series are available. Each circle represents the average within one of twenty ventiles of the opioid prescription rate distribution. The red curve is the line of best fit from a quadratic regression.

Source: Author calculations using data from the Dartmouth Atlas of Health Care and the CDC.

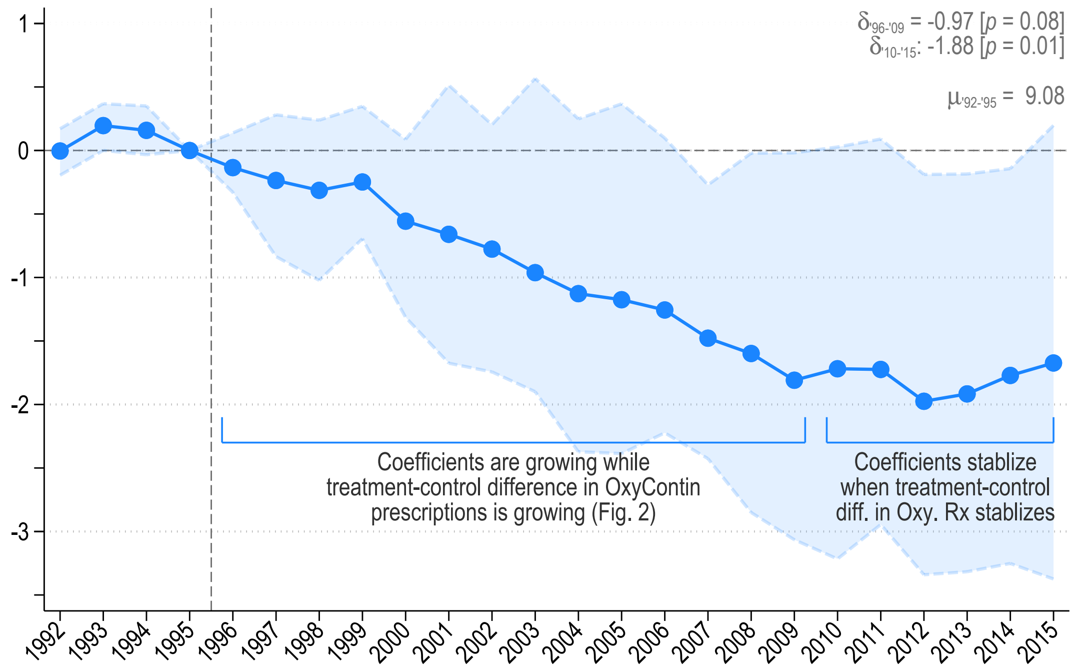
Figure 2: Oxycodone and Hydrocodone Shipments Over Time by Triplicate Status



Note: This is a recreation of Figure III, Panel (C) in Alpert et al. (2022). The figure shows the annual distribution of oxycodone and hydrocodone, reported in morphine equivalent grams per capita, separately by triplicate status.

Source: Author calculations using data from ARCOS.

Figure 3: Event Study Difference-in-Differences Coefficients: Elective Surgeries



Note: This figure plots the β_k coefficients from equation 3. The shaded region represents the 95 percent confidence intervals for each coefficient, computed using a clustered wild bootstrap at the state level. We display the summary coefficients from equation 4, along with the corresponding p-values, in the top right corner. We also display the mean pre-period mean of the outcome variable.

Source: Author calculations using data from the Dartmouth Atlas of Health Care.

Table 1: Summary Statistics

	Triplicate		Nontriplicate	
	Pre	Post	Pre	Post
	(1)	(2)	(3)	(4)
Discharges				
Medical	209.767	221.993	222.405	228.924
Total Surgery	92.863	90.093	94.616	94.640
Non-Elective Surgery	16.345	12.696	17.120	13.732
Elective Surgery (Total)	8.540	13.329	9.321	15.354
(Back)	2.407	3.696	2.638	4.260
(Hip)	2.139	3.070	2.248	3.469
(Knee)	3.995	6.563	4.435	7.625
Death Rates				
All Drug	5.818	9.583	4.060	11.730
Opioid	1.229	4.468	0.597	6.118
Drug				
Opioid RX Rate	.	57.831	.	86.097
Oxycodone Grams Per Capita	.	750.862	.	1,705.106
Hydrocodone Grams Per Capita	.	976.655	.	1,021.946
Other				
Population (in millions)	81.145	92.300	180.419	204.248
Observations	20	100	184	920

Note: This table shows means of key variables, except population, which is summed. Columns (1) and (2) include triplicate states, while columns (3) and (4) include nontriplicate states. Odd columns are restricted to pre-period years (1992-1995), while even columns are post-treatment years (1996-2015). Statistics are population weighted.

Source: Author calculations using data from the Dartmouth Atlas of Health Care, ARCOS, and the CDC.

Table 2: Difference-in-Differences Regression Results: Discretionary, Non-Discretionary, and Medical Discharges

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Triplicate ×	**Elective Surgery**		**Non-Elective Surgery**		**Non-Surgical**	
1996-2009						
Estimate (δ)	-0.97*	-0.83**	-0.30	-0.33	6.52	3.05
CI (Upper)	-1.81	-1.59	-1.49	-1.57	-5.34	-6.73
CI (Lower)	0.07	-0.06	0.58	0.55	17.20	14.37
p-value	0.08	0.03	0.80	0.71	0.26	0.49
Est. as % of Mean	-10.7%	-9.1%	-1.8%	-2.0%	3.0%	1.4%
2010-2015						
Estimate (δ)	-1.88***	-1.53***	-0.17	-0.19	3.80	-0.89
CI (Upper)	-3.24	-2.83	-0.93	-1.15	-8.34	-15.92
CI (Lower)	-0.37	-0.39	0.85	0.83	20.09	11.28
p-value	0.01	0.01	0.72	0.67	0.58	0.86
Est. as % of Mean	-20.8%	-16.8%	-1.0%	-1.1%	1.7%	-0.4%
Controls		X		X		X
Observations	1,224	1,224	1,224	1,224	1,224	1,224
Pretreatment Mean	9.079	9.079	16.880	16.880	218.479	218.479

Note: This table shows the regression results from equation 4. The outcome variables are the rates of elective surgeries, non-elective surgeries, and non-surgical discharges per 100 Medicare beneficiaries. Confidence intervals are constructed using a clustered wild bootstrap at the state level. Even-numbered columns adds controls for those aged 65 and older within a state, with covariates for demographics, education, disability, socioeconomic variables, and urbanicity. Significance at 10%, 5%, and 1% levels is represented by *, **, and ***, respectively.

Source: Author calculations using data from the Dartmouth Atlas of Health Care and the 1992-2015 Annual Social and Economic Supplements of the Consumer Population Survey (Flood et al., 2024).

Table 3: Difference-in-Differences Regression Results: Discretionary Surgery Breakdown

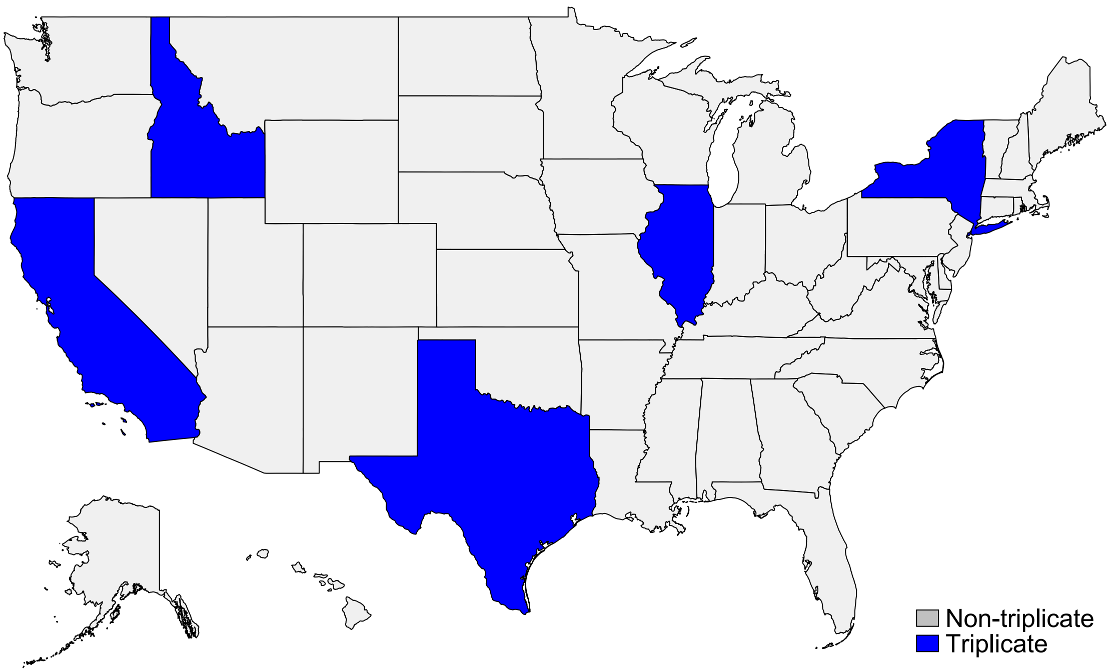
	(1)	(2)	(3)	(4)	(5)	(6)
Triplicate ×	**Knee Replacement**		**Hip Replacement**		**Back Surgery**	
1996-2009						
Estimate (δ)	-0.44	-0.40*	-0.21	-0.19	-0.32	-0.24
CI (Upper)	-0.83	-0.75	-0.46	-0.40	-0.72	-0.60
CI (Lower)	0.28	0.11	0.22	0.16	0.51	0.53
p-value	0.22	0.09	0.30	0.32	0.30	0.50
Est. as % of Mean	-10.3%	-9.2%	-9.4%	-8.5%	-12.3%	-9.4%
2010-2015						
Estimate (δ)	-1.04*	-0.89**	-0.48	-0.43	-0.37	-0.21
CI (Upper)	-1.56	-1.45	-0.93	-0.84	-0.95	-0.80
CI (Lower)	0.32	-0.01	0.11	0.10	0.67	0.73
p-value	0.09	0.04	0.20	0.14	0.40	0.54
Est. as % of Mean	-24.1%	-20.6%	-21.5%	-19.4%	-14.4%	-8.4%
Controls		X		X		X
Observations	1,224	1,224	1,224	1,224	1,224	1,224
Pretreatment Mean	4.30	4.30	2.21	2.21	2.57	2.57

Note: This table shows the regression results from equation 4 for each type of elective surgery. The outcome variables are the rates of surgical discharges per 100 Medicare beneficiaries for knee replacements, hip replacements, and back surgeries. Confidence intervals are constructed using a clustered wild bootstrap at the state level. Even-numbered columns adds controls for those aged 65 and older within a state, with covariates for demographics, education, disability, socioeconomic variables, and urbanicity. Significance at 10%, 5%, and 1% levels is represented by *, **, and ***, respectively.

Source: Author calculations using data from the Dartmouth Atlas of Health Care and the 1992-2015 Annual Social and Economic Supplements of the Consumer Population Survey (Flood et al., 2024).

A Online Appendix

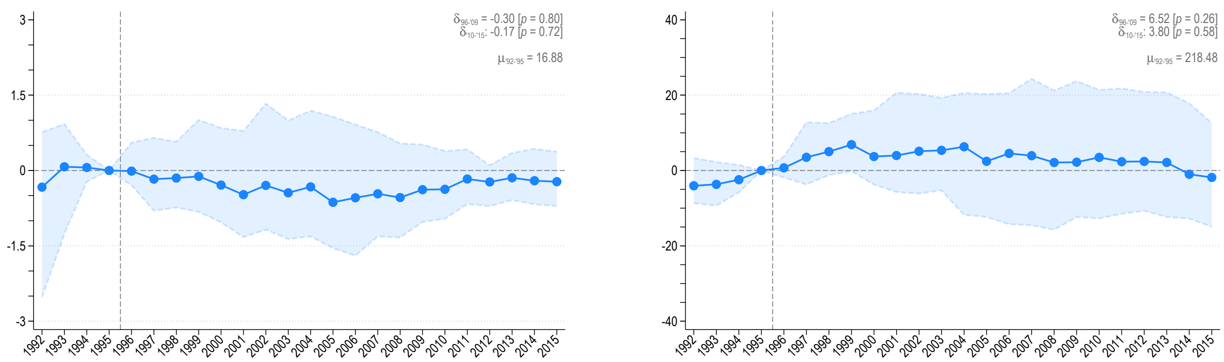
Figure A1: Map of Triplicate and Non-Triplicate States



Note: This figure shows a map of the United States. States with triplicate laws in place as of 1996 are shaded in blue, while nontriplicate states are shaded light gray.

Source: Alpert et al. (2022)

Figure A2: Event Study Difference-in-Differences Coefficients: Non-Elective Surgeries and Medical Discharges



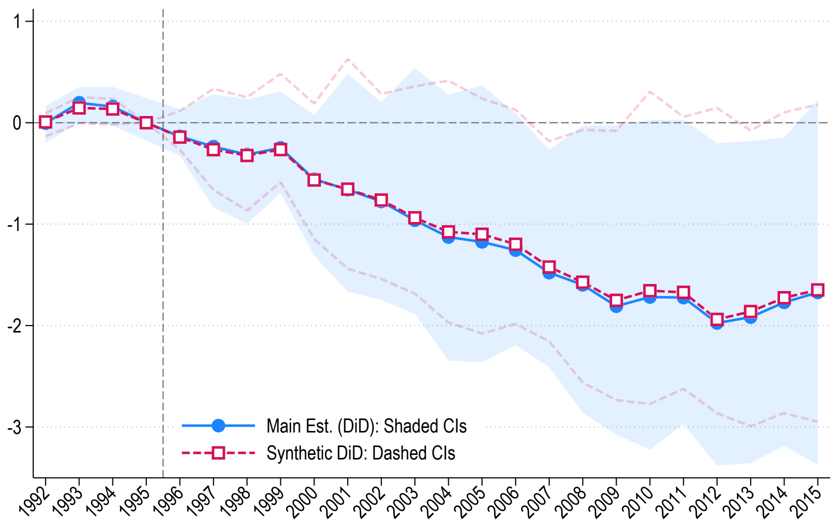
(a) Non-Elective Surgeries

(b) Non-Surgical Discharges

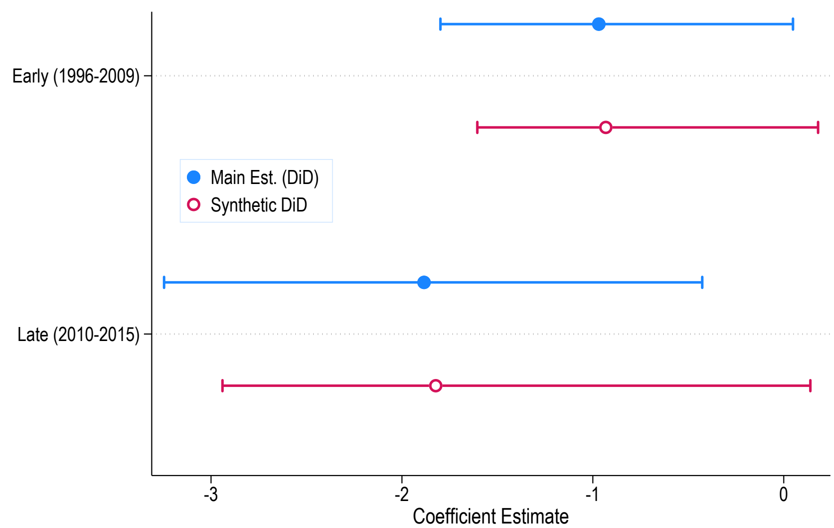
Note: This figure plots the β_k coefficients from equation 3 for non-elective surgeries and non-surgical discharges in panels (a) and (b), respectively. The shaded regions represent the 95 percent confidence intervals for each coefficient, computed using a clustered wild bootstrap at the state level. We display the summary coefficients from equation 4, along with the corresponding p-values, in the top right corner of each panel. We also display the mean pre-period mean of the outcome variable.

Source: Author calculations using data from the Dartmouth Atlas of Health Care.

Figure A3: Synthetic Difference-in-Differences Event Study Coefficients: Elective Surgeries



(a) Event Study



(b) Summary Coefficients

Note: This figure plots compares our primary results from Figure 3 ("Main Est.") to those utilizing an alternative methodology, synthetic difference-in-differences ("Synthetic DiD") (Arkhangelsky et al., 2021). The top panel displays our original event study from Figure 3 (blue dots with shaded confidence intervals), with the annual Synthetic DiD estimates (the corollaries to the β_k coefficients from equation 3) denoted by red squares, with bootstrapped confidence intervals displayed by dashed lines. Panel B displays performs a similar exercise, comparing the δ_{early} and δ_{late} coefficients from Equation 4 with their Synthetic DiD analogs.

Source: Author calculations using data from the Dartmouth Atlas of Health Care.