An Intervention to Reduce Opioid Over-Prescription

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1 Introduction

Between 2000 and 2014, drug overdoses involving opioids rose 200%, fueling widespread concern about an opioid epidemic and spurring calls for changes in public policy (Chen et al., 2014; Dart et al., 2015; Rudd et al., 2016). A distinguishing feature of the current epidemic of drug abuse is that many overdoses and deaths can be attributed to legal opioids that were prescribed by a physician. The clinical use of opioids in the United States has quadrupled since 1999, contributing to the rise in drug overdoses, emergency room visits, and admissions for drug treatment. Despite significant efforts to restrict the prescribing of opioids over the past decade, prescription opioid abuse and drug overdoses due to prescription opioids have continued to rise (Health and Human Services, 2014; Meara et al., 2016).

Recent evidence suggests that doctors play a key role in the opioid epidemic. While prescription drug monitoring programs (PDMPs)—prescription databases that allow physicians to check for signs of opioid abuse before prescribing—have little effect on average (Paulozzi et al., 2011; Reifler et al., 2012; Haegerich et al., 2014; Meara et al., 2016), research shows that PDMPs might be more effective in states that require physicians to consult these databases (Dowell et al., 2016; Buchmueller and Carey, 2017; Dave et al., 2017). Furthermore, among patients treated in the same emergency room, Barnett et al. (2017) demonstrate that those who happen to be treated by a physician with a higher propensity to prescribe opioids are more likely to be dependent on opioids 12 months later. Despite being the gatekeepers of the legal opioid supply, very little is known about why some physicians are more likely to prescribe opioids than others or about what role physician training can play in bringing the epidemic under control.

2 Study design

We implemented an information dissemination program in partnership with the Centers for Medicare and Medicaid Services (CMS), which included the $N = 3,000$ largest hospitals in the contiguous 48 states in the experiment.

Intervention. On one day in March 2016, CMS sent teams to treated hospitals for a day-long presentation about opioid over-prescription. The presentation included information on rising mortality due to prescription painkillers, safer alternatives, and testimonial from people who had last family members to addiction. Breakfast and lunch were provided and the ultimate cost of administering the treatment was $1,254 per hospital on average (1st Quartile: $859; 3rd Quartile: $1,592).

Randomization and covariate balance. We studied 3,000 hospitals, 1,249 of which were assigned to treatment. Randomization was stratified by geography (urban, suburban, or rural) and local income levels (hospital zip code income above or below national median), creating six blocks total. In urban and suburban hospitals, 50 percent were assigned to treatment and control. Due to higher travel costs, only 25 percent of rural hospitals were assigned to treatment.

Table 1 presents hospital-level summary statistics by treatment assignment including p-values of the differences. We show differences for age, the average age of patients treated the hospital; heart attacks, the share of patients treated for heart attacks; zip income, median household income in the hospital's zip code; and physicians, the number of staff physicians at the hospital. Differences across treatment groups are small in magnitude, suggesting that the randomization was effective at creating balance between the groups.

Compliance. CMS teams recorded attendance for all sessions using physicians' security badges. We define full attendance (or compliance) as having at least 30 percent of eligible physicians present for the event. We repeated the analysis using several different thresholds for full attendance but this yielded the largest causal effect; this suggests that 30 percent is the most robust measure for our context. By this measure, we achieve 60 percent compliance at the treatment hospitals, or substantially less than full compliance. We discuss this issue below.

Outcomes. Later in 2016, CMS solicited data on prescriptions given out by both treatment and control hospitals covering the two months after the presentation date. CMS then used internal methods to classify opioid usage during those two months as high- or low-intensity, a measure that we use as our outcome variable. On average, CMS designated about half of hospitals as high-intensity. This fact helps assuage power concerns in our study since the standard error of a Bernoulli outcome, $\sqrt{p(1-p)}$, is minimized when p is 0.50.

Attrition. Under current regulations, hospitals are no longer required to report this data to CMS. (While CMS maintains historical data on hospital-level opioid usage going back several years, this information was not useful for either planning or analysis since that data are about the hospitals prior to the experiment.) Despite this, follow-up rates were very high, roughly 90 percent for both groups. The similar attrition rate in both samples suggests that there was no differential attrition across treatment and control; for completeness we include a table in the appendix relating covariates to attrition within the two groups.

3 Estimation strategy

Intention-to-treat. The impact of the program can be evaluated by comparing outcomes across treatment groups in a simple regression framework. For each hospital-level outcome, the estimating equation is

$$Y_i = \alpha + \beta_1 T_i + \epsilon_i \tag{1}$$

where i indexes hospitals, Y_i is a dummy variable indicating high (1) or low (0) opioid prescription intensity, T_i is a dummy variable the treatment assignment, and ϵ_i is the error,

Table 1: Baseline characteristics of Treatment and Control Groups

	Treatment	Control	p-value
Age	37.10	36.82	0.33
	(7.07)	(6.75)	
Percent heart attacks	0.51	0.42	0.01
	(0.49)	(0.40)	
Zip Income	89,570	90,326	0.56
	(19,894)	(19,533)	
Physicians	60.4	61.0	0.62
	(29.6)	(30.5)	
N	1,249	1,751	-

Standard deviations in parentheses.

which is assumed to be independent across hospitals. We estimate this using OLS. The estimate $\widehat{\beta}_1$ gives the intention-to-treat impact, or the difference in mean outcomes based on randomization alone.

Treatment on the Treated. Denote D_i as the binary indicator for compliance, which we defined above. we next turn to the effect of treatment on the treated (TOT). As mentioned above, our study suffers from one-sided non-compliance. To account for this, we estimate the following specification, known as "per protocol" analysis:

$$Y_i = \alpha + \beta_2 D_+ \epsilon_i. \tag{2}$$

This differs from Equation 1 in two important ways. First, we substitute the dummy D_i for T_i. Second, we omit from the analysis all hospitals assigned to treatment who are observed not to comply with treatment (i.e., all hospitals with $T_i = 1$ and $D_i = 0$). Note that all hospitals from the control group are included. This approach means that we do not need to use an Instrumental Variables analysis, which requires more stringent assumptions. The estimate $\widehat{\beta}_2$ gives the treatment on treated impact, or the impact of actually receiving the intervention.

Covariate adjustment. In both cases, we first present a simple comparison of means. Next, include certain covariates known to be correlated with hospital-level opioid prescriptions, namely the average age of patients and the number of physicians. Because we block, we do not need to control block membership in the regression.

4 Results

We show the results in Tables 2 and 3. Table 2 shows the intention-to-treat estimates, while Table 3 shows to treatment on the treated estimates. Column (1) of each table gives a simple bivariate regression without covariates, thus giving the simple difference in means between treatment and control. In column (2), we control for the average age of patients in the hospital and the number of staff physicians.

Table 2: Intention to Treat

	(1)	(2)
treatment	-0.0610***	-0.0614***
	(0.0185)	(0.0183)
age		0.0104***
		(0.0013)
physicians		-0.0003
		(0.0003)
Constant	0.5077***	0.1434**
	(0.0119)	(0.0527)
Observations	2,700	2,700

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The TOT estimates are considerably larger than the ITT estimates. This is expected since the TOT gives the effect on units in the treatment group who received the treatment. Focusing on Table 3, column 1, the treatment effect suggests that hospitals assigned to treatment with sufficient attendance decreased their likelihood of being high-intensity prescribers by 10 percent, or by 5 percentage points assuming a baseline rate of 50 percent.

Table 3: Treatment on Treated

	(1)	(2)
treatment	-0.1002***	-0.1046***
	(0.0217)	(0.0215)
age		0.0101***
		(0.0014)
physicians		-0.0001
		(0.0003)
Constant	0.4997***	0.1351*
	(0.0119)	(0.0568)
Observations	2,252	2,252

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.1 Subgroup analysis

In this section, we report the ITT and TOT for hospital subgroups. We partition on hospital size, which is known to be an important determinant of patient outcomes. We proxy for size with the number of staff physicians, separating hospitals into deciles based on this number.

The results, shown in Table 4, are striking. For both ITT and TOT, the causal estimates appear to be focused in the third decile of hospital size (i.e., column 3). This is unsurprising, since small- to mid-level hospitals are known to have greater difficulties with addiction prevention as well as physicians who are more invested in the patient population.

5 Conclusion

Based on our analysis, the program caused significant decreases in opioid prescriptions. Focusing on the effect of treatment on the treated, we find a 6 percentage point decrease in the likelihood that a hospital is a high-intensity prescriber. Since, according to CMS estimates, a high-prescribing hospital incurs an extra $4,000 in medical costs due to overuse and addiction, our results suggest that the program benefits clearly outweigh the costs.

Table 4: Subgroup analysis

	Hospital Decile									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intention to Treat										
Treatment indicator	-0.1036	-0.0869	-0.1226*	0.0028	0.0629	-0.1133	-0.0594	-0.0670	-0.0798	-0.1020
	(0.0579)	(0.0580)	(0.0584)	(0.0589)	(0.0586)	(0.0584)	(0.0594)	(0.0578)	(0.0583)	(0.0591)
Observations	300	300	300	300	300	300	300	300	300	300
Treatment on the treated										
Treatment indicator	-0.1162	-0.0660	-0.1486*	0.0137	0.0066	-0.1279	-0.0779	-0.0903	-0.0998	-0.1071
	(0.0673)	(0.0670)	(0.0685)	(0.0684)	(0.0711)	(0.0690)	(0.0693)	(0.0673)	(0.0696)	(0.0717)
Observations	251	250	250	256	244	251	257	248	246	249

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

References

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A 1. Regression of attrition on covariates

	(1) Treatment	(2) Control
Suburban	0.0654^{*}	0.0105
	(0.0312)	(0.0305)
Rural	0.0036	0.0113
	(0.0383)	(0.0279)
age	0.0053^{**}	-0.0082^{***}
	(0.0020)	(0.0016)
physicians	-0.0004	0.0018^{***}
	(0.0005)	(0.0004)
Observations	1249	1751

Standard errors in parentheses

The omitted geography category is Urban

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$