Essays on Prescription Drug Policy and Education as Determinants of Health

By

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Dissertation

Submitted to the Faculty of the

Graduate School of Vanderbilt University

in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Economics

August 10, 2018

Nashville, Tennessee

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To all the many teachers from whom I have been fortunate to learn,

chief among them my parents.

ACKNOWLEDGMENTS

I am grateful for the extensive guidance and insight I have received from my dissertation committee members Kathryn Anderson, Andrew Goodman-Bacon, Federico Gutierrez, and Peter Savelyev. I appreciate the selfless ways in which they have shared their sound advice and their earnest curiosity, which have guided me well in my research efforts. I also thank Christopher Carpenter, Michael Richards, Peter Rousseau, Kosali Simon, and Alex Hollingsworth for generously lending their research and professional expertise along the way. I appreciate the hard work done by the faculty and staff of the Vanderbilt University Department of Economics, from which I have greatly benefited. I gratefully acknowledge financial support from Vanderbilt Economics in the form of a graduate fellowship, Kirk Dornbush Summer Research Grant, and Faculty-Graduate Student Collaborative Grant.

I wish to thank Jere Behrman for graciously facilitating the use of data from the Socieconomic Survey of Twins for Chapter 1 of this dissertation and for his insights into that project. I appreciate the extensive help of Robert Krueger, Matthew McGue, and Shandell Pahlen in facilitating the use of data from the Minnesota Twin Registry. I am especially grateful to Federico Gutierrez for his considerable help in conceiving and designing the analysis of schooling allocation in the family as included in Chapter 1.

I thank my colleagues Aaron Gamino and Lindsey Bullinger for dedicatedly working alongside me in my research. I likewise thank Peter Savelyev for his partnership, support, and valuable mentorship in our joint work. I thank my compatriots who have studied and worked alongside me as Ph.D. students at Vanderbilt for their cooperation and camaraderie.

I appreciate the steadfast support I have enjoyed from all my friends and family during the course of my graduate studies. They uplift me and they keep me grounded.

Finally, I thank my extraordinary wife Emily Lawler for sharing this graduate school journey with me, for contributing her own time and brainwaves to help improve my work, for supporting me in countless ways, and for always believing in me.

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Chapter 1

Human Capital, Schooling Allocation in the Family, and Longevity: Evidence from US Twins

with Peter A. Savelyev, Robert F. Krueger, and Matthew K. McGue

1.1 Introduction

Although demographic data reveal a sharp positive correlation between education and longevity, this correlation alone is insufficient evidence to suggest that there is a causal effect of education on longevity, since confounding factors and reverse causality are likely present. Based on our analysis of a unique US twins data set, we present new empirical evidence that acquiring additional years of education does, in fact, lead to greater longevity, at least for men. We further leverage the data to examine the role of potential mediators between education and mortality and to investigate the degree to which families distribute educational investments to either compensate for or reinforce differences in the genetic endowments of their children.

This research contributes to two bodies of literature. First, we demonstrate a beneficial causal effect of education on health and longevity. Prior investigations of this effect based on natural experiments and twin studies yielded mixed findings, despite a long history of research. One class of estimation approaches uses compulsory schooling laws as natural experiments. Lleras-Muney (2005) finds a strong effect of compulsory schooling on mortality in the US. However, Mazumder (2008) argues that Lleras-Muney's estimated effect is weak at best and that her instrument may not be valid. Clark and Royer (2013) find no effect of compulsory schooling on mortality in their study of UK data, a result which matches Arendt (2005) and Albouy and Lequien (2009). Many other studies find a causal effect of compulsory education on health-related outcomes (Grossman, 2004; Grossman

and Kaestner, 1997; Silles, 2009).

Similarly, there is no uniformity among results based on the twins fixed effect approach. Some papers using twins data find that there is a causal effect of education on health or longevity (Fujiwara and Kawachi, 2009; Lundborg et al., 2016), while other papers find no effect (Behrman et al., 2011; Kohler et al., 2011). We contribute to the resolution of this controversy by using the largest available dataset of US twins. In particular, we address this question through the analysis of monozygotic (MZ, or "identical") twins, who share both a common genetic makeup and a shared childhood environment.

We also examine a second controversial topic: the manner in which families allocate resources among siblings. If one twin has a poorer genetic health endowment, will he or she receive more or less schooling than the other twin? We perform this analysis by comparing results for monozygotic twins with those for dizygotic (DZ, or "fraternal") twins, who have a shared childhood environment, just as MZ twins do, but differ from each other genetically, unlike MZ twins.

Our unique data come from the Minnesota Twin Registry (MTR), which is a project that was initiated in 1983 to identify and study twins born in Minnesota, and from the Socioeconomic Survey of Twins (SST), which is a 1994 follow-up survey of MTR participants. In particular, we consider same-sex twin pairs of whom both twins participated in both surveys and both twins provided information about their education levels. Our sample contains 944 twin pairs born between 1936 and 1955. We match the twin respondents from these surveys with information about their mortality that we recently collected. For identification we rely on the presence of identical genes in monozygotic (MZ, or "identical") twins, genetic variation in dizygotic (DZ, or "fraternal") twins, shared family background within all observed twin pairs, and the availability of essential background controls: birth weight and disabling injury. We also account for measurement error in schooling.

We apply a linear probability model to within-pair first-differences in order to estimate the extent to which a twin who has more years of schooling is expected to outlive his or

her twin who has fewer years of schooling. This within-twin-pair approach leverages the common background shared by twins in a given pair.

Our findings for males indicate that there is a strong protective effect of education on longevity, though we do not find a similar statistically significant effect for females. We do, however, find a protective effect of education on physical health among females. The statistically significant longevity effect that we find for males persists when we control for birth weight and for early life health events or conditions which may have affected education choices. We argue that the twin-based approach, combined with these robustness checks, reasonably enables us to interpret the estimated effect of education on longevity as a causal one. We also find that there is a protective effect of education on overweight for males. Finally, we show that parents tend to use educational investments to compensate for genetic health endowment gaps between their children.

1.2 Data

We use multiple data sets, gathered between 1983 and 2017, which longitudinally describe the lives of a set of twin pairs who were born in Minnesota. Each pair in the sample was raised together.

1.2.1 Minnesota Twin Registry

Collected by researchers at the University of Minnesota, the Minnesota Twin Registry (MTR) represents the largest twins study conducted in the US, and it has been used in a number of influential publications in economics (e.g., Antonovics and Town, 2004; Behrman and Rosenzweig, 1999, 2002, 2004; Zyphur et al., 2009), in addition to numerous publications in other fields of study. As described in Krueger and Johnson (2002), the MTR was initiated in 1983 and includes data on twins born in Minnesota between 1936 and 1955. The MTR staff identified the twins retrospectively from their birth records and contacted twins to ask for their participation in surveys in person, by mail, and over the

phone. The MTR staff located about 80% of the identified twins. Among those who were located, about 80% agreed to participate. There were 4307 twin pairs in which both twins participated. MTR participants answered survey questions about an array of topics, including their education and health backgrounds, and researchers gathered participants' birth weight data directly from their birth certificates.

1.2.2 Socioeconomic Survey of Twins

In 1994, MTR respondents from same-sex twin pairs were re-surveyed in the Socioeconomic Survey of Twins (SST), which gathered further information from each twin regarding labor market participation, wages, health, and education.[1] SST respondents were also asked to provide information about their parents, siblings, spouses, and children. Importantly for our analysis, the SST gathered reports of educational background for both the respondent and the respondent's twin, meaning that we have two separate observations on years of schooling for each twin who is part of a participating SST pair. 1325 intact twin pairs returned valid SST questionnaires. (Behrman and Rosenzweig, 2002)

1.2.3 Characteristics of the Twin Sample

The MTR sample is almost entirely white, which is consistent with the historical demographics of Minnesota. We exclude from the estimation sample the small number of twins with at least one non-white parent, since the data are insufficient for a reliable study of the minority population.[2] Our twin sample thus consists of twin pairs who are white and the same sex, and in which both twins provided education information and also participated in both the initial MTR and the follow-up SST surveys. This gives us a sample of 674 male and 1214 female twins, whose individual characteristics are described in Table 1.1. Summary statistics for pairs of twins are shown in Table 1.2, including intra-pair differences

[1] See Behrman and Rosenzweig (1999) for a more thorough description of the SST.

[2] In 1960, people of color represented 1.2% of Minnesota's total population (US Census Bureau, 1960). Only two non-white twin pairs with non-missing data are excluded from the estimation sample.

for key measures. Although twins are generally similar to one another, they do differ in education level by over one year on average. Twins have lower birth weight on average than singletons, but the MTR twins in our sample are otherwise reasonably representative of their Minnesota birth cohort (Krueger and Johnson, 2002).

In the initial surveys conducted in the mid 1980s, respondents indicated whether they had experienced any disabling injury; we code this information as a dummy variable equal to one if any such injury is reported and zero otherwise. From the 1994 SST survey, we also obtained respondents' self-rated overall health on a five-point scale (1 being bad and 5 being excellent), as well as dummy variables for a report of any "family, job, or health problems due to alcohol use" and for any report of physical health problems.[3]

Table 1.1: Summary Statistics for Individual Twins

		Males		Females	
Variable	Year of observation	Mean	Standard Deviation	Mean	Standard Deviation
Year of birth	At birth	1947.7	5.4	1947.9	5.6
Birth weight in pounds	(1936-1955)	5.96	1.14	5.68	1.11
Monozygotic	1983	0.61	0.49	0.58	0.49
Ever had a disabling injury?		0.32	0.47	0.33	0.47
Years of education		15.1	2.3	14.1	2.2
Physical health problems		0.51	0.50	0.51	0.50
Self-reported health (1-5)	1994	4.42	0.64	4.36	0.66
Overweight		0.66	0.47	0.42	0.49
Alcohol problems		0.045	0.206	0.016	0.124
Died by 2014?		0.096	0.295	0.063	0.244
Age at death if died	1994-2014	62.8	7.6	62.9	8.8
Number of individuals		674		1214	

[3]Respondents were asked about a list of physical health problems which included migraine headaches; hay fever; frequently occurring skin rash; hearing impairment; high blood pressure; heart condition; and loss of function in the neck, back, arms, or legs.

Table 1.2: Summary Statistics for Pairs of Twins

Variable	Year of observation	Males		Females	
		Mean	Standard Deviation	Mean	Standard Deviation
Both died by 2014	Death	0.030	0.170	0.020	0.139
At least one died by 2014	(1994-2014)	0.16	0.37	0.11	0.31
Absolute difference within pairs					
Birth weight, pounds	Birth (1936-1955)	0.68	0.77	0.69	0.61
Ever had a disabling injury?	1983	0.13	0.33	0.12	0.32
Education, years		1.52	1.62	1.19	1.42
Physical health problems		0.42	0.49	0.38	0.49
Self-reported health (1-5)	1994	0.53	0.63	0.51	0.61
Overweight		0.29	0.46	0.30	0.46
Alcohol problems		0.07	0.26	0.03	0.17
Age at death if both died	1994-2014	4.90	5.16	8.00	6.30
Number of individuals		674		1214	

1.2.4 Mortality Data

To construct mortality data for these twins, we gathered data from both the Social Security Death Master File and Centers for Disease Control's National Death Index. Research on the reliability of these databases in identifying deaths found that over 90% of deaths are correctly identified by each database (Hauser and Ho, 2001; Wentworth et al., 1983). We further improve this figure by cross-referencing multiple databases, as well as contacting next-of-kin in some cases. The most recent year for which decedent data are currently available is 2014; we are reasonably certain that we know 1) whether any given twin respondent died before 2015 and 2) the date of death for any respondent who died before 2015. The last SST surveys were completed by respondents in December 1994. The available mortality data provides us with a 20 year risk period window between the initial date when living twins participated in the SST and the date when the mortality status of respondents was last observed.

1.3 Methodology

We apply a linear probability model to within-twin-pair first differences among monozygotic twins in order to determine the effect of education on health outcomes. We then compare results for monozygotic twins to those for dizygotic twins in order to characterize the manner in which families allocate educational resources among siblings with differing health endowments.

1.3.1 Model of Schooling Decision and Health

Behrman et al. (1994) lay out a model for the determination of schooling and wages for twins. We adapt this model to the case of schooling and health outcomes as follows.

Consider a family j with twin children i and k. The family allots S years of schooling to each twin according to the equations

$$S_{ij} = \alpha_1 a_{ij} + \alpha_2 a_{kj} + \delta h_{ij} + f_j + \boldsymbol{\alpha_3} \mathbf{x}_{ij} + u_{ij} \tag{1.1}$$

and

$$S_{kj} = \alpha_1 a_{kj} + \alpha_2 a_{ij} + \delta h_{kj} + f_j + \boldsymbol{\alpha_3} \mathbf{x}_{kj} + u_{kj}. \tag{1.2}$$

Here, a_{ij} and a_{kj} represent individual-specific endowments which are associated with poor health. h_{ij} and h_{ij} represent genetic background. f_j represents family environment. $\mathbf{x}_{ij}$ and $\mathbf{x}_{kj}$ represent vectors of exogenous confounders—in our case, birth weight and history of disabling injury. u_{ij} and u_{kj} are random shocks to educational attainment.

Mortality outcomes M for each twin are determined by equations:

$$M_{ij} = \beta_1 S_{ij} + h_j + a_{ij} + \gamma f_j + \boldsymbol{\beta_2} \mathbf{x}_{ij} + \upsilon_{ij} \tag{1.3}$$

and

$$M_{kj} = \beta_1 S_{kj} + h_j + a_{kj} + \gamma f_j + \boldsymbol{\beta_2} \mathbf{x}_{kj} + \upsilon_{kj}, \tag{1.4}$$

where υ_{ij} and υ_{kj} are random shocks to health.

β_1 is a key parameter of interest. $\beta_1 < 0$ would imply that additional years of schooling beneficially reduce mortality. Other key parameters are α_1 and α_2, which describe the own- and cross-effects of individual health endowments on the family's distribution of educational resources between twins. If we have $\alpha_1 < 0$ and $\alpha_2 > 0$ (case 1), this would imply that families reinforce differences in health endowments by increasing years of schooling for the better-endowed twin (the twin with the lower value of a) at the expense of the worse-endowed twin (the twin with the higher value of a).[4] If we have $\alpha_1 > 0$ and $\alpha_2 < 0$ (case 2), this would imply that families instead compensate for differences in health endowments. Finally, $\alpha_1 = \delta$ and $\alpha_2 = 0$ (case 3) would imply that the educational investment for each twin is set individualistically and without regard to the other twin's endowment.

1.3.2 Within-Twin-Pair First Differences

Monozygotic twins have identical genetic endowments; for monozygotic twin pairs in our model, $a_{ij} = a_{kj}$. Taking the difference between Equations (1.1) and (1.2) yields the following equations for the difference in years of schooling and for the difference in mortality among monozygotic twin pairs:

$$\Delta S_j^M = \boldsymbol{\alpha_3} \Delta \mathbf{x_j^M} + \Delta u_j^M \tag{1.5}$$

and

$$\Delta M_j^M = \beta_1 \Delta S_j^M + \boldsymbol{\beta_2} \Delta \mathbf{x_j^M} + \Delta \upsilon_j^M, \tag{1.6}$$

from which we can identify β_1.

Dizygotic twins from the same pair have different genetic endowments, a, so the anal-

[4] Note that the signs of these relationships are reversed from those described in Behrman et al. (1994), since we normalize the latent health endowment associated with the adverse outcome of mortality, while they normalize the latent wage-earning endowment associated with the beneficial outcome of wage.

ogous equations for dizygotic twin pairs are:

$$\Delta S_j^D = (\alpha_1 - \alpha_2)\Delta a_j + \boldsymbol{\alpha_3}\Delta \mathbf{x_j^D} + \Delta u_j^D \tag{1.7}$$

and

$$\Delta M_j^D = \beta_1 \Delta S_j^D + \Delta a_j + \boldsymbol{\beta_2}\Delta V x_j^D + \Delta \upsilon_j^D. \tag{1.8}$$

In the system of equations represented by Equations (1.5)–(1.8), α_1 and α_2 are not individually identified. However, as shown by Behrman et al. (1994), if we impose two restrictions—β_1 is identical for both MZ and DZ twins, and the individual-specific stochastic components υ_{ij} and u_{ij} are drawn from the same distribution for both MZ and DZ twins—then the difference $(\alpha_1 - \alpha_2)$ is identified and can be calculated in the following way:

$$\alpha_1 - \alpha_2 = \frac{1-R}{\beta_1^D - \beta_1^M} \tag{1.9}$$

where $0 < R = \frac{var(\Delta S^M)}{var(\Delta S^D)} < 1$, and β_1^D and β_1^M represent the estimates from Equation (1.6) for the DZ and MZ twin subsamples, respectively. The sign of $(\alpha_1 - \alpha_2)$ matches the sign of $(\beta_1^D - \beta_1^M)$. Identification of this difference is sufficient to determine whether families engage in compensating behavior versus two alternatives. As follows from cases 1-3 discussed in the previous section, $\alpha_1 - \alpha_2 > 0$ implies compensation (case 2). $\alpha_1 - \alpha_2 < 0$ implies either reinforcement or allocation without regard for the other twin (cases 1 and 3). Here we assume that in case 3 $\delta < 0$, given the well-known complementarity between education and health (e.g. Becker (2007)).

1.3.3 Addressing Measurement Error through Instrumental Variables

As Griliches (1979) points out, measurement error bias is particularly troublesome in estimates derived from twin data, like ours. Although birth weights as well as dates of death and birth for the twins in our sample are drawn from official records, years of schooling

are reported by the twins themselves and are likely subject to sizable measurement error.[5] Ashenfelter and Krueger's (1994) elegant instrumental variables approach eliminates measurement error bias resulting from any individual's tendency to over- or under-report education levels. The Ashenfelter-Krueger approach uses one twin's report of the intra-twin-pair difference in education as an instrument for the other twin's report of the same difference. We apply this IV approach in our analysis, using the 1994 SST survey data in which each twin reported both own and twin's education backgrounds.

Consider twins 1 and 2 from same-sex pair j. Let S_k^i represent twin i's report of twin k's years of schooling, and let $\Delta_{Si} = (S_1^i - S_2^i), i = 1,2$, which is how many more years of schooling twin 1 had than twin 2 based on twin i's reports. Then the first stage regression for each gender in this two-stage least squares framework can be written as

$$\Delta_{S1} = a_0 + b\Delta_{S2} + \mathbf{c}\Delta\mathbf{x}_j + \varepsilon_j. \tag{1.10}$$

In the second stage of this IV approach, the observed difference in mortality outcomes is regressed on the predicted value of the difference in education $\widehat{\Delta_{S1}}$ as calculated in the first stage regression:

$$\Delta M_j = \beta_1 \widehat{\Delta_{S1}} + \gamma\Delta\mathbf{x}_j + \Delta\varepsilon_j, \tag{1.11}$$

Ashenfelter and Krueger demonstrate that this approach generates unbiased estimates of β_1, the coefficient of interest, even when a twin's reports of her own education and of her twin's education have measurement errors that are correlated with one another. We estimate bootstrapped standard errors for the estimate of β_1.

We estimate versions of (1.10) and (1.11) under each of two alternative specifications: (1) with no control for $\Delta\mathbf{x}_j$; and (2) controlling for $\Delta\mathbf{x}_j$, with missing values for $\Delta\mathbf{x}_j$ imputed using Markov chain Monte Carlo multiple imputation as described in Rubin (1987) and

[5] Twins from the same pair were likely weighed on the same hospital scale at birth, so differencing birth weights cancels out any possible systematic additive measurement error that may result from miscalibration of scales.

Schafer (1997), a method that preserves the variance-covariance matrix of variables in the data. Results from estimating approaches (1) and (2) are close to one another. We report results for specification (2) in the main text and show a comparison with specification (1) in appendix table A.1.

As evidenced by Behrman and Rosenzweig (2002) and follow-ups to that work (Antonovics and Goldberger, 2005; Behrman and Rosenzweig, 2005), there is no single, straightforward way to translate each individual's responses to the array of SST questions into a count of total years of schooling. We follow the same procedure used by Antonovics and Goldberger (2005) for coding years of schooling from the raw SST responses, in which years of schooling are defined based on the highest degree achieved as well as any additional reported schooling beyond the highest degree.[6]

1.4 Results

1.4.1 Health Outcomes

Results from estimation of the first stage regression in Equation (1.10) each for monozygotic (MZ) twins and dizygotic (DZ) twins are shown in Table 1.3. Unsurprisingly, one twin's report of the intra-pair difference in years of schooling is a strong instrument for the other twin's report of the same difference.

Applying the 2SLS approach described above to the samples of MZ twins of each sex allows us to identify β_1 from Equation (1.11), the effect of education on health outcomes. Specifically, we consider the outcomes of mortality (death within 20 years after the 1994 SST survey), any report of physical health problems in the SST, and overall health as self-reported in the SST.

[6]For example, a high school degree is coded as 12 years of schooling or a college degree as 16. A twin who reports a high school degree plus one year of college will be coded 13 years of schooling. However, a twin who has not completed a particular degree can be coded with at most one less year than is associated with that degree, regardless of how many years they report. Thus a twin who reports a high school degree plus five years of college but no college degree will be coded with 15 years rather than 17.

Table 1.3: First Stage Results, Intra-Pair Difference in Years of Schooling as Reported by Twin One

		Pooled genders	Males	Females
Monozygotic	coefficient	0.780 ***	0.860 ***	0.710 ***
	standard error	*(0.040)*	*(0.068)*	*(0.049)*
	F -statistic	*380*	*160*	*210*
	# of twin pairs	558	204	354
Dizygotic	coefficient	0.854 ***	0.855 ***	0.850 ***
	standard error	*(0.029)*	*(0.039)*	*(0.039)*
	F -statistic	*867*	*481*	*475*
	# of twin pairs	386	133	253

Notes: We use specification (1.10) for each gender. For the pooled sample we additionally control for a gender dummy.

Rather than assume any particular asymptotic behavior of our coefficient estimates, we use bootstrap standard errors. In light of consistent evidence in the literature that the effects of education on health, longevity, and a number of health behaviors are non-harmful,[7] we use one-sided tests for the estimated coefficients on years of schooling in our regressions.[8]

Second stage results for the estimated values of β_1 are shown in Table 1.4. The estimate for males indicates that each additional year of schooling yields a 3.1 percentage point drop in the probability of death during the 20 years immediately following the 1994 SST survey. The comparable estimate for females is not statistically significant at conventional levels, though the estimate for the pooled sample of males and females is statistically significant and negative. Among the sample of all MZ twins, each additional year of schooling is associated with improvements of 3 percentage points in the probability of reported physical health problems and .03 standard deviations in self-reported overall health (both as reported

[7] See, for example, Grossman (2006) and Grossman and Kaestner (1997).

[8] There is one exception to this practice in this paper: due to the lack of consensus on the existence or direction of a causal effect of education on alcohol consumption, we perform two-sided significance tests when estimating the influence of years of schooling on the occurrence of alcohol-related problems.

Table 1.4: Second Stage Results, Effects of Education on Mortality and Health

		Pooled genders	Males	Females	Gender Difference
Mortality	coefficient	-0.023**	-0.031**	-0.015	-0.015
	standard error	*(0.012)*	*(0.018)*	*(0.015)*	*(0.024)*
	# of twin pairs	558	204	354	
Physical Health Problems	coefficient	-0.029**	-0.022	-0.034*	0.012
	standard error	*(0.019)*	*(0.028)*	*(0.025)*	*(0.038)*
	# of twin pairs	694	244	450	
Self-reported Health	coefficient	0.029*	0.040	0.015	0.025
	standard error	*(0.022)*	*(0.032)*	*(0.033)*	*(0.046)*
	# of twin pairs	680	241	439	

Notes: Bootstrapped standard errors reported from 300 replications. One-sided p-values reported for estimated coefficients on years of education, two-sided for difference in coefficient estimates across genders. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

in the 1994 SST).

Given the lack of a strong relationship between education and mortality among women, the presence of a stronger connection between education and physical health problems may seem puzzling. This pattern of results could be related to the established fact that at any given age women tend to report worse health than men but are also less likely to die than men (Case and Paxson, 2005). It is also important to note that the list of health conditions captured by our dependent dummy variable includes relatively minor conditions like hay fever or frequently occurring skin rash alongside major ones. Such minor conditions are unlikely to be strongly related to middle age mortality.

On the other hand, prior research established that individuals with higher income and education consume more health care, all else equal (Strauss and Thomas, 1998). Some health conditions, like high blood pressure or heart conditions, are not likely to be known to

the respondent in the absence of a diagnosis from a medical professional. Accordingly, our coefficient estimates, which describe the protective effect of education on the probability of reporting *awareness* of having experienced a physical health problem, likely understate education's effect on the probability of truly *experiencing* a physical health problem. Indeed, if the more educated had the same amount of health issues as the less educated but were more aware of them, we would find a positive effect of education on the probability of reporting health problems. Despite this expected bias, we find a negative effect, suggesting a substantial true effect of education on the probability of experiencing health problems. While health problems are self-reported by twins and measure awareness rather than objective health status, mortality, which is our main outcome of interest, is an objective measure and is not susceptible to the same type of bias.

At the start of our observed mortality window on December 1, 1994, twins' ages ranged from 39 to 58. At the end of the mortality window, ages ranged from 59 to 78. In order to identify possible heterogeneity in the effect of education across birth cohorts and at different ages, we divide the 20 year mortality window into two halves, divide the twin sample's twenty year birth cohort into two halves, and re-estimate our mortality model for each of the four combinations of birth cohort and mortality window. The results, displayed in Table 1.5, show that the protective effect of education on mortality is concentrated at the oldest ages. Among the subsample born 1946–1955, there is no evidence that additional years of education improved mortality between December 1994 and November 2004, i.e. between ages 39 and 58. The younger cohort during the later mortality window fell in the same age range that the older cohort did during the earlier mortality window; in each case, the twins ranged from age 49 to 68. Effects for each birth cohort during this age range are not precisely determined. We find statistically significant evidence of an improvement in mortality due to additional years of schooling among male twins only among the older cohort and in the later ten-year mortality window (between ages 59 and 78). Among this older subsample, for each additional year of schooling there is an estimated 4.8 percent

reduction in the likelihood of dying during the ten year span beginning December 1, 2004.

Table 1.5: Effects of Education on Mortality by Cohort and Date of Death

			Pooled genders	Males	Females
Born 1936-1945	Mortality	coefficient	0.012	0.016	-0.004
	12/94 - 11/04	standard error	*(0.019)*	*(0.027)*	*(0.010)*
		# of twin pairs	242	86	156
	Mortality	coefficient	-0.035*	-0.048**	-0.007
	12/04 - 11/14	standard error	*(0.022)*	*(0.025)*	*(0.041)*
		# of twin pairs	224	74	150
Born 1946-1955	Mortality	coefficient	0.001	-0.001	0.001
	12/94 - 11/04	standard error	*(0.003)*	*(0.003)*	*(0.006)*
		# of twin pairs	378	129	249
	Mortality	coefficient	-0.004	-0.006	-0.004
	12/04 - 11/14	standard error	*(0.011)*	*(0.021)*	*(0.012)*
		# of twin pairs	369	126	243

Notes: Bootstrapped standard errors reported from 300 replications. One-sided p-values reported for estimated coefficients on years of education. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

The available sample of twins is not sufficiently large to allow us to fully analyze the effects of education on mortality separately by each cause of death. However, the fact that the overall effect is concentrated among relatively older ages suggests that the key mediating factors between education and mortality relate to health stock (e.g. due to smoking or diet) more than they relate to accidental or external causes of death (e.g. due to seat belt use), which are relatively more frequent at younger ages of the US population (CDC, 2015). Indeed, the most common causes of death observed among the twin sample are those related to cancer, heart disease, or respiratory disease.

1.4.2 Mediators

We have limited data on the possible mediators that drive the effects of education on health outcomes, but alcohol problems and overweight are two key health-related factors which were queried in the 1994 SST. Twins indicated their height and weight at the time of the SST, from which we generate a dummy variable for overweight based on a body mass index of 25 or above. The twins also indicated whether they had ever experienced "family, job, or health problems due to alcohol use."

2SLS estimates for the effects of education on overweight and alcohol problems are presented in Table 1.6. Among men, each additional year of education yields a statistically significant 4.5 percentage point decrease in the likelihood of being overweight. No such statistically significant relationship between education and overweight is identified among women. We find statistically weak evidence suggesting that education causes a reduced propensity for alcohol problems among the pooled sample of men and women. Because of the lack of a consensus in the health economics literature regarding the relationship between educational attainment and alcohol use, we apply a two-sided test of statistical significance to the coefficient estimates for alcohol problems.

Table 1.6: Effects of Education on Potential Mediators of Health

		Pooled genders	Males	Females	Gender Difference
Overweight	coefficient	-0.010	-0.045**	0.025	-0.070**
	standard error	*(0.014)*	*(0.022)*	*(0.017)*	*(0.028)*
	# of twin pairs	670	240	430	
Alcohol	coefficient	-0.012*	-0.016	-0.009	-0.007
	standard error	*(0.007)*	*(0.013)*	*(0.008)*	*(0.015)*
	# of twin pairs	694	244	450	

Notes: Bootstrapped standard errors reported from 300 replications. One-sided p-values reported for coefficient estimates for overweight, two-sided for alcohol and for gender differences. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

The overweight dependent variable is based on the twins' self-reported height and weight, and the alcohol problem dummy variable is based on twins' answers to the question of whether they ever experienced "family, job, or health problems due to alcohol use." Neither of these two measures would involve any specific diagnosis from a medical professional, unlike some of the physical health measures considered in Table 1.4.

1.4.3 Intra-Household Allocation of Resources

We can determine whether parents compensate for or reinforce endowment differences in siblings by estimating the value of $(\beta_D - \beta_M)$. Our estimates of this difference are shown in Table 1.7. Statistically significant positive numbers are indicative of compensating behavior, in which the less healthy twin receives more education, and the healthier twin less education. Our estimation results provide some evidence of compensating behavior when health is measured in terms of eventual mortality, and strong evidence of compensating behavior when health is measured in terms of physical health problems. The outcome of self-reported health provides no precisely determined estimate of the difference, but this outcome is arguably the noisiest. The implied point estimates for the values of $(\alpha_1 - \alpha_2)$ are shown in Table A.2. Our finding of compensating behavior with respect to health endowments contrasts with Behrman et al. (1994), who find that families reinforce earnings endowments.

There are different dimensions of latent endowments. In particular, each individual may have a wage-earning endowment which is not perfectly correlated with the health endowment. Since wages as well as education can affect longevity, our estimates of $(\alpha_1 - \alpha_2)$ may be biased due to the omission of an unobserved earning endowment. The data available for our sample of twins does not include early-life measures of ability which would allow us to model such a multi-dimensional endowment framework. We do, however, have good data from the SST on the wages of male respondents.[9] We leverage this wage data by

[9] Wage data were gathered for women as well as men, but an insufficient number of female twin pairs have

Table 1.7: Difference in Education Coefficients between DZ and MZ Twins, $(\beta_1^D - \beta_1^M)$

		Pooled genders	Males	Females
Mortality	coefficient	0.026*	0.036	0.019
	standard error	*(0.015)*	*(0.022)*	*(0.018)*
Physical Health Problems	coefficient	0.066***	0.060*	0.073**
	standard error	*(0.024)*	*(0.036)*	*(0.032)*
Self-reported Health	coefficient	0.020	0.022	0.029
	standard error	*(0.030)*	*(0.044)*	*(0.044)*

Notes: Bootstrapped standard errors calculated from 300 replications. Two-sided p-values reported for difference in coefficient estimates between DZ and MZ twins. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

re-estimating the DZ-MZ differences in estimates of β_1 as shown in Table 1.7 with wage as an added regressor. In this way, wage proxies for earning ability. Though wage itself is an endogenous outcome, we argue that this approach provides a helpful robustness check. If the DZ-MZ differences are robust to the inclusion of the wage control, then we have evidence that our results are not driven by the wage-earning endowment.

Results from estimating models that include wage as a background control are shown in Table 1.8. The implied effect of education on mortality is not greatly affected by the inclusion of a wage control (and in fact the result is strengthened). There is little change in the implied effect of education on self-reported health and a modest decrease for the effect on physical health problems. Taken together, we interpret these results as evidence that the single-dimensional measure of child's endowment a_1 which we consider in our analysis does in fact capture a health-related endowment and is not dominated by non-health-related wage-earning potential.

wage information provided for both twins.

Table 1.8: Robustness Check: Difference in Education Coefficients between DZ and MZ Twins, $(\beta_1^D - \beta_1^M)$, with and without Wage Controls, Males

		No wage control	Wage control
Mortality	coefficient	0.036	0.039*
	standard error	*(0.022)*	*(0.023)*
Physical Health Problems	coefficient	0.060*	0.051
	standard error	*(0.036)*	*(0.036)*
Self-reported Health	coefficient	0.022	0.021
	standard error	*(0.044)*	*(0.066)*

Notes: Bootstrapped standard errors calculated from 300 replications. Two-sided p-values reported for difference in coefficient estimates between DZ and MZ twins. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

It is likewise possible that parents respond to differences in children's health endowments by reinforcing some of the resulting gaps in outcomes while simultaneously compensating for other gaps. This is precisely the finding of Yi et al. (2015), who use Chinese twins data to show that when one twin experiences a negative health shock in childhood, the parents divert educational resources toward the healthier twin (reinforce) but divert health resources toward the less healthy twin (compensate). Parents compensate more than they reinforce, so the authors find that families' overall behavior is compensatory. We do not observe information about childhood health investments, e.g. physician visits, for our twin sample, so we are unable to identify this kind of multidimensional response to differences in health endowments. However, our findings for US twins indicate that parents divert educational resources toward the less healthy twin, which is the opposite of the finding by Yi et al. for Chinese twins, suggesting differences in the representative household objective

function for the Chinese versus US families sampled.[10]

1.5 Conclusions

We use a longitudinal data set of US twins and newly collected mortality data to examine the relationship between educational attainment, health, and longevity. We find that increased education is associated with improved mortality among male twins. We find no statistically significant relationship between schooling and mortality among female twins.

Our results for the effect of schooling on overweight follow a similar pattern; men but not women show a reduction in probability of overweight for each additional year of schooling. Furthermore, we find suggestive evidence of a small beneficial effect of schooling on the incidence of problems associated with alcohol use. Together, these findings suggest a mediating role for diet and exercise in translating education into improved health. This hypothesis is further supported by our finding that the effects of education on mortality are concentrated on ages 59–78 when death usually stems from medical conditions, rather than on younger ages when death from accidental causes is more prevalent.

Our analysis has limitations given the specific population that we consider. Generalization outside of the twin population should be done with caution, as twins have the unique experience of having another twin present throughout childhood.[11] Our findings are not representative of racial minorities, who are absent from our sample of Minnesotans, nor are our findings representative of cohorts born after 1955. In particular, it is possible that changes in females' demand for education, the market for women's labor, and societal gender roles may mean that these effects will be different for young women today than they are for the women in the sample we consider here.

[10] Apart from the differences in countries considered in these papers, there are also differences in the types of educational investments and health differences considered. Yi et al. analyze payments for schooling, while we analyze total years of schooling. They consider responses to early life health shocks, while we consider health endowments identified by observed longevity.

[11] However, to a first-order approximation, our approach of taking within-twin-pair differences successfully controls for any unique aspects of being a twin.

We do find evidence among the pooled-gender twin sample indicating that additional schooling reduces the likelihood of reporting any physical health problems at middle age. This effect is stronger among women. We do find evidence that education improves women's physical health, but we find no effect of education on mortality for women at the ages we observe. This combination of findings may indicate that education affects women's health in ways that improve quality of life but not mortality; alternatively, it is possible that the effects of education on mortality among women are significant only beyond the ages that we observe (up to a maximum of 78).

Finally, by comparing calculations for monozygotic twins who share the same genes to dizygotic twins who differ genetically, we show that families tend to distribute educational investments between their twin children in a way that compensates for differences in the health endowments of those twins. Among families with twins who differ in terms of genetic health endowments, there is an average tendency to divert schooling resources away from the healthier twin and toward the less healthy twin. This finding contributes to our economic understanding of patterns of intra-family allocation of resources. Given that related studies found evidence that families reinforce rather than compensate for gaps in children's non-health endowments, our results suggest that families consider the health and non-health endowments of their children separately when they allocate educational resources. Simultaneous estimation of families' responses to multiple dimensions of endowments requires richer background data than is available to us; pursuit of this type of multidimensional approach represents a promising avenue for further research.

Chapter 2

Random Urine Screening as a Tool to Deter Opioid Abuse: Evidence from Louisiana

with Aaron M. Gamino

2.1 Introduction

Between 1999 and 2014, the rate of deaths from opioid painkiller overdoses in the US quadrupled, with a total growing to over 18,000 deaths per year (CDC, 2016b). Due largely to this drug problem, overall mortality rates have worsened among middle-aged white non-Hispanic males during this period, even as medical technologies and access to health care have improved (Case and Deaton, 2015). As the devastating scope of this epidemic has come into focus, a number of policy responses have been implemented in order to curtail the misuse and abuse of opioids. One such policy is the requirement of routine drug testing for pain clinic patients who receive opioid prescriptions for the treatment of their chronic pain. Such patients are at a particularly high risk of developing an opioid addiction (Volkow and McLellan, 2016). Mandates for drug testing are intended to enable practitioners to quickly identify problematic drug use patterns among patients and to address such problems appropriately and proactively. In particular, Louisiana introduced a policy in 2008 requiring that prescribers practicing at pain clinics conduct random urine drug testing (UDT) of each patient receiving long term opioid prescriptions at least four times per year.

In this paper, we examine the effects of Louisiana's UDT policy on outcomes related to the use and abuse of opioids. We find evidence of sharp reductions in the quantity of opioids distributed and in overdose deaths following the introduction of this policy.

2.1.1 Policy background

A number of policy responses have been deployed in an effort to remedy the devastating opioid epidemic in the US. New training initiatives have been introduced to better educate medical practitioners about the potential dangers of prescribing opioids for pain. Starting with the popular prescription drug Oxycontin in 2010, abuse-deterrent formulations of opioids have been introduced which prevent users from inappropriately consuming the active ingredients all at once.[1] Law enforcement agencies have carried out crackdowns on so-called "pill mills"—medical practices at which professionals irresponsibly issue excessive quantities of prescriptions to patients, many of whom may be addicted.

The most widely used weapon in the fight against the US opioid epidemic has been the introduction of prescription drug monitoring programs (PDMPs), which are centralized databases that track patients' prescription histories in an effort to help practitioners and pharmacists identify and prevent excessive drug use. Of particular concern is "doctor shopping" behavior by addicted patients, who visit multiple practitioners in hopes of securing opioid prescriptions to satisfy their addictions. As of 2017, every US state has initiated some form of PDMP. Not all PDMP's are created equal, however. Recent studies by Patrick et al. (2016) as well as Buchmueller and Carey (2018) have found that state PDMP's which include stringent requirements on practitioners or pharmacists to use the database when issuing prescriptions have served to reduce rates of drug misuse and overdose deaths. These researchers find no evidence that less stringent state PDMP's have had any beneficial effect on opioid outcomes.

The above-listed policy efforts have made strides in fighting the US opioid epidemic, but they also have limitations. In particular, none offers a consistent means of identifying patients who misuse or divert legitimately prescribed opioids. By administering urine drug testing (UDT) to his or her patient, a prescriber can determine what drugs, and in what con-

[1] See Alpert et al. (2018) for analysis of the consequences, both intended and unintended, of introducing abuse-deterrent opioids.

centrations, are in the patient's system. The results can indicate multiple possible issues. The presence of narcotics or other unprescribed drugs is indicative of possible drug abuse. If UDT indicates a greater than expected concentration of the prescribed opioid in the patient's system, this may mean that the patient is overusing the prescribed drug or acquiring additional quantities of the drug through either doctor shopping or illegal acquisitions from a third party. If the prescribed drug is absent from the UDT results, this may mean that the patient is inappropriately diverting the prescribed drug by giving it to acquaintances or selling it on the black market. All of these possibilities involve a high risk of addiction or overdose. Given the potential of UDT to identify these problems or prevent them altogether, the CDC issued a 2016 guideline recommending that clinicians use UDT at the commencement of a patient's therapy when prescribing opioids for the treatment of chronic pain and that they consider re-using UDT at least annually as treatment continues(CDC, 2016a).

Despite the CDC's recommendation, practitioners may be hesitant to adopt a blanket strategy of ordering UDT for all of their patients who receive opioid prescriptions for chronic pain treatment. Prescribers may expect that even those patients who have a legitimate medical need for opioids might balk at participating in a drug test and instead seek another prescriber who doesn't administer UDT. To induce the consistent use of UDT among a high risk set of patients, Louisiana introduced a policy in 2008 mandating UDT practices among the state's pain clinics, defined as facilities that primarily engage in the treatment of pain by prescribing narcotic medications.

Though UDT requirements have received little research attention, researchers have investigated the effects of laws regulating pain clinics generally. Meara et al. (2016) find that state pain clinic regulations are associated with reductions in long-term receipt of prescription opioids among Medicaid recipients. Dowell et al. (2016) find that the combined use of mandatory PDMP review laws and pain clinic laws were associated with a decrease in opioid prescriptions and overdose deaths.

The 2008 Louisiana pain clinic mandate required that pain clinic patients receiving

opioid prescriptions for the treatment of chronic pain have UDT administered once at the start of treatment and at least four times per year thereafter. Furthermore, the timing of the ongoing tests should be "unannounced," such that the patient cannot easily feign adherence to the appropriate prescription regimen by following it only when a test is anticipated. If a pain clinic patient or staff member is found to be diverting or illegally using opioids, administrators of the clinic are required to make a medical referral to an addiction facility. Patients must sign a document acknowledging that they are subject to the established rules, including periodic, unannounced drug screens. As long as a patient is treated by a pain clinic and for six years thereafter, the clinic is required to maintain records of the patient's agreement to the rules as well as drug screening results and PDMP inquiry results (LA Admin Code Title 48:1 7801-7861). Individuals who violate Louisiana's pain clinic law face up to five years in prison and a fine of up to $50,000 (LA Rev Stat 40:971.2).

Florida introduced a similar UDT requirement for its pain clinics in 2011. Because of the idiosyncratic way in which the opioid epidemic and the battle against it unfolded in Florida during our sample period, we do not analyze Florida's UDT policy here, nor do we include Florida in the set of donor control states.[2]

A handful of other states also implemented UDT policies between 2000 and 2015, though none of these states' policies were as stringent as those of Louisiana and Florida. In particular, these other states' policies lacked either a requirement that the timing of drug tests be unannounced or a stipulation that UDT must be used more than once per year. Accordingly, we classify these other state policies as "weak" UDT requirements, in contrast to the "strong" ones implemented in Louisiana and Florida. In our analysis, we control separately for the presence of a weak UDT policy, and in all cases we find no evidence that such policies have an impact on the outcomes considered.

[2]Prior to 2010, Florida was an epicenter of the opioid epidemic, driven largely by a glut of so-called "pill mills," medical facilities which catered to drug addicts through unscrupulous or negligent prescribing practices. Between 2010 and 2012, Florida policymakers deployed a number of policy remedies, including conventional ones like a PDMP but also a less conventional crackdown on pill mills. See Rutkow et al. (2015) for further discussion and analysis of Florida's experience with opioids.

Louisiana's 2008 pain clinic policy is unique in its inclusion of a strong UDT requirement, but additional regulatory changes were included as well. In particular, the 2008 policy further required practitioners at pain clinics to consult Louisiana's PDMP when issuing opioid prescriptions. We are thus able to estimate the effects of Louisiana's entire policy bundle but not the effects of its UDT requirement alone. However, because a number of other states also introduced must-access PDMP requirements in between 2000 and 2015, we are able to estimate the extent to which improvements stemming from Louisiana's bundle of policies exceed improvements from other states' PDMP-only policies. Though evidence suggests that strong state requirements of PDMP use show promise in reducing opioid overdose rates, these rates remain well above their pre-2000 levels. There remains an unmet demand among policymakers for additional weapons in the fight against the opioid epidemic. In 2017, the President's Commission on Combating Drug Addiction and the Opioid Crisis was established to identify ways to combat problems associated with opioid abuse.

2.1.2 Opioids and US drug regulation

The US federal government classifies regulated drug types into five classifications, called "Schedules," under the provisions of the 1970 Controlled Substances Act. Drugs in Schedule I, the most heavily regulated class which includes heroin and LSD, are deemed to have a high potential for abuse and no currently accepted medical use. Under federal law, Schedule I substances may not be prescribed by anyone. Schedule II drugs are those which also have a high potential for abuse but which do have accepted medical use. Schedule II drugs may be administered or prescribed only under strict regulations, which are governed by both federal and state laws. There are a number of ways in which states differ from one another in the regulation of Scheduled drugs; for example, some states allow nurse practitioners to prescribe Schedule II drugs while others do not. The majority of the opioids which we consider in our analysis are Schedule II drugs, but not all Schedule II drugs

are opioids. Accordingly, each state's market for opioids may be affected both by opioid-specific regulations, like UDT requirements, and any changes to the market for Schedule II drugs generally.

One of the most commonly prescribed non-opioid Schedule II drugs is amphetamine, which is included in Adderall and other medications used for the treatment of attention deficit hyperactivity disorder. Although abuse of legal amphetamine medications does occur (Maxwell and Rutkowski, 2007), amphetamines are not used to treat the same conditions as opioids, nor would taking an amphetamine satisfy the drug dependence of an opioid addict. There is no reason to suspect that legal amphetamine medications serve as a substitute or complement for legal opioid medications. Because of this fact and because of amphetamine's Schedule II status, we compare opioid totals to amphetamine totals in order to evaluate the results of opioid policies while controlling for contemporaneous changes in each state's supply of heavily-regulated drugs overall.

2.2 Data

In this section, we describe the datasets used in our analysis. We use two data sets: Automation of Reports and Consolidated Orders System (ARCOS) and National Vital Statistics System (NVSS) Multiple Cause-of-Death file.

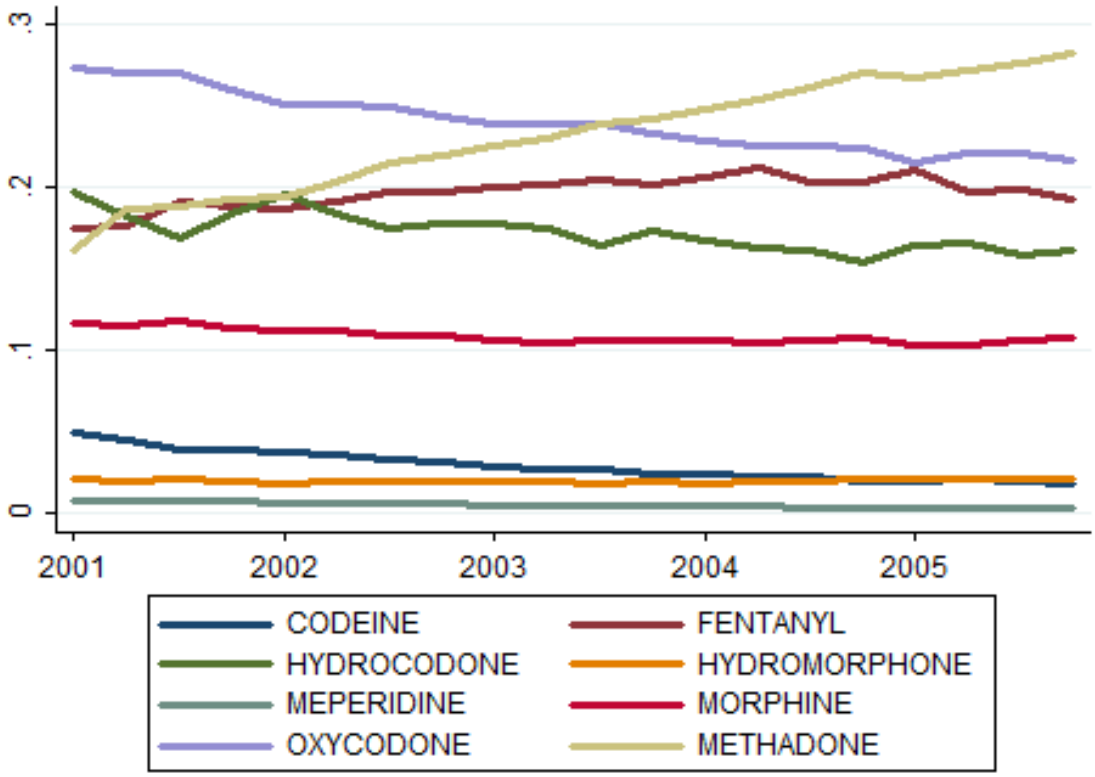
2.2.1 ARCOS

To track changes in the distribution quantities of legal opioids, we use data gathered between 2006 and 2010 by the US Drug Enforcement Administration's (DEA's) Automation of Reports and Consolidated Orders System (ARCOS). The ARCOS database tracks the quarterly quantity of certain drugs distributed to retailers—including pharmacies as well as hospitals and practitioners—in each ZIP3 (a cluster of ZIP codes that share the same three-digit prefix). For our analysis, we aggregate individual ZIP3 drug totals up to the state level.

In particular, we consider the quantities distributed for nine commonly prescribed opioid analgesics: buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, meperidine, methadone, morphine, and oxycodone. Since the pharmacological strength per milligram varies across opioid types, we convert the distributed quantity of each drug in grams into a morphine-equivalent quantity, using multipliers that have been established in the medical literature (Gammaitoni et al., 2003; Paulozzi et al., 2011), and sum across all opioid types to generate an aggregate quantity. Besides these opioids, we also consider the reported quantities of the non-opioid drug amphetamine, which is tracked by the ARCOS database in the same fashion.

Although ARCOS data is available dating back to 2000, only in 2006 did ARCOS reports begin to include drugs distributed to facilities for the treatment of substance use disorder (SUD). This results in a large increase in methadone quantities recorded beginning in 2006, as methadone is an opioid that is widely used in the treatment of opioid use disorder. To explore trends in distribution before the change in reporting, we show each opioid's share of the total morphine equivalent quantity of tracked opioids in Figure 2.1 between 2001 and 2005. Methadone's share of morphine equivalent quantity among retailers other than SUD treatment facilities nearly doubles in the entire United States during this period. This suggests that methadone was been being increasingly used for reasons other than substance use disorder treatment over this time. Buprenorphine is another opioid that is commonly used in the treatment of opioid use disorder; it was generally absent from ARCOS reports prior to 2006 and is included thereafter. In light of these data considerations, for our analysis of drug quantity data we consider a sample period of 2006 to 2010, centered around the 2008 introduction of Louisiana's UDT policy.

Figure 2.1: Individual drugs' share of total morphine equivalent opioid quantity



Notes: ARCOS quarterly data from 2001 to 2005, Florida excluded.

A limitation of the ARCOS data set is the fact that it tracks only legally distributed quantities of drugs. If crackdowns on abuse of prescription opioids should induce an increase in rates of abuse of heroin or illicitly manufactured opioids, as has been indicated by prior research of another supply-side intervention in the market for opioids (Alpert et al., 2018), then ARCOS will not enable us to observe this particular change in the drug market.

2.2.2 NVSS

To examine the effect on overdose deaths we use data from the National Vital Statistics System (NVSS) Multiple Cause-of-Death file, gathered by the CDC. This data set tracks the number of deaths by state, year, and cause of death. Because the coding system used for

classifying causes of death changed from ICD-9 to ICD-10 beginning in 1999, we consider a sample period that begins in 1999 and ends in 2015. We consider multiple drug-related causes of death: those associated with prescription opioids, any opioids, and any illicit drugs.

2.3 Empirical Strategy

2.3.1 Main estimation strategy

Our research designs take advantage of the variation across states and over time in the regulatory framework for UDT, as well as comparisons of drug types which are affected by UDT requirements with a drug type that is unaffected.

Our first approach employs a difference-in-differences (DD) model exploiting the policy variation across states and over time. The estimating equation for outcome Y_{st} for state s in period t is:

$$Y_{st} = \alpha + \beta(Post_t \times Treat_s) + \sigma X_{st} + \rho Z_{st} + \eta_s + \delta_t + \varepsilon_{st} \tag{2.1}$$

The coefficient of interest is β. $Post_t \times Treat_s$ is an indicator that takes a value of one for Louisiana after the implementation of random urine drug testing and zero for all other state-years. X_{st} contains controls for key state policies that may affect the market for opioids: the presence of a PDMP, the inclusion of "must-access" PDMP regulation requiring use of the system by medical professionals,[3] medical marijuana,[4] naloxone access laws and good Samaritan laws.[5] Z_{st} includes state demographic controls: distributions of age, race,

[3]Variation in the effects of states' PDMP's with respect to the characteristics of those programs are examined by Buchmueller and Carey (2018) and Patrick et al. (2016), who find evidence of significant improvements in opioid outcomes only when a state's PDMP includes robust provisions like required use or frequent updating.

[4]Bradford and Bradford (2016) and Powell et al. (2018) find that prescription opioid use drops among the examined populations when legal access to medical marijuana is introduced.

[5]Rees et al. (2017) find that opioid-related death rates fall when states introduce laws that expand access to naloxone, a drug which can counteract the effects of ingested opioids in an effort to prevent overdose. They also examine good Samaritan laws, which protect individuals from legal repercussions when seeking

and schooling level; unemployment; average income; poverty rate; and uninsurance rate. State and time fixed effects, η_s and δ_t, are included. We consider two types of outcome Y: quarterly ARCOS drug totals and annual rates of death from drug-related causes.

2.3.2 Additional regression strategies

Our additional regression strategies take advantage of further ARCOS data tracking pharmaceutical amphetamine, a drug which is not an opioid and is most notably contained in the prescription drug Adderall, as a comparison group. Like most of the opioids we consider, amphetamine is classified as a Schedule II drug under the US Controlled Substances Act, the most heavily-restricted class of prescription drugs. Because it treats an entirely different set of conditions than do opioids, prescription amphetamine is neither a substitute nor a complement for prescription opioids. It thus represents a suitable control for opioids, as the opioid-specific state policies which we examine here should have no effect on the prescribing or dispensing of non-opioid amphetamine, while state or federal policies which target Schedule II drugs generally should affect both opioids and amphetamine in a similar manner.

Our first additional estimation strategy is a DD model using variation in type of drug (opioid vs amphetamine) and timing, within a single state. The estimating equation for outcome Y_{dt} for drug d in quarter t is:

$$Y_{dst} = \alpha + \beta(Post_t \times Opioid_d) + \sigma X_{st} + \rho Z_{st} + \delta_t + \tau_d + \varepsilon_{dst} \tag{2.2}$$

The coefficient of interest is β. $Opioid_d$ is an indicator taking a value of one if drug d is an opioid and a value of 0 otherwise. The equation follows the same definitions as equation 2.1, with the addition of a drug fixed effect τ_d.

Our second additional estimation strategy is a difference-in-difference-in-differences (DDD) model using variation in type of drug, state, and timing.

assistance for someone who is in danger of overdose.

$$
\begin{aligned}
Y_{st} =& \alpha + \beta(Post_t \times Treat_s \times Opioid_d) \\
&+ \sigma X_{st} + \rho Z_{st} + \eta_s + \delta_t + \tau_d + sigma_{st} + \nu_{dt} + \gamma_{ds} + \varepsilon_{dst},
\end{aligned}
\tag{2.3}
$$

where the coefficient of interest is β and the terms follow the prior definitions. The additional terms included are drug-time and drug-state fixed effects, ν_{dt} and γ_{ds}, respectively. This DDD approach enables us to estimate the effect of Louisiana's 2008 opioid policy specifically on opioid distribution rates, net of any unobserved factors affecting the overall use of heavily-regulated drugs in Louisiana or any other state.

2.3.3 Permutation tests

Because we consider only a single state that is treated by a strong UDT mandate, determining the appropriate way to handle standard errors is not straightforward, and clustering at the state level in particular is likely to generate inaccurate standard errors. As an alternative means of assessing the statistical significance of our coefficient estimates, we apply a permutation test approach. For this approach, we re-estimate the regression models described in Equations 2.1 and 2.2 once for each donor state other than Louisiana under the counterfactual assumption that the state in question, and not Louisiana, introduced the strong UDT policy in 2008. We then compare the point estimate for the policy's effect in Louisiana with the distribution of the counterfactual point estimates for the other states. If the Louisiana point estimate is larger in absolute value than all or nearly all of the counterfactual point estimates, then this represents evidence against the null hypothesis that the strong UDT policy had no effect on Louisiana outcomes.

2.3.4 Synthetic control

We also apply to our analysis the synthetic control method developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010). This approach is analogous to the difference-in-differences approach, but rather than using as a comparison control the equally-weighted set of all untreated states, we generate a "synthetic" Louisiana comprised of a weighted subset of donor states, with weights chosen so as to match the observable characteristics of the control in the pre-treatment periods to those of Louisiana. We then compare the post-treatment trajectory for the outcome variable in Louisiana with the same trajectory for the synthetic control. If the trajectories for Louisiana and the control do match well in the pre-treatment periods but don't match well in the post-treatment periods, then this is evidence that the treatment did in fact have an impact on the measured outcome.

2.4 Results

2.4.1 Drug quantities

Results from estimating the DD model described in Equation 2.1 for drug quantities are shown in Table 2.1. The first outcome considered is log of morphine equivalent grams of legal opioids distributed per capita, aggregated within each state by quarter year. The second outcome is log of grams of amphetamine per capita, a non-opioid drug not affected by the UDT requirement. The coefficient estimates shown are for β, the coefficient on the $Post \times Treat$ term in equation 2.1. Each column represents a different set of regression controls. The coefficient estimates for opioids imply approximately a 20 percent reduction in opioid quantities distributed in Louisiana following the start of the UDT policy. Controlling for demographic patterns and several relevant policy controls, Louisiana amphetamine distribution decreased slightly relative to other states following the introduction of the UDT policy, though these estimates are not statistically significant at conventional levels. Amphetamine is not affected by the UDT policy, so we use observed changes in amphetamine

totals in our DDD approach, depicted later in this section, to control for underlying changes in the distribution of heavily regulated drugs overall.

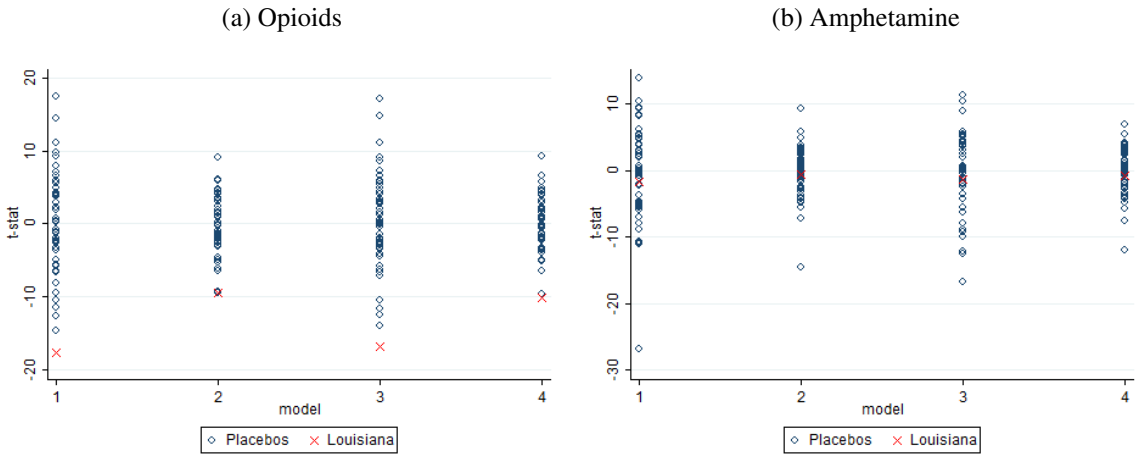
Table 2.1: DD results: Drug quantities

	(1)	(2)	(3)	(4)
Opioids	-0.222***	-0.231***	-0.219***	-0.227***
	(0.013)	(0.024)	(0.013)	(0.022)
Louisiana pre-treatment mean	0.313			
Amphetamine	-0.017*	-0.014	-0.014	-0.018
	(0.010)	(0.025)	(0.011)	(0.025)
Louisiana pre-treatment mean	0.013			
Demographic controls	No	Yes	No	Yes
State policy controls	No	No	Yes	Yes

Notes: Outcomes are log of morphine equivalent grams of opioids distributed per capita and log of grams of amphetamine distributed per capita. Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Permutation tests results corresponding to this DD approach are shown in Figure 2.2. The set of controls included for each model corresponds to the same-numbered regression model in Table 2.1. Depending on the specific model, the DD estimates for prescription opioid quantities in Louisiana lie somewhere in the two lowest estimates among all 50 states considered, a threshold which corresponds to at least a 5% significance level for a one-sided test or a 10% significance level for a two-sided test. Permutation tests of the DD estimates for Louisiana amphetamines do not yield statistically significant results.

Figure 2.2: Permutation tests: Drug quantities



We supplement this DD analysis with synthetic control estimation of the effects of Louisiana's UDT policy. In the DD framework, we are able to control for changes in other state policies that affect opioid outcomes by including policy dummies among our control variables. There is no such mechanism in the synthetic control framework for differencing out the effects of other state policies aside from UDT. Instead, we discard from the set of donor controls the five states which introduced either a must-access PDMP or legal access to medical marijuana during our sample period, two key policy types which have been linked to opioid outcomes in recent studies (Bradford and Bradford, 2016; Buchmueller and Carey, 2018; Powell et al., 2018).

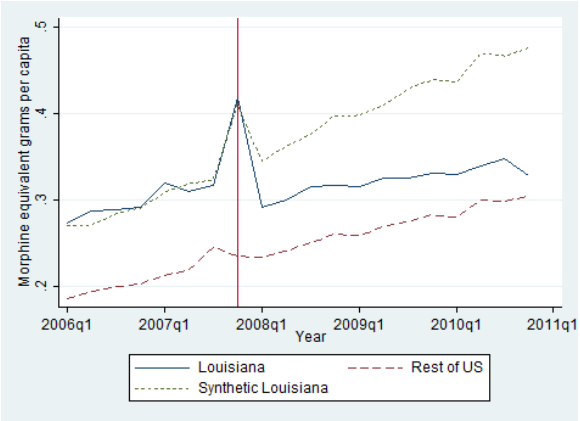
Synthetic control results for log of per capita opioid quantities are shown in Figure 2.3.[6] The estimated effect size for each quarter, equal to the difference between the amount for the synthetic control and the actual observed amount for Louisiana, is depicted in Figure 2.3. Panel (a) of this figure shows that the "synthetic Louisiana," generated from a weighted average of donor states, closely tracks with the true Louisiana opioid distribution data prior to 2008, after which time the observed Louisiana totals are considerably lower than the

[6]Synthetic control calculations were performed with Stata, using the synth_runner package created by Quistorff and Galiani (2017).

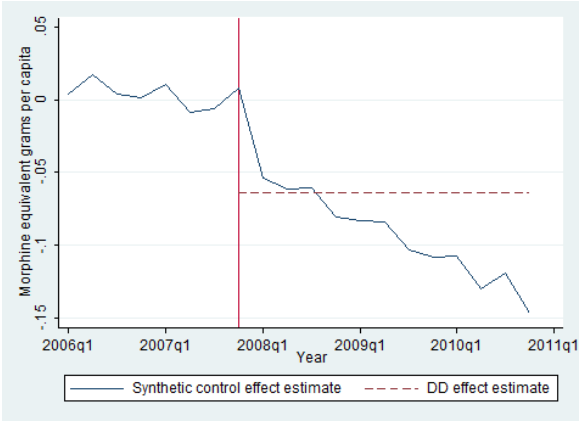
synthetic ones. Panel (b) shows the estimated effect of the policy for each quarter. The dotted line in panel (b) represents the effect size implied by the DD estimate shown in column 4 of Table 2.1.

Figure 2.3: Synthetic control: Opioids

(a) Distribution quantities



(b) Effect size estimates



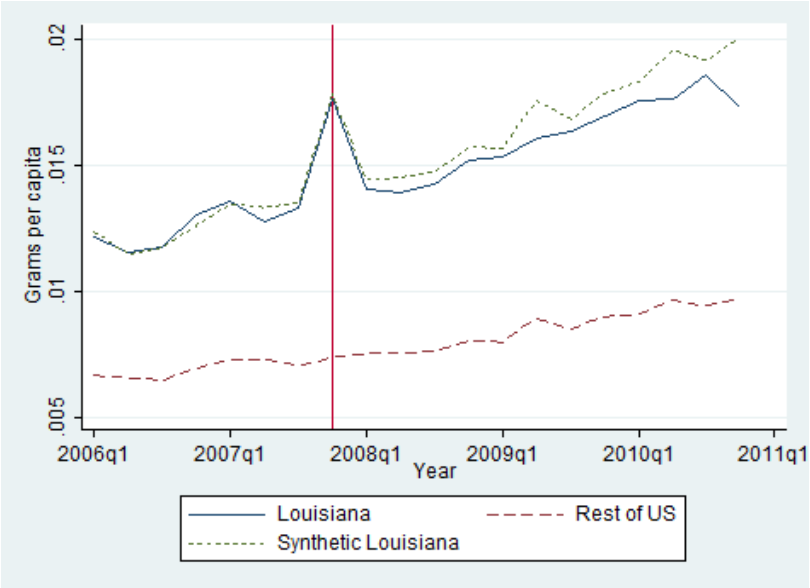
Notes: ARCOS quarterly data, Florida excluded.

An identifying assumption of the DD estimation approach is that the difference in the outcome variable between the treated and untreated states would have remained unchanged in the post-treatment period had the treatment not occurred. To examine the validity of this parallel trend assumption, we plot time series graphs of quarterly per-capita morphine equivalent totals of legally distributed opioids, for Louisiana and for all other US states plus DC (excepting Florida, for reasons which are discussed above). These respective lines, also shown in panel (a) of Figure 2.3, appear to support the parallel trends assumption. Although opioid distribution levels were higher in Louisiana than in the rest of the US, the two trends are very similar in the pretreatment period. The same is true of distribution quantities of the non-opioid drug amphetamine, as shown in Figure 2.4.

The ARCOS data exhibit a sharp upward spike in the final quarter of 2007 for a number of drug types and across many states. This spike appears to be an artifact of the way the data were gathered by the DEA in that particular quarter. To ensure that our results are not

driven by this anomalous quarter, we repeat our DD and DDD analysis of opioid quantities while omitting data for the final quarter of 2007. The results are shown in Appendix B; there is no qualitative change to our findings from this robustness check.

Figure 2.4: Amphetamine distribution quantities



Notes: ARCOS quarterly data, Florida excluded.

Statistical analysis of the synthetic control results for Louisiana is performed through placebo trials in which we counterfactually assign treatment to each untreated state and re-estimate the synthetic control model as though that state were treated, much like the permutation tests we apply in the DD framework above. For each post-treatment year, we compare the estimated effect size for Louisiana with the distribution of counterfactual estimated effect sizes for the control states. Each state is assigned a pseudo t-statistic equal to that state's effect size estimate scaled by the quality of the state's pretreatment match to Louisiana. The associated p-value for each Louisiana estimate is thus the proportion of all states' pseudo t-statistics which are at least as large as Louisiana's. For opioid quantities, the estimated effect for each post-treatment quarter has a p-value less than or equal to 0.05.

Next we perform the DD approach described in Equation 2.2, in which we compare the

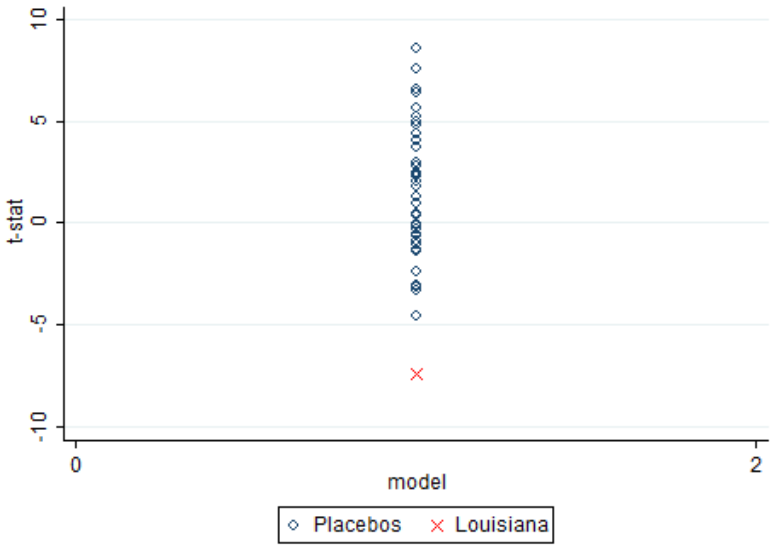
difference between opioid quantities and amphetamine quantities within Louisiana only, before versus after the 2008 introduction of the UDT policy. The results, shown in Table 2.2, imply a drop of approximately 15 percent in opioid distribution quantities in Louisiana following the introduction of its UDT requirement. Permutation testing, illustrated in Figure 2.5, shows that the t-statistic associated with Louisiana's post-2008 drop in opioid quantities relative to amphetamine is larger than that of any other state.

Table 2.2: DD results: Louisiana opioid vs. amphetamine quantities

	(1)
Post×Opioid	-0.166***
	(0.023)
Louisiana pre-treatment mean	0.313

Notes: Outcome is log of grams of drug type distributed per capita (opioids measured in morphine equivalent grams). Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Figure 2.5: Permutation tests: Opioid versus amphetamine quantities, Louisiana



Finally, we estimate the DDD model described in Equation 2.3 using variation in state,

policy timing, and type of drug. Here we compare the difference between opioid quantities and amphetamine quantities, in Louisiana versus untreated states, before and after the 2008 introduction of Louisiana's UDT policy. If there are post-2008 changes in the market for heavily-regulated drugs overall that are unique to Louisiana, then this DDD approach should account for such a circumstance (since amphetamine, like most of the opioids we consider, is a heavily-regulated Schedule II drug), and our DDD estimates should represent changes that are specific to opioids. Results from estimating the DDD model are shown in Table 2.3. The point estimate of -0.205 implies a drop of approximately 19 percent in Louisiana opioid quantities following introduction of the UDT policy.

Table 2.3: DDD results

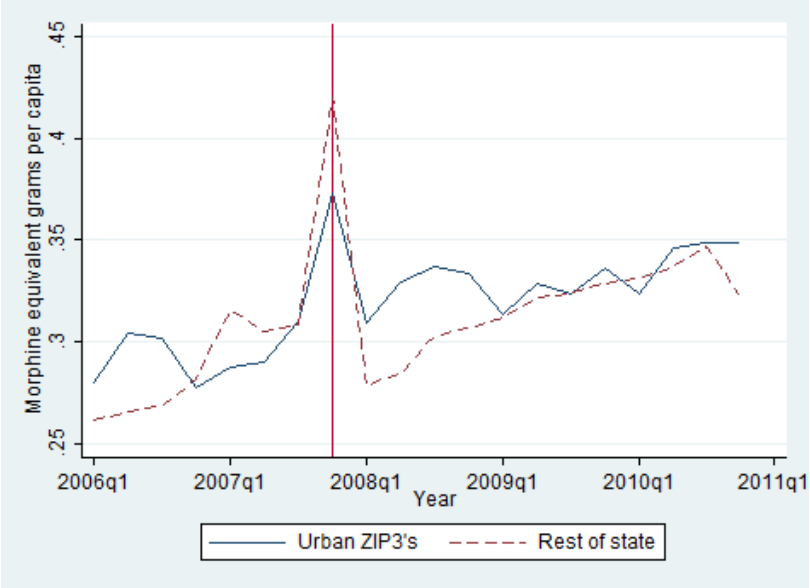
	(1)
Post×Opioid×Treat	-0.205***
	(0.019)

Notes: Outcome is log of grams of drug type distributed per capita (opioids measured in morphine equivalent grams). Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Louisiana's pain clinics are located entirely within a small number of urban areas. To examine whether the effects of the pain clinic policy exhibited spillovers into rural areas of the state that are farther from pain clinics, we plot time series of Louisiana opioid totals separately for the state's three ZIP3's that are centered on urban areas—specifically, the cities of New Orleans, Baton Rouge, and Shreveport—and the rest of the state. This graph, shown in Figure 2.6, suggests that spillovers to rural areas did occur, as opioid distribution to rural areas dropped at least as much as distribution to urban areas after the policy began. This pattern would be consistent with rural residents traveling to urban pain clinics but filling prescriptions at pharmacies nearer to their homes. The pattern would also be consistent with diffusion of standardized practices among physicians, as those not bound by the UDT

requirement mimic the use of UDT among pain clinic practitioners.[7]

Figure 2.6: Opioid distribution patterns by urbanicity, Louisiana

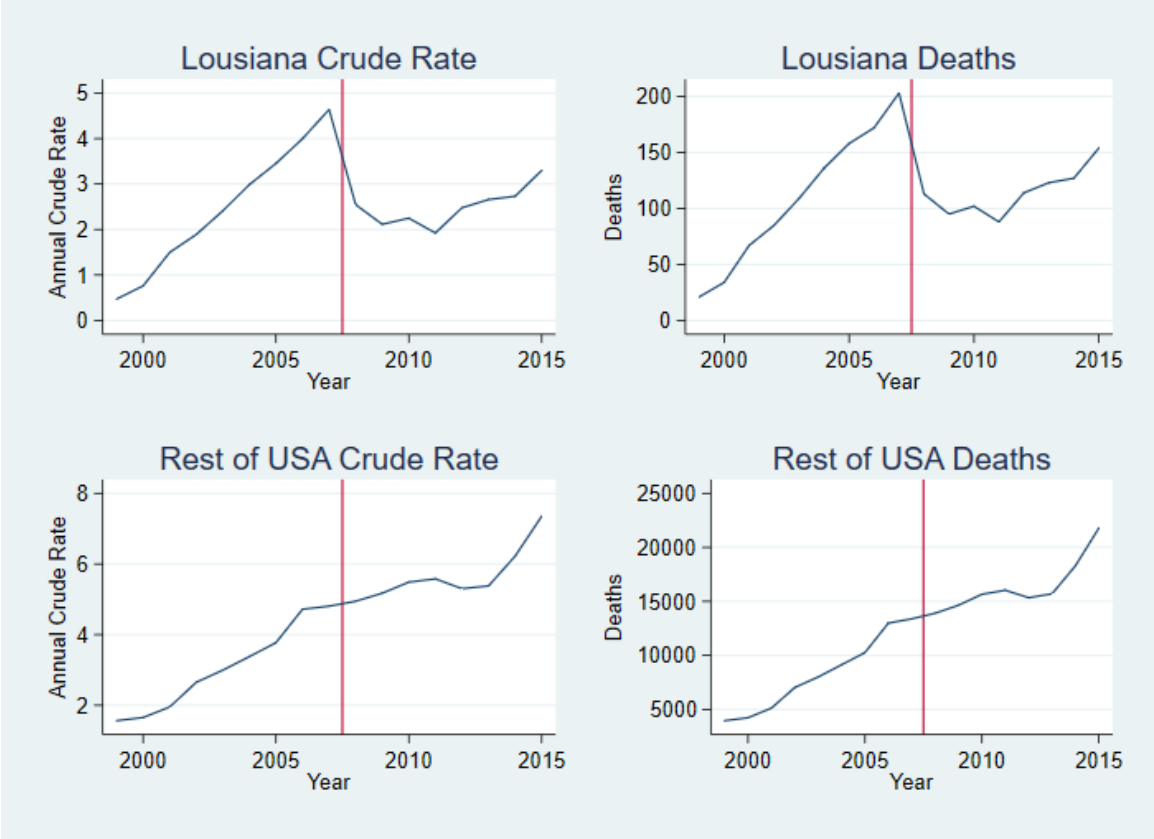


2.4.2 Opioid deaths

We next consider a measure that captures the tremendous human cost of the opioid epidemic: deaths related to opioid use. To examine the validity of the parallel trend assumption, we plot time series graphs of annual deaths from prescription opioids, both in counts and in rate per 100,000 people, for Louisiana and for all other US states plus DC, excepting Florida. These graphs, shown in Figure 2.7, appear to support the parallel trends assumption, as the pattern of opioid deaths in Louisiana prior to 2008 is not dissimilar to that in the rest of the country.

[7]Coleman et al. (1957) find that physicians may be influenced by what peers practice in uncertain situations, which in the context of Louisiana's policy could lead physicians not bound by random drug testing requirements to adopt the practice as well.

Figure 2.7: Deaths from prescription opioids



NVSS annual data, Florida excluded.

Results from estimating the DD model described in Equation 2.1 for mortality rates are shown in Table 2.4. The outcome considered is log of the death rate associated with a particular cause per 100,000 population.[8] Each of the first two rows shows regressions for a different drug type indicated in the death record: prescription opioids, all opioids (both prescription and illicit combined), and all illicit drugs. The outcome measured in the final row is the overall death rate from all causes. Each column represents a different set of regression controls.

[8]Ruhm (2017) presents statistical evidence that death records systematically undercount the number of deaths associated with specific drugs, including opioids. The mean death rates we present here for each drug type thus understate the true death rates. We perform regressions for logged mortality rates. The resulting estimates of proportional changes in mortality rate will be unbiased by underreporting of drug deaths, so long as the policies in question do not differentially alter states' reporting practices.

Table 2.4: DD results: Mortality rates

	(1)	(2)	(3)	(4)
Rx opioids				
Post×Treat	-0.387**	-0.614***	-0.426***	-0.600***
	(0.173)	(0.075)	(0.139)	(0.074)
Louisiana pre-treatment mean	2.456			
All opioids				
Post×Treat	-0.192	-0.443***	-0.233**	-0.422***
	(0.127)	(0.078)	(0.092)	(0.081)
Louisiana pre-treatment mean	2.552			
Illicit drugs				
Post×Treat	0.068	-0.096	0.012	-0.087
	(0.048)	(0.092)	(0.063)	(0.084)
Louisiana pre-treatment mean	2.044			
All causes				
Post×Treat	0.000	-0.009*	-0.006	-0.009**
	(0.009)	(0.005)	(0.008)	(0.004)
Louisiana pre-treatment mean	932.948			
Demographic controls	No	Yes	No	Yes
State policy controls	No	No	Yes	Yes

Notes: Data used is CDC Wonder annual state aggregates from 1999 to 2015. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

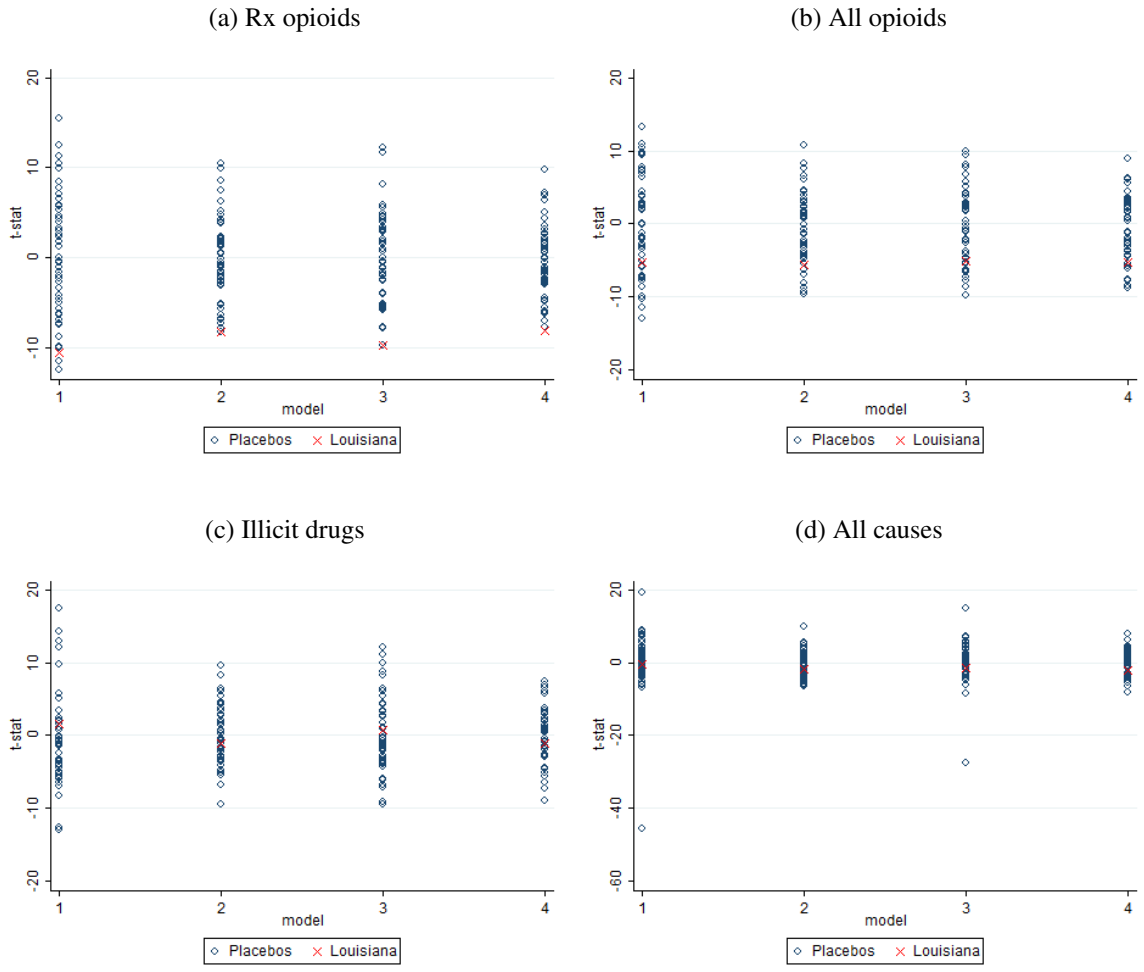
The regressions from column 4 of Table 2.4, which include a full vector of demographic and policy controls, imply that Louisiana's UDT requirement led to reductions of approximately 45 and 34 percent in deaths from prescription opioids and all opioids, respectively. Permutation tests support the statistical significance of the estimated change in prescription opioid deaths.

We find no statistically significant evidence of a change to deaths from illicit drugs. An opioid-addicted individual may view both heroin and illicitly manufactured fentanyl as possible substitutes for prescription opioids. Policies that erect barriers to the legal acquisition of opioids by addicts can thus have the unintended consequence of increasing illicit drug use while decreasing legal drug use. We find no evidence that Louisiana's pain clinic policy caused such an increase in abuse of illicit drugs.

Case and Deaton (2015) demonstrate that the opioid epidemic has had such severe

effects on mortality that overall mortality rates have actually worsened among certain US demographic groups in the early 21st century. With this pattern in mind, we estimate the effect of Louisiana's UDT law on the all-cause mortality rate. When controlling for a full vector of demographic and policy controls, we find that this effect was approximately a one percent reduction in mortality. This suggests that the pain clinic regulation had a measurable impact on the overall health of Louisiana residents. However, permutation testing does not indicate that Louisiana's reduction in all-cause mortality after 2007 is unique among the set of states. Closer examination of patterns in the all-cause mortality rate is warranted to determine whether estimated effect sizes are biased due to migration patterns or other omitted variables; we leave this question for future work.

Figure 2.8: Permutation tests: Mortality rates



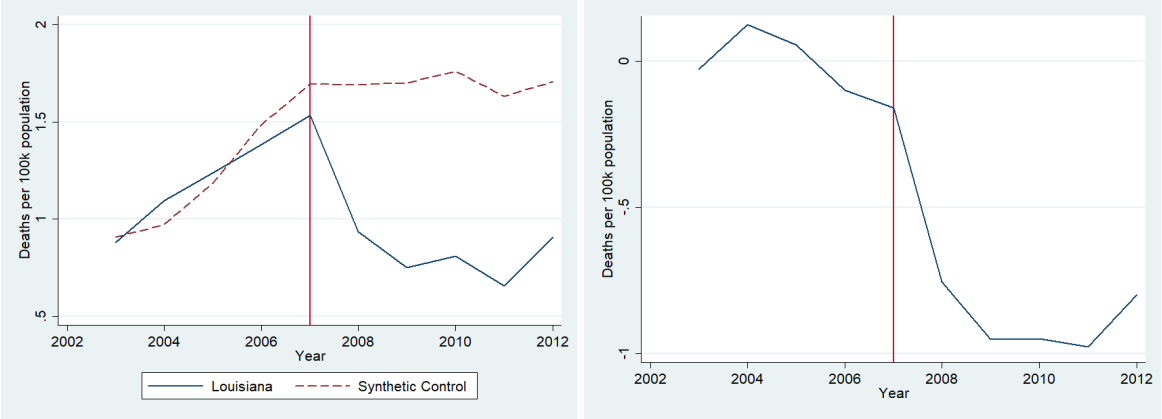
We next perform synthetic control estimation of the effect of Louisiana's UDT policy on opioid deaths. We discard from the set of donor controls the five states which introduced either a must-access PDMP or legal access to medical marijuana during our sample period, just as we do for our synthetic control analysis of drug quantities.

Synthetic control results for log of opioid death rates are shown in Figures 2.9 and 2.10. For death rates from both prescription opioids and all opioids, the pre-treatment fit for synthetic Louisiana to actual Louisiana is very good. Post-treatment, there is a sharp and persistent drop in Louisiana's opioid death rate, while the constructed synthetic control shows no such drop and instead holds steady near to pre-treatment levels. The difference in

log death rate between Louisiana and the synthetic control in each year is shown in Figures 2.9b and 2.10b. In each individual post-treatment period for both prescription opioids and all opioids, the Louisiana estimate has a placebo test p-value below five percent. The synthetic control results support the findings of the DD model that Louisiana's 2008 pain clinic policy led to a large and statistically significant decrease in opioid overdose deaths.

Figure 2.9: Synthetic control: Prescription opioid deaths

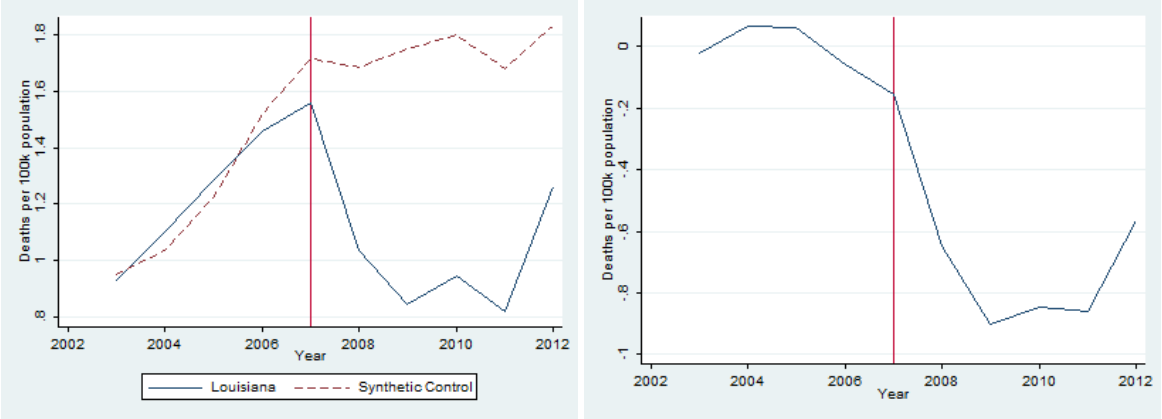
(a) Lousisiana vs. control (b) Effect size estimates



Notes: Log of deaths per 100k population. Annual data from CDC Wonder, Florida excluded.

Figure 2.10: Synthetic control: All opioid deaths

(a) Lousisiana vs. control (b) Effect size estimates



Notes: Log of deaths per 100k population. Annual data from CDC Wonder, Florida excluded.

Finally, we repeat the DD analysis to examine changes in the percentage of deaths due to each drug-related cause. The outcome in each case is simply the annual number of deaths due to the specified cause divided by the total annual number of deaths from all causes, multiplied by 100. Results are displayed in Table 2.5. Results in column 4, which include demographic and policy controls, indicate drops of approximately 0.4 percentage points in the proportion of deaths due to each prescription opioids and all opioids. Permutation tests strongly support the significance of the change for prescription opioid deaths regardless of the set of regression controls used, but this is not the case for deaths from all opioids. We find no evidence of a significant change in the percentage of deaths that are related to illicit drugs.

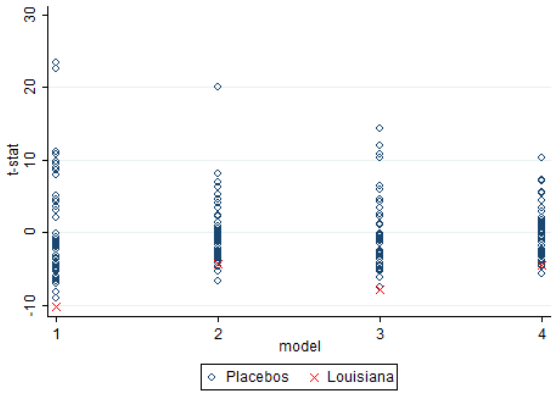
Table 2.5: DD results: Percentage of all deaths

	(1)	(2)	(3)	(4)
Rx opioids				
Post×Treat	-0.263*	-0.438***	-0.265**	-0.396***
	(0.138)	(0.100)	(0.101)	(0.088)
Louisiana pre-treatment mean	0.263			
All opioids				
Post×Treat	-0.279**	-0.439***	-0.275***	-0.381***
	(0.137)	(0.107)	(0.085)	(0.092)
Louisiana pre-treatment mean	0.273			
Illicit drugs				
Post×Treat	0.078***	-0.054	-0.082	-0.032
	(0.025)	(0.038)	(0.050)	(0.033)
Louisiana pre-treatment mean	0.219			
Demographic controls	No	Yes	No	Yes
State policy controls	No	No	Yes	Yes

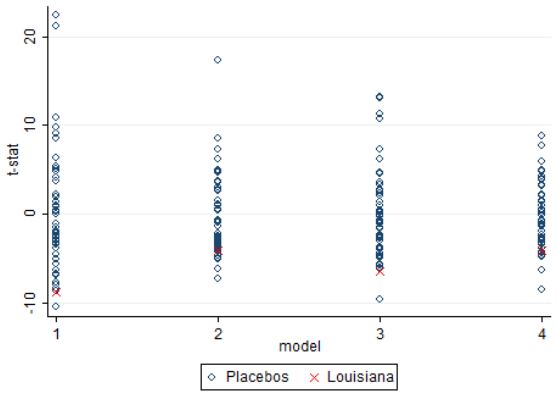
Notes: Data used is CDC Wonder annual state aggregates from 1999 to 2015. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Figure 2.11: Permutation tests: Percentage of all deaths

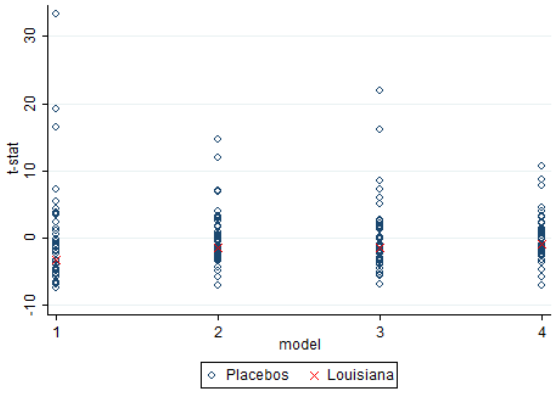
(a) Rx opioids



(b) All opioids



(c) Illicit drugs



2.5 Discussion and Conclusion

Public awareness of the US opioid epidemic has improved dramatically since 2010, when the first federal efforts were undertaken to crack down on practitioners who were over-prescribing the pain drugs. Even as government task forces and celebrity overdose deaths have generated extensive media reporting of the addiction problem, there remains a dearth of evidence-based policy options available for reducing rates of prescription opioid abuse and misuse. Examining the effects of a 2008 Louisiana law which required pain clinics to carefully monitor patients' behavior through urine drug testing and consultation

of a prescription database, we find evidence that it led to large reductions in the distribution of prescription opioids and the rate of overdose deaths. Based on clinical evidence, the US Centers for Disease Control and Prevention have recommended that prescribers use drug testing as a standard part of their practices in treating chronic pain (CDC, 2016a). Our findings suggest that laws mandating the use of such practices have the potential to greatly reduce rates of opioid overdose in states where no such law is in place. Further work is warranted to examine drug testing patterns that result from UDT requirements.[9] Urine testing rates in multiple states have increased in recent years as medical professionals have recognized the profitability of the tests, a development which has earned media scrutiny (Schulte, 2018; Schulte and Lucas, 2017).

Because substitutes for legal opioids exist in the forms of heroin and illicitly manufactured fentanyl, there is a reasonable concern that any given policy effort to reduce access to legal opioids may increase rates of abuse of legal drugs. Alpert et al. (2018) find just such an effect following the 2010 abuse-deterrent reformulation of the popular opioid product OxyContin, and Mallatt (2017) finds that PDMP's have the unintended consequence of increasing heroin crime rates. Further examination of Louisiana's UDT policy is warranted to determine whether it induces this type of substitution behavior, particularly with regard to crime rates. Our analysis of mortality rates shows no evidence that this Louisiana law regulating legal drugs increased the rate of deaths from illicit drugs.

PDMP's are the most prominent policies aimed at curtailing abuse of prescription drugs; they have been rolled out in nearly every US state as of 2018. Buchmueller and Carey (2018) find that legally requiring practitioners to access a PDMP leads to a 2.4 percent decline in the share of Medicare Part D enrollees who take opioids, but they find no statistical evidence of an associated drop in overdose rates. Patrick et al. (2016) find that

[9] In Appendix Figure B.3, we present preliminary evidence using aggregate measures of drug test claims among Medicare Part B enrollees. These data show a large increase in Louisiana UDT claims that begins several years after the introduction of Louisiana's UDT law. This Medicare claims data set extends back only to 2005, which limits our ability to examine whether the parallel trends assumption holds. As a large majority of Medicare recipients are over age 65, they are not likely representative of the US population overall.

PDMP monitoring of four or more drug schedules is associated with 0.55 fewer opioid overdose deaths per 100,000 residents and that updating PDMP data at least weekly is associated with a reduction of 0.82. Our results indicate that Louisiana's unique pain clinic law, which included a strong drug testing mandate as well as a PDMP access requirement, led to a drop in prescription opioid mortality of nearly half, much larger than the effects of PDMP-only policies found in other studies, underscoring the untapped potential of strong UDT requirements to battle the opioid epidemic.

Chapter 3

What About the Children? How Opioid Use Relates to Child Well-Being

with Lindsey R. Bullinger

3.1 Introduction

Prescription opioid pain relievers are effective therapies, and use of prescription opioid medications has increased greatly in the U.S. over the past decade. These drugs are highly addictive, however, which can lead to dependence and abuse. Indeed, deaths due to drug overdose have more than doubled between 2000 and 2014 (Rudd et al., 2016), exceeding deaths from motor vehicle accidents and firearms since 2009 (Paulozzi, 2012), driven largely by overdose deaths from prescription opioid pain relievers (CDC, 2016b).

Although the literature on opioid users is growing, whether and how children are affected by the increases in opioid drug use and misuse is not well understood. Direct exposure to opioids among children has been documented through increases in the past decade in the incidence of neonatal abstinence syndrome (Brown et al., 2016; Patrick et al., 2012) and child hospitalizations due to opioid poisoning (Gaither et al., 2016). But children may experience indirect effects of opioid abuse without ingesting the drugs. Bullinger and Wing (2017) find opioid dependency has increased among adults living with children. Specifically, 40 percent of new opioid-dependent adults live in households with children. Although there is no existing evidence on the indirect effects of opioid abuse on children, other types of substance abuse might serve as a model. For example, crackdowns on the production of illicit methamphetamine led to reductions in the number of foster care cases and in rates of child neglect and abuse (Cunningham and Finlay, 2013). Together, these findings imply that the drastic increase in rates of opioid misuse and abuse since 2000 might also have an impact on child welfare.

Clearly, increases in rates of opioid overdose have the capacity to negatively affect children if caregivers are among those abusing the drugs. However, it is unclear what relationship we should expect *a priori* between child well-being and distribution rates of legal opioids for medical use. The addictive nature of these drugs may lead to substandard parenting or even the death of a caregiver. On the other hand, recent research has uncovered evidence suggesting that decreases in opioid prescribing (Ray et al., 2017) and an intervention to prevent the abuse of the prescription opioid OxyContin (Alpert et al., 2018) have had the unintended consequence of increasing rates of abuse of illegal drugs like heroin and illicitly manufactured fentanyl. Furthermore, the opioid epidemic has affected a different demographic than have other addiction epidemics (Ho, 2017; Hollingsworth et al., 2017; Martins et al., 2017), and so opioid abuse patterns may have different implications for children than do other types of substance abuse.

To comprehensively study the relationship between opioid use and child well-being, we use data from several sources including child maltreatment reports, records of foster care entrance, retail opioid distribution data, opioid-related emergency department visits, and records of drug-induced mortality. Our analysis (at both the state and county levels) controls for time-varying characteristics such as macroeconomic conditions, access to medical care, sociodemographic composition, social safety net program participation, time-invariant differences across states or counties, temporal changes that occur nationwide, and underlying secular trends specific to each state or county.

Results suggest that the way we measure opioid patterns matters. Measures that reflect the human costs of the opioid epidemic—opioid-related emergency department visits and drug-induced mortality—are associated with higher rates of foster care entrance, demonstrating how the opioid epidemic has had an indirect and devastating effect on children. In contrast, a higher rate of legal opioid distribution is associated with a lower rate of reported child maltreatment and foster care entrance. This relationship is especially pronounced among maltreatment reports attributed to physical abuse of all children ages 0-17 and those

attributed to caretaker absence/inability among all ages. These results suggest that while the consequences of opioid abuse negatively affect children, there is also reason to suspect that decreases in access to legal opioids have led caregivers to instead abuse illegal drugs, to the detriment of child well-being.

3.2 Data

We combine data from a variety of sources to construct state and county-level panel datasets. Not all data are available at the same geographic level for the same time period, so we merge each measure of child well-being with each measure of opioid use separately.

3.2.1 Measures of Child Well-being

Child maltreatment reports come from the California Child Welfare Indicators Project (CCWIP). These data include the number of child maltreatment allegations in each county and year from 2002-2015, by maltreatment type, and by age of the alleged victim. Children may receive more than one report, and each report can contain multiple allegations of child maltreatment. Therefore, there may be duplicate child counts. We construct a total report rate, which is the total number of reports made to the California Department of Social Services (CDSS) in a particular county-year scaled by the number of children in the county. We also create maltreatment-specific rates for severe neglect, general neglect, physical abuse, at-risk of maltreatment (sibling abused), and caretaker absence or inability.

The Administration for Children and Families (ACF) administers the Adoption and Foster Care Analysis and Reporting System (AFCARS), which provides case-level data on every child in the foster care system. Since we are concerned with how changes in opioid use affects the flow of children into the foster care system and the demand on the system, from 2003-2015, we restrict the sample to children entering the foster care system, rather than the total number of children in the foster care system during the time period. Children can experience multiple episodes of removal. We allow for multiple episodes treating each

case separately. We construct a foster-care entrance rate for each state, where the numerator is the total number of entrances in a state during a particular quarter and the denominator is the number of children in the state. We also create rates for each of the following specific reasons for removal: parental substance abuse, neglect, and parental death.

3.2.2 Measures of Opioid Use

To track changes in the quantity of legal opioids purchased, we use data gathered between 2002 and 2015 by the US Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS). The ARCOS database tracks the quarterly quantity of certain drugs distributed to retailers—including pharmacies as well as hospitals and practitioners—in each ZIP3 (a cluster of ZIP codes that share the same three-digit prefix). In particular, we consider the quantities distributed for each of seven of the most commonly prescribed opioid analgesics: codeine, fentanyl, hydrocodone, hydromorphone, meperidine, morphine, and oxycodone. For the purposes of our analysis we exclude methadone and buprenorphine, two types of opioids that are used in the treatment of substance use disorders, as they are inconsistently tracked by ARCOS over our sample period. Since different opioid types have differing levels of per-milligram potency, we convert the distributed quantity of each drug in grams into a morphine-equivalent quantity, using multipliers that have been established in the medical literature (Gammaitoni et al., 2003; Paulozzi et al., 2011), and sum across all opioid types to generate an aggregate quantity. We also use a population-weighted crosswalk to convert ARCOS ZIP3 drug quantities into county-level totals to facilitate our analysis of child maltreatment data gathered at the county level. Finally, we scale drug quantities by population estimates from the National Center for Health Statistics to obtain a rate.

Though the ARCOS data quantify the supply of legal opioids in circulation, they do not necessarily provide an appropriate measure of the scope of the opioid epidemic's public health threat. One way in which we capture opioid misuse and abuse is by measur-

ing the rate of opioid-related emergency department visits per capita, which come from the Agency for Healthcare Research and Quality (AHRQ) Healthcare Cost and Utilization Project (HCUP) State Emergency Department Databases (SEDD), available through HCUP FastStats. These data are derived from uniform medical billings at the ED visit level, but are limited to visits that did not result in an inpatient stay for patients treated in community hospital-owned EDs. We count each visit separately, and may include the same person multiple times if the individual visits the ED multiple times per year. Since we are interested in the parent and caretaker population, we limit to visits among individuals aged 25-44. Not all states are included in the data; 32 states are included, and among those states, each state is in the data for various years. We opt for increased data rather than a balanced panel, leaving us with 1,109 state-quarter observations. Table C.2 indicates the years in which each state was included.

Data on drug mortality come from National Center for Health Statistics Vital Statistics Multiple Cause-of-Death files, which represent information gathered from death certificates. Because of underreporting of deaths that are specifically related to opioids (see Ruhm, 2017), and in light of evidence that opioid addiction has led to increased use of illicit drugs such as heroin (Alpert et al., 2018), we consider totals of all drug-related deaths. We consider age-adjusted rates of deaths in the state-level analyses, and crude drug-induced death rates in the county-level analyses. Sensitivity checks suggest this difference is inconsequential.

3.2.3 Control Variables

Other variables in our analyses include several characteristics that change over time and may have an independent relationship with both opioid use and child well-being. We control for a variety of socioeconomic and demographic characteristics which could confound our estimates. These variables include unemployment rate, poverty rate, percent of population receiving Supplemental Nutrition Assistance Program (SNAP) nutritional benefits,

median income, percent of the population that is under age 18, and percent White, Black, Asian, and Hispanic (all measured annually). The supply of legal opioids in a particular locale could fluctuate for a number of reasons, including changes in the degree of access to medical care generally. To distinguish between changes in the prevalence of pharmaceutical opioids and changes in access to medical care generally, we control for yearly observations of the number of patient care physicians per capita in each state and the number of community mental health centers per capita. These data come from the Area Health Resource File, with the exception of patient care physician data which come from the American Medical Association.

3.3 Estimation Approach

We perform regression analyses to estimate the relationship between measures of opioid use or misuse/abuse and rates of adverse experiences among children. Estimated regressions take the form

$$C_{it} = aO_{it} + \boldsymbol{b}\boldsymbol{X}_{it} + \gamma_i + \tau_t + \delta\gamma_i \times TREND + \varepsilon_{it}. \tag{3.1}$$

Here C_{it} represents the rate of reports of a particular type of child adverse event in geographic area (state or county) i and time period t. O_{it} is a selected measure of opioid use (rate of morphine-equivalent grams of legally distributed opioids; rate of opioid-related ED visits; drug-induced death rate). $\boldsymbol{X}_{it}$ is a vector of time-varying socioeconomic and demographic controls, which are described individually above. γ_i and τ_t are territory and time period fixed effects, respectively, which control for unobserved heterogeneity in child welfare conditions across geographical areas and between time periods. We also include quadratic territory-specific trends, $\gamma_i \times TREND$. Results are not sensitive to the omission of territory-specific trends nor to the application of linear or cubic trends rather than quadratic. ε_{it} is a random error term.

For both the rate of reported child maltreatment and foster care entrance, we estimate results for the full sample of children under age 18. We also examine potential heterogeneity in the relationship between opioid use and child well-being across age groups by performing subsample analyses for children in each of four age bins (0-1; 2-5; 6-12; and 13-17), where the denominator for each of these measures is the number of children in each corresponding age group. We further consider heterogeneity across specific types of maltreatment and specific reasons for foster care entry. In each regression, standard errors are clustered at the territory level, and analytic weights are assigned to each state or county based on population.

3.4 Results

3.4.1 Supply of Legal Opioids

We begin by identifying the relationship between the supply of legal pharmaceutical opioids and child well-being in Table 3.1. Panel A represents all foster care entrances, regardless of the reason for removal. Results suggest that states with greater quantities of legal opioids in distribution have lower rates of foster care entrance. Specifically, one additional morphine-equivalent gram per capita distributed is associated with 23.5 fewer entrances per 100,000 children, though this relationship is marginally statistically significant. Relative to the mean during this time period of about 109 entrances per 100,000 children, this change represents about 20 percent. The negative relationship appears to persist for foster care entrances due to parental drug abuse, neglect, and death of a parent. The relationship is larger among older children, between ages 13 and 17.

The negative relationship between the quantity of legal opioids and adverse events among children is even stronger when examining rates of child maltreatment, an outcome that does not necessarily lead to removal from the home (Table 3.2). When looking at county-level rates of child maltreatment in California, an additional morphine-equivalent

Table 3.1: Legal Opioid Distribution and Foster Care Entrance Rate, Nationwide, 2003-2015

	FC Entrance Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
PANEL A: REMOVAL REASON: ALL					
Morphine-Equivalent Grams Distributed Per 10,000 Capita	-23.52*	-48.33	-30.09	-21.40	-11.75*
	(13.85)	(32.81)	(18.86)	(13.79)	(6.82)
Mean Y	108.90	220.13	108.47	75.41	111.59
PANEL B: REMOVAL REASON: PARENTAL DRUG ABUSE					
Morphine-Equivalent Grams Distributed Per 10,000 Capita	-11.69	-33.17	-13.53	-9.11	-4.70
	(7.88)	(20.94)	(10.38)	(6.06)	(4.04)
Mean Y	25.26	78.23	31.69	17.96	10.10
PANEL C: REMOVAL REASON: NEGLECT					
Morphine-Equivalent Grams Distributed Per 10,000 Capita	-16.90*	-37.83	-22.41	-13.00	-11.50***
	(9.53)	(23.26)	(14.38)	(8.94)	(4.19)
Mean Y	56.60	134.89	70.67	44.05	33.22
PANEL D: REMOVAL REASON: PARENTAL DEATH					
Morphine-Equivalent Grams Distributed Per 10,000 Capita	0.18	0.07	-0.13	-0.06	0.87**
	(0.18)	(0.40)	(0.16)	(0.17)	(0.39)
Mean Y	0.70	0.58	0.50	0.58	1.08

Notes: Foster care data are from AFCARS. Opioid distribution data are from ARCOS. Sample consists of all 50 states plus D.C., measured quarterly (N=2,601). Robust SE clustered at state-level in parentheses. Models control for unemployment rate, poverty rate, median income, percent SNAP beneficiaries, number of primary care physicians per capita, number of community mental health facilities per capita, percent white, black, Asian, and Hispanic, percent child population, state FE, year FE, state-specific quadratic time trends, and are weighted by state population. * p<0.10, **p<0.05, ***p<0.01

gram per capita in supply is associated with 195 fewer reports of maltreatment per 100,000 children (roughly 37 percent). This relationship is driven largely by changes in rates of reports of physical abuse and of caretaker absence or inability to care for a child, and the relationship is consistent across all age groups. Together, results from Tables 3.1 and 3.2 suggest that reductions in access to legal opioids have had negative consequences for children which have more than offset any benefits achieved by reducing exposure to the dangers of these prescription drugs. Taken at face value, this set of findings seems to indicate a beneficial effect of access to opioids for child well-being. In the discussion section below, we consider our findings in the context of related research and explain why this face value interpretation is likely misleading.

3.4.2 Opioid-Related Emergency Department Visits

Relative to drug distribution rates, opioid-related emergency department visits better capture the addictive nature of opioids and some of the primary concerns of increased opioid use. Table 3.3 provides suggestive evidence that an increase in opioid-related visits to emergency departments is associated with greater foster care entry, though these estimates are mostly statistically indistinguishable from zero. The exception is foster care entrance due to neglect, which may be indicative of non-specific coding by case workers in cases related to drug abuse. Specifically, one more opioid-related ED visits per 100,000 individuals is associated with about 3 more children out of 100,000 entering the foster care system due to neglect, which is about 5 percent more. We do not have ED visit data at the county-level, so we cannot empirically test the relationship between opioid-related ED visits and child maltreatment at the county-level in California.

3.4.3 Drug-Induced Mortality

The most extreme consequence of opioid use is death from overdose. Tables 3.4 and 3.5 indicate that increases in the rate of negative outcomes for opioid users has troubling

Table 3.2: Legal Opioid Distribution and Child Maltreatment Report Rate in California, 2002-2015

	Report Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
TOTAL REPORTS					
Morphine-Equivalent Grams Distributed Per Capita	-195.43**	-149.67**	-283.22**	-275.57*	-139.23**
	(93.56)	(65.26)	(132.36)	(140.94)	(62.87)
Mean Y	532.64	395.29	738.67	765.13	346.11
SEVERE NEGLECT					
Morphine-Equivalent Grams Distributed Per Capita	-23.94	-55.89	-34.91	-26.19	-7.83
	(15.95)	(34.10)	(24.25)	(17.62)	(5.03)
Mean Y	8.74	20.97	12.78	7.96	2.65
GENERAL NEGLECT					
Morphine-Equivalent Grams Distributed Per Capita	-23.48	11.20	-27.29	-77.39	-10.09
	(56.07)	(63.54)	(86.02)	(77.93)	(34.87)
Mean Y	224.09	217.36	336.56	308.97	123.18
PHYSICAL					
Morphine-Equivalent Grams Distributed Per Capita	-99.79***	-71.46***	-155.23***	-129.53**	-64.20***
	(36.26)	(26.51)	(57.09)	(53.75)	(21.27)
Mean Y	100.20	36.18	117.81	160.95	77.46
AT RISK (SIBLING ABUSED)					
Morphine-Equivalent Grams Distributed Per Capita	-11.36	-19.50	-24.53	-5.11	-13.57
	(36.09)	(24.21)	(49.15)	(56.67)	(22.72)
Mean Y	65.74	40.08	92.83	102.48	40.04
CARETAKER ABSENCE					
Morphine-Equivalent Grams Distributed Per Capita	-20.17***	-24.66***	-33.41***	-22.99**	-11.45**
	(7.38)	(8.03)	(10.69)	(10.92)	(5.62)
Mean Y	15.07	12.82	16.89	16.32	14.44

Notes: Child maltreatment data are from CCWIP. Opioid distribution data are from ARCOS. Sample consists of 58 counties, measured annually, in California (n=812). Robust SE clustered at county-level in parentheses. Models control for unemployment rate, number of community mental health facilities per capita, percent of county receiving SNAP benefits, percent white, black, Asian, and Hispanic, percent child population, county FE, year FE, county-specific quadratic time trends, and are weighted by county population. * p<0.10, **p<0.05, ***p<0.01

Table 3.3: Opioid-Related Emergency Department Visits and Foster Care Entrance Rate, 2005-2014

	FC Entrance Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
PANEL A: REMOVAL REASON: ALL					
Opioid-Related Emergency Department Visits Per 100,000 Capita	2.23	5.43	3.32	2.12	0.71
	(1.82)	(5.37)	(2.61)	(1.66)	(2.20)
Mean Y	108.90	220.13	108.47	75.41	111.59
PANEL B: REMOVAL REASON: PARENTAL DRUG ABUSE					
Opioid-Related Emergency Department Visits Per 100,000 Capita	0.17	1.37	0.74	-0.32	0.07
	(1.40)	(4.22)	(2.13)	(1.14)	(0.50)
Mean Y	25.26	78.23	31.69	17.96	10.10
PANEL C: REMOVAL REASON: NEGLECT					
Opioid-Related Emergency Department Visits Per 100,000 Capita	2.88*	6.26	3.13	2.70**	1.60
	(1.46)	(4.16)	(2.09)	(1.12)	(1.03)
Mean Y	56.60	134.89	70.67	44.05	33.22
PANEL D: REMOVAL REASON: PARENTAL DEATH					
Opioid-Related Emergency Department Visits Per 100,000 Capita	-0.00	0.12	-0.03	-0.04	0.02
	(0.05)	(0.08)	(0.05)	(0.05)	(0.09)
Mean Y	0.70	0.58	0.50	0.58	1.08

Notes: Foster care data are from AFCARS. Opioid-related ED visit data are from HCUP FastStats. Sample consists of an unbalanced panel of 22-32 states plus, measured quarterly (n=1,109). Robust SE clustered at state-level in parentheses. Models control for unemployment rate, poverty rate, median income, percent SNAP beneficiaries, number of primary care physicians per capita, number of community mental health facilities per capita, percent white, black, Asian, and Hispanic, percent child population, state FE, year FE, state-specific quadratic time trends, and are weighted by state population.
* p<0.10, **p<0.05, ***p<0.01

effects on children as well. Areas with higher rates of drug-induced mortality have higher rates of foster care entrance, especially among young children between ages 0 and 12. In particular, one additional death as a result of drug use is associated with an additional 24 out of 100,000 children entering the foster care system, reflecting increases of about 6 percent. Though evidence of the relationship between drug-induced mortality and child maltreatment is inconclusive, coefficient estimates suggest a positive relationship (Table 3.5). Our child maltreatment analysis is limited to the 32 most populous counties in California, due to data access limitations on drug mortality. Since rural counties have experienced larger increases in drug poisoning mortality than urban counties (Paulozzi and Xi, 2008), this sample reduction may be biasing our results toward no consistent and significant relationship.

3.5 Sensitivity Checks: PDMP Policies

Our main results suggest the way in which we measure opioid use matters. We find a negative relationship between the supply of legal opioids and child maltreatment and foster care entrance. This relationship is merely correlational, however, and given the positive relationship found between opioid-related emergency department visits and drug-induced death, may be the result of endogeneity of legal opioid distribution. For example, areas with greater access to opioids might also have higher income and socioeconomic status, or greater access to medical care more generally. Although we control for several observable demographic factors, access to opioids may change over time for a number of unobserved reasons, and there may remain omitted variables.

To more exogenously measure access to opioids, we use variation in state-level public policies. One policy approach that states have adopted to reduce the problems of overprescribing and of diversion of prescription opioids into the black market is the implementation of prescription drug monitoring programs (PDMPs). PDMPs place requirements on practitioners and/or pharmacists to consult a central database when providing opioid pre-

Table 3.4: Drug-Induced Death Rate and Foster Care Entrance Rate, 2003-2015

	FC Entrance Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
PANEL A: REMOVAL REASON: ALL					
Annual Age-Adjusted Drug-Induced Death Rate Per 1,000 Capita	24.31*	62.13**	35.08***	17.96*	10.91
	(12.19)	(27.35)	(12.64)	(8.95)	(14.27)
Mean Y	427.56	864.20	425.91	296.11	438.05
PANEL B: REMOVAL REASON: PARENTAL DRUG ABUSE					
Annual Age-Adjusted Drug-Induced Death Rate Per 1,000 Capita	3.03	15.22	5.80	2.07	-0.97
	(6.86)	(21.25)	(8.44)	(5.84)	(4.04)
Mean Y	99.20	307.21	124.45	70.54	39.63
PANEL C: REMOVAL REASON: NEGLECT					
Annual Age-Adjusted Drug-Induced Death Rate Per 1,000 Capita	7.97	19.80	18.25*	8.13	-2.96
	(7.94)	(21.57)	(10.17)	(5.74)	(6.12)
Mean Y	222.25	529.56	277.48	172.99	130.44
PANEL D: REMOVAL REASON: PARENTAL DEATH					
Annual Age-Adjusted Drug-Induced Death Rate Per 1,000 Capita	0.37*	0.05	0.20	0.20	0.78
	(0.21)	(0.32)	(0.20)	(0.23)	(0.51)
Mean Y	2.77	2.28	1.95	2.28	4.24

Notes: Foster care data are from AFCARS. Drug-induced death rate data are from NCHS Vital Statistics. Sample consists of all 50 states plus D.C., measured annually (N=663).Robust SE clustered at state-level in parentheses. Models control for unemployment rate, poverty rate, median income, percent SNAP beneficiaries, number of primary care physicians per capita, number of community mental health facilities per capita, percent white, black, Asian, and Hispanic, percent child population, state FE, year FE, state-specific quadratic time trends, and are weighted by state population. * p<0.10, **p<0.05, ***p<0.01

Table 3.5: Drug-Induced Death Rate and Child Maltreatment Rate in Large California Counties, 2002-2015

	Report Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
TOTAL REPORTS					
Drug-Induced Death Rate Per 100,000 Capita	1.48	0.66	2.29	1.54	1.19*
	(1.06)	(0.72)	(1.78)	(1.61)	(0.68)
Mean Y	526.43	388.26	726.76	757.37	343.46
SEVERE NEGLECT					
Drug-Induced Death Rate Per 100,000 Capita	-0.08	-0.38	-0.05	-0.06	-0.01
	(0.12)	(0.30)	(0.17)	(0.14)	(0.04)
Mean Y	8.53	20.55	12.44	7.74	2.59
GENERAL NEGLECT					
Drug-Induced Death Rate Per 100,000 Capita	1.42	1.44	2.32	1.61	0.83
	(1.03)	(0.89)	(1.75)	(1.50)	(0.57)
Mean Y	218.20	211.12	326.17	300.96	120.57
PHYSICAL					
Drug-Induced Death Rate Per 100,000 Capita	0.28	-0.08	0.51	0.51	0.18
	(0.31)	(0.21)	(0.48)	(0.50)	(0.20)
Mean Y	99.98	36.00	117.19	160.81	77.36
AT RISK (SIBLING ABUSED)					
Drug-Induced Death Rate Per 100,000 Capita	-0.04	-0.06	-0.13	-0.07	-0.03
	(0.38)	(0.21)	(0.50)	(0.62)	(0.26)
Mean Y	67.59	41.10	95.17	105.33	41.35
CARETAKER ABSENCE					
Drug-Induced Death Rate Per 100,000 Capita	-0.04	0.01	-0.12	-0.20	0.08
	(0.16)	(0.11)	(0.22)	(0.22)	(0.13)
Mean Y	14.55	12.42	16.14	15.69	14.05

Notes: Child maltreatment data are from CCWIP. Drug-induced death rate data are from NCHS Vital Statistics. Sample consists of 32 counties, measured annually, in California (n=448). Robust SE clustered at county-level in parentheses. Models control for unemployment rate, number of community mental health facilities per capita, percent of county receiving SNAP benefits, percent white, black, Asian, and Hispanic, percent child population, county FE, year FE, county-specific quadratic time trends, and are weighted by county population. * p<0.10, **p<0.05, ***p<0.01

scriptions to patients. This system is designed to prevent patients with opioid addictions or dependencies from being able to "doctor shop," by attempting to improperly acquire pharmaceutical opioids from multiple sources. A number of states instituted or strengthened PDMPs during our sample period. As a sensitivity check to our analysis of drug outcomes, we examine how states' foster care entrance rates are associated with commencement or strengthening of a state PDMP.

In light of evidence that more robust PDMP regulations have a beneficial impact on opioid-related mortality in ways that less robust programs do not (Patrick et al., 2016), we control for multiple dimensions of PDMP characteristics. "Proactive" PDMPs are those in which information is distributed to practitioners and authorities automatically rather than only when requested. "Real time" PDMPs are those in which the state database is updated at least weekly. A mandated PDMP may be proactive, real time, both, or neither; it is simply mandated that the state have a program. We include a dummy variable for the presence of each of these programs, and the coefficient estimates on these individual policy dummies in our regressions are additive. Data on the characteristics and timing of state PDMP policies come from the Prescription Drug Abuse Policy System.

Table 3.6 shows results of our regressions where PDMP characteristics are the opioid use measures of interest. They show no clear relationship between the simple mandating of a PDMP and foster care entrance rates. However, a more stringent requirement that the PDMP be updated in real time is associated with an increase in all-cause foster care entrance rates for children across a range of ages, with an estimated increase of about 6 cases per 100,000 children of all ages. In contrast, an increase of about 7 removals due to parental drug abuse per 100,000 children is associated with the implementation of a proactive PDMP system. It is unclear why a proactive PDMP would affect drug abuse-related removals specifically while a real time requirement would not. It could be the case that these two policies differentially affect intentional doctor shopping behavior by patients versus appropriate opioid prescribing practices. Alternatively, this set of results

could simply be an artifact of unobserved variation across states and over time in the degree of specificity in coding by case workers. In any case, our PDMP findings generally echo our analysis of opioid distribution quantities, indicating that restrictions on the ease of access to medical opioids are associated with worsening statistics for child well-being.

3.6 Discussion

3.6.1 Substitution Away from Legal Opioids

Our analysis shows that increased distribution of legal opioids is associated with decreases in rates of certain adverse events among children. Given the known dangers of drug dependence and abuse associated with prescription opioids, this result seems rather puzzling. It would however, be consistent with a series of findings from recent studies which have illustrated a pattern of substitution from legal opioids toward illegal drugs like heroin or illicitly manufactured fentanyl. If it is the case that these substituted illicit drugs are more dangerous than the quality controlled, legally manufactured products they serve to replace, and if it is the case that children's caregivers are among the population engaged in such substitution, then our pattern of results is consistent with this substitution story.

One of the most popular opioid products both for legitimate analgesic use and for abuse is the oxycodone-containing pill OxyContin. For many years, the most common means of abusing OxyContin was to crush the pill and then chew, inject, or snort it, thereby circumventing the delayed-release design of the swallowed pill and instead quickly achieving the desired high. In response to this means of abuse, the pill was reformulated in 2010 to make it much more difficult to crush and thus much less appealing for abuse. Alpert et al. (2018) examine the aftermath of this reformulation and find that, while it had the intended effect of reducing misuse of OxyContin, it also had the unintended effect of increasing abuse of heroin and possibly also fentanyl, two drugs with an even greater capacity for harm than oxycodone, and on net there is thus no evidence that the reformulation reduced overdose

Table 3.6: PDMP Laws and Foster Care Entrance Rate, 2003-2012

	FC Entrance Rate Per 100,000 Children of Corresponding Age Group				
	Total	Aged 0-1	Aged 2-5	Aged 6-12	Aged 13-17
PANEL A: REMOVAL REASON: ALL					
PDMP Mandatory	0.01	0.03	0.92	1.05	-2.18
	(3.17)	(6.89)	(3.54)	(2.47)	(3.32)
PDMP Proactive	3.58	6.22	2.43	1.37	6.62*
	(3.87)	(9.52)	(4.43)	(2.76)	(3.81)
PDMP Real Time	6.02**	8.78	6.89**	4.99**	5.39**
	(2.75)	(6.91)	(3.34)	(2.01)	(2.24)
Mean Y	108.90	220.13	108.47	75.41	111.59
PANEL B: REMOVAL REASON: PARENTAL DRUG ABUSE					
PDMP Mandatory	0.61	-0.47	0.97	0.42	0.76
	(1.72)	(5.08)	(1.88)	(1.40)	(1.07)
PDMP Proactive	7.36*	20.98*	8.46*	4.59	4.51**
	(4.20)	(12.31)	(4.99)	(3.10)	(1.85)
PDMP Real Time	0.94	0.98	1.73	1.07	0.12
	(1.24)	(3.38)	(1.32)	(0.95)	(0.97)
Mean Y	25.26	78.23	31.69	17.96	10.10
PANEL C: REMOVAL REASON: NEGLECT					
PDMP Mandatory	-1.81	-6.48	-1.99	-0.38	-1.98
	(2.73)	(6.69)	(3.52)	(2.21)	(1.81)
PDMP Proactive	5.00	13.13	3.77	2.91	5.74**
	(2.99)	(8.47)	(3.40)	(2.14)	(2.46)
PDMP Real Time	0.79	-2.18	1.78	1.02	0.81
	(1.99)	(5.63)	(2.57)	(1.41)	(1.43)
Mean Y	56.60	134.89	70.67	44.05	33.22
PANEL D: REMOVAL REASON: PARENTAL DEATH					
PDMP Mandatory	0.08	0.05	0.05	0.17**	-0.02
	(0.07)	(0.10)	(0.10)	(0.08)	(0.13)
PDMP Proactive	-0.02	-0.08	-0.07	-0.11	0.16
	(0.08)	(0.20)	(0.10)	(0.10)	(0.13)
PDMP Real Time	-0.05	-0.06	-0.14**	0.00	-0.06
	(0.05)	(0.07)	(0.06)	(0.06)	(0.08)
Mean Y	0.70	0.58	0.50	0.58	1.08

Notes: Foster care data are from AFCARS. PDMP state policy data from PDAPS. Sample consists of 50 plus DC, measured quarterly (n=1,836). Robust SE clustered at state-level in parentheses. Models control for unemployment rate, poverty rate, median income, percent SNAP beneficiaries, number of primary care physicians per capita, number of community mental health facilities per capita, percent white, black, Asian, and Hispanic, percent child population, state FE, year FE, state-specific quadratic time trends, and are weighted by state population. * p<0.10, **p<0.05, ***p<0.01

death rates across all drugs.

Ray et al. (2017) examine data gathered between 2010 and 2015 in Marion County, Indiana, which contains the city of Indianapolis. Using prescription drug data tracked by the state's PDMP, they document a decline during this period in the number of prescriptions for opioids in general and for fentanyl in particular. They show that this decline in prescriptions coincided with large increases in both heroin and fentanyl overdose deaths, as well as increases in the rates at which law enforcement investigations detected heroin and fentanyl. Together, these patterns strongly suggest that Marion County residents substituted toward heroin and illicit sources of fentanyl as ease of access to prescription opioids tightened.

As our study lacks any data which documents illicit drug distribution or rates of death from such drugs, we are unable to directly document the same types of substitution patterns from legal to illegal drugs. However, it is clear that our set of results would be consistent with this dangerous substitution behavior on the part of caregivers. Further research is warranted to more closely examine the mechanisms linking access to legal opioids with child well-being.

3.6.2 Limitations

Our analyses illustrate patterns relating opioid use measures with child welfare outcomes, and suggest differing relationships depending on the measure of opioid use employed. Our seemingly conflicting findings may be evidence of substitution between legal opioids and illegal substitutes, as we outline above. Our findings may also, however, be partially explained by several noteworthy limitations of our study. Although we control for a number of potential confounding characteristics of the states and counties considered, the observed changes to opioid outcomes are not exogenously driven, and thus our coefficient estimates cannot be interpreted as causal.

Our data are aggregated to the state or county level, rather than at the household- or individual-level. Although this research design is common, such high aggregation may

mask important variation within counties or states that play an important role in the relationship between opioid use and child well-being. Our data are also limited in other ways. For example, our measure of opioid prescriptions consists only of legal distribution. We do not capture opioids in distribution obtained from illegal origins. We do, however, observe opioids in distribution that may have been sold or consumed illegally, but were originally prescribed legally as long as the original prescription was between 2002-2015. If opioids are subsequently sold in markets beyond the state in which they were originally prescribed, our measure of legal opioid prescriptions would include measurement error.

Second, our data on child maltreatment are limited to counties in California. Other states may have different patterns in both opioid use and child maltreatment than those observed in California. In addition, our analysis on drug mortality and child maltreatment is further limited to the 32 most populous counties in California. Removing rural counties may be problematic since the prescribing rate for adults in rural areas is greater than that in urban areas (Garcia et al., 2017).

3.7 Conclusion

This study contributes to understanding the true scope of opioid use and dependency, and has implications for what the goals of public policy should be regarding this public health problem. For example, these results imply that, from a child welfare perspective, policies to reduce the negative effects of opioid addiction should focus less on reducing the overall supply of opioids and more on improving the ability to predict addiction and dependency. Our results also imply the relationship between opioid use and child well-being is complicated, and depends on how opioid use is measured. Research on increased opioid abuse in recent years has utilized various sources of data in an effort to quantify the reach of the epidemic. Our research suggests it is important that researchers think carefully about data, explore multiple forms of measurement, and understand the implications and limitations of using certain data sources.

The findings that increased rates of opioid-related emergency department visits and drug overdose deaths are associated with simultaneous increases in foster care entry are striking and concerning. As evidence mounts of the tremendous toll taken by the recent US opioid epidemic on adults' health and well-being, these findings suggest that children have also suffered serious consequences. Policy responses like PDMPs and education campaigns have endeavored to reverse the trend of increasing misuse and abuse of opioids, and public resources have been directed toward improving access to the treatment of substance use disorders among those directly affected. Our findings indicate that there is also a key role for policies that limit or mitigate the harmful, indirect effects of the opioid epidemic on children, which appear to be troublingly large.

The national average public cost for a child in foster care annually is $10,302 (Zill, 2011). In addition to the immediate effects child victims of the opioid crisis suffer, our findings suggest increased opioid abuse may generate considerable additional costs for the foster care system. These costs are on top of the incredible toll the opioid epidemic has had through medical costs and loss of life (Birnbaum et al., 2011). Accordingly, policy approaches designed to reduce the impact of the opioid epidemic on children have the potential to generate considerable public savings, besides affecting the lives of children who are at risk.

Our analysis of pharmaceutical drug distribution data paints a very different picture than our analysis of drug-related hospitalizations and deaths. We show that increased distribution of legal opioids is associated with decreases in the rates of adverse events among children. These results are seemingly suggestive of a beneficial effect of access to opioid painkillers on child welfare. This pattern could be consistent with the hypothesis that restrictions on access to these powerful pain drugs lead to worsened medical problems for families which in turn spill over into child welfare. Kilby (2015) finds evidence that restrictions on opioid prescribing through the implementation of state prescription drug monitoring programs lead to increases in days of work missed due to pain. Her findings

illustrate the unintended costs of curbing the use of prescription opioids, drugs which are potentially deadly and very addictive, but also highly effective at treating pain. Currie et al. (2018) find that opioids may facilitate labor market participation among women. Raissian (2015) and Nguyen (2013) use data from New York State and California, respectively, to show that the rate of child maltreatment reports tends to be related to employment market conditions. If restrictions on opioid prescribing result in greater pain and interruptions to work among parents, then this pattern may have ill effects for their children too.

In our view, however, a more likely explanation for our findings is that some caregivers who develop dependence on prescription opioids respond to the cessation of access to legally prescribed opioids by taking up alternative drugs like heroin or illicitly manufactured opioids such as fentanyl, highly dangerous drugs not captured by the data on legally distributed pharmaceutical opioids which we consider here. Other researchers have documented evidence of this pattern of substitution in US populations irrespective of caretaker status (Alpert et al., 2018; Ray et al., 2017). Further research is warranted to examine these and other hypotheses regarding the mechanisms which connect access to opioids with child welfare in particular.

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Appendix A

Supplementary Tables for Chapter 1

Table A.1: Effects of Education on Mortality, Omitting Background Controls

		With controls	No controls
Pooled	coefficient	-0.021*	-0.019*
	standard error	*(0.013)*	*(0.010)*
	# of twin pairs	558	558
Males	coefficient	-0.031*	-0.029*
	standard error	*(0.020)*	*(0.019)*
	# of twin pairs	204	204
Females	coefficient	-0.014	-0.008
	standard error	*(0.014)*	*(0.014)*
	# of twin pairs	354	680

Notes: Bootstrapped standard errors reported from 300 replications. One-sided p-values reported for estimated coefficients on years of education. Asterisks represent statistical significance levels: *** 1%, ** 5%, * 10%.

Table A.2: Estimates of $(\alpha_1 - \alpha_2)$

	Pooled genders	Males	Females
Mortality	17.4	11.3	29.4
Physical Health	0.7	6.8	0.7
Log earnings		6.4[(a)]	

Notes: (a) Estimate for imputed earnings of male MTR respondents as calculated by Behrman et al. (1994). Positive estimates for mortality and physical health problems imply compensating behavior. Positive estimate for earnings implies reinforcing behavior.

Appendix B

Supplementary Tables and Figures for Chapter 2

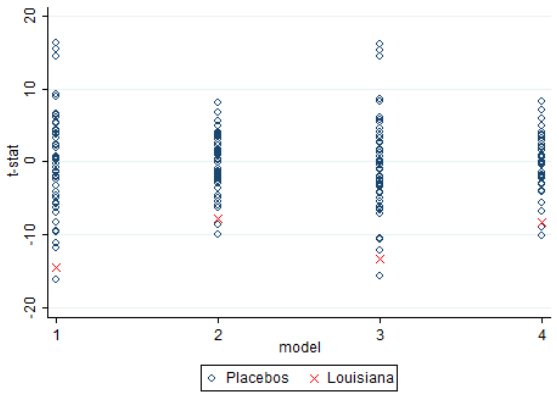
Table B.1: DD results: Drug quantities omitting 2007 Q4

	(1)	(2)	(3)	(4)
Opioids	-0.196***	-0.204***	-0.192***	-0.200***
	(0.014)	(0.026)	(0.014)	(0.024)
Louisiana pre-treatment mean	0.298			
Amphetamine	0.016	0.015	0.019	0.011
	(0.010)	(0.027)	(0.012)	(0.026)
Louisiana pre-treatment mean	0.013			
Demographic controls	No	Yes	No	Yes
State policy controls	No	No	Yes	Yes

Notes: Outcomes are log of morphine equivalent grams of opioids distributed per capita and log of grams of amphetamine distributed per capita. Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010, omitting 2007 Q4. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Figure B.1: Permutation tests: Drug quantities omitting 2007 Q4

(a) Opioids



(b) Amphetamine

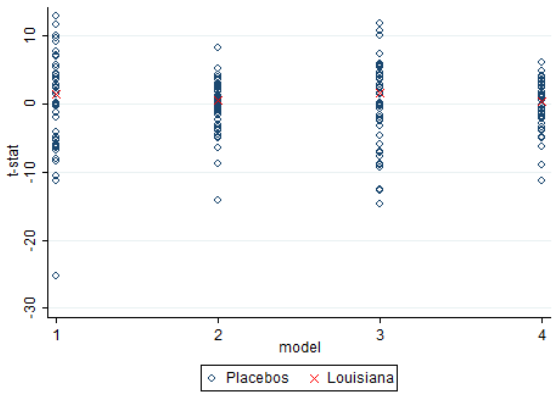


Table B.2: DD results: Louisiana opioid vs. amphetamine quantities omitting 2007 Q4

	(1)
Post×Opioid	-0.166***
	(0.024)
Louisiana pre-treatment mean	0.298

Notes: Outcome is log of grams of drug type distributed per capita (opioids measured in morphine equivalent grams). Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010, omitting 2007 Q4. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Figure B.2: Permutation tests: Louisiana opioid versus amphetamine quantities omitting 2007 Q4

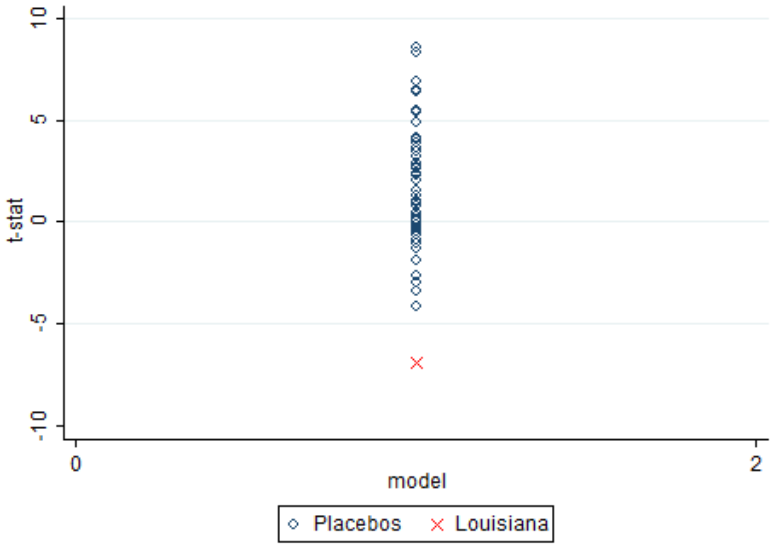


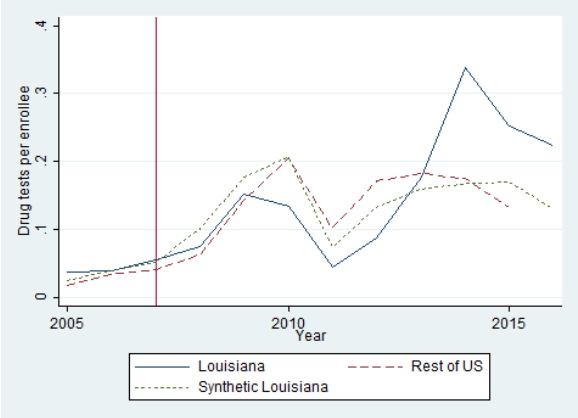
Table B.3: DDD results omitting 2007 Q4

	(1)
Post×Opioid×Treat	-0.212***
	(0.020)

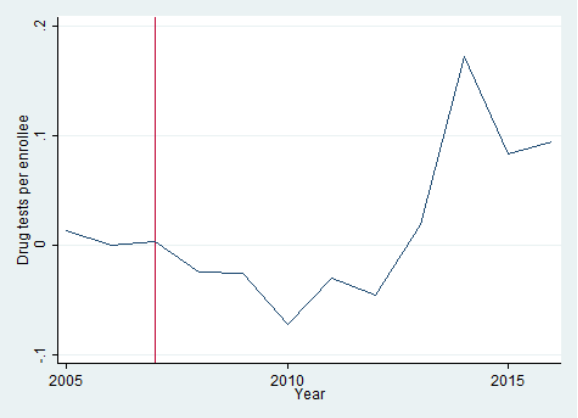
Notes: Outcome is log of grams of drug type distributed per capita (opioids measured in morphine equivalent grams). Outcome data represent quarterly state aggregate drug quantities measured in ARCOS from 2006 to 2010, omitting 2007 Q4. Standard errors clustered by state are presented in parentheses. The sample includes all US states except Florida, plus DC. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Figure B.3: Synthetic control: Medicare drug test claims

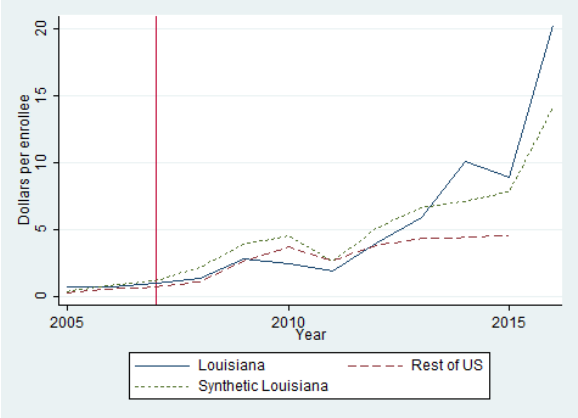
(a) Tests: Lousisiana vs. control



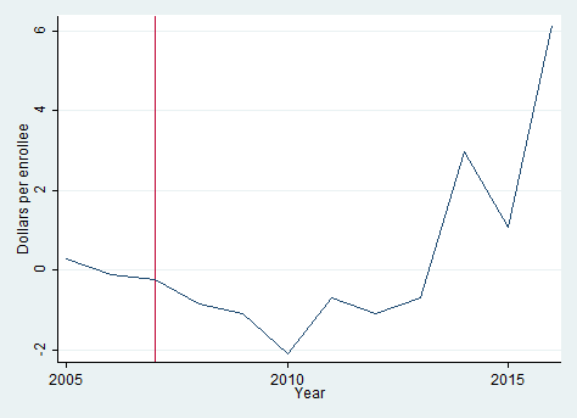
(b) Tests: Effect size estimates



(c) Test charges: Lousisiana vs. control



(d) Test charges: Effect size estimates



Notes: Figures are per Medicare enrollee and represent procedures that were approved as eligible for reimbursement under Medicare Part B. Annual data constructed from CMS (2017) represent aggregates for each of the 39 states other than Florida for which complete Medicare carrier summaries are available for 2005-2016.

Appendix C

Supplementary Tables and Figures for Chapter 3

To examine possible heterogeneity in the relationship between drug distribution and foster care entry, we produce a set of scatterplots of states or counties, with percent increase in the considered opioid measure over the sample period plotted on the horizontal axis and percent increase in total child welfare measure (all causes and all ages) on the vertical axis. Shown in Figures C.1 through C.5, these graphs do not appear to show clear evidence that the relationships between rates of adverse child welfare outcomes and opioid distribution or abuse are significantly different for low- versus high-opioid increase states (or counties).

To examine this difference statistically, we divide the estimation sample for each regression from Tables 3.1 to 3.5 into halves based on the median increase in the opioid measure considered. We then re-run each regression while allowing a, the coefficient on the opioid measure, to differ between high- versus low-opioid increase states (or counties). Table C.1 shows the estimates of this difference in coefficients for each regression. The statistically significant result in column 2 indicates that the negative relationship between opioid distribution and child maltreatment reports is stronger in those California counties which experienced the greatest percentage increases in opioid quantities. The borderline statistically significant result in column 3 indicates that the observed relationship between opioid-related ED visit rates and foster care entry is stronger in those states which experienced the greatest percentage increases in ED visits.

Figure C.1: State changes in opioid distribution and foster care entry, 2003-2015

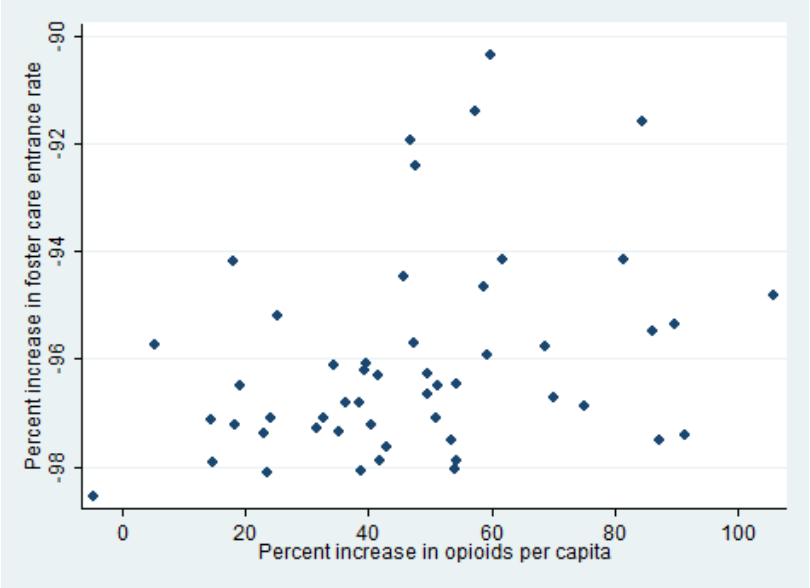


Figure C.2: County changes in opioid distribution and child maltreatment reports, California 2002-2015

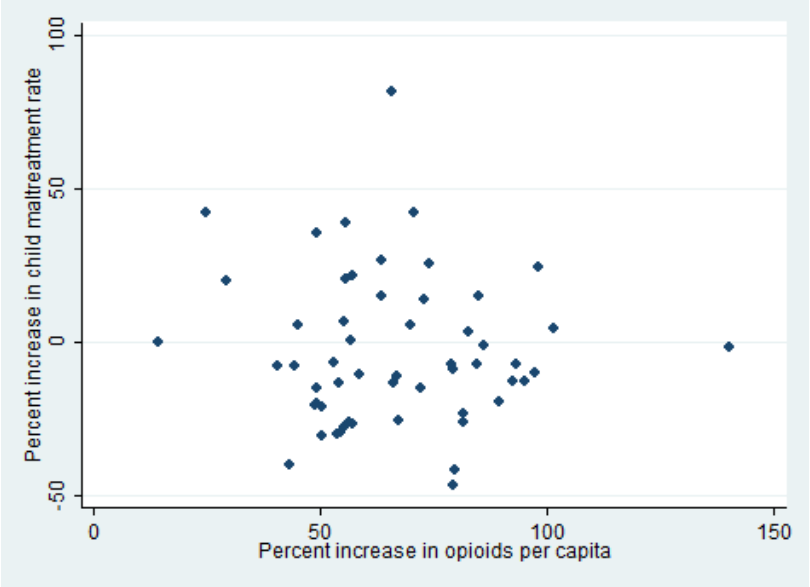


Figure C.3: State changes in opioid ED visits and foster care entry, 2005-2014

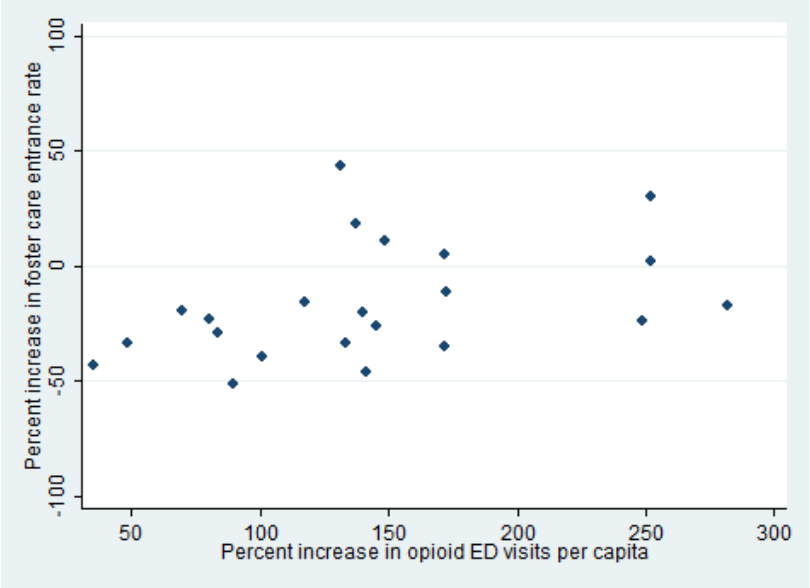


Figure C.4: State changes in drug-induced death rates and foster care entry 2003-2015

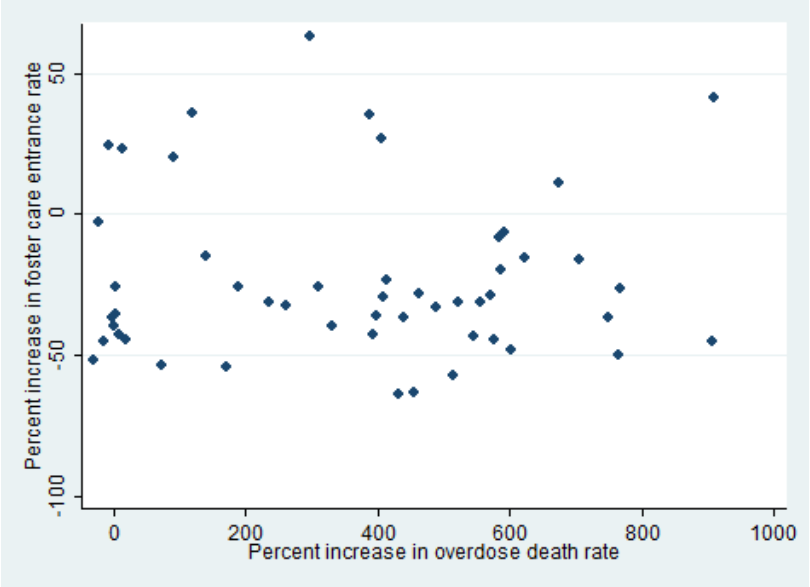


Figure C.5: County changes in drug-induced death rates and child maltreatment reports, 2002-2015

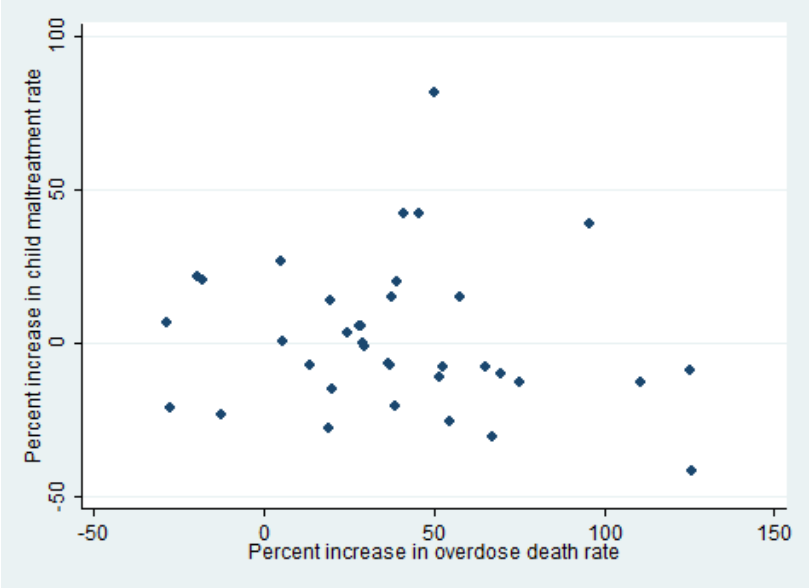


Table C.1: Heterogeneity in areas with high vs. low opioid increases

	1	2	3	4	5
Child outcome	FC entry	Maltreatment reports	FC entry	Maltreatment reports	FC entry
Opioid measure	Distribution	Distribution	ED visits	Mortality	Mortality
Level of observation	State-quarter	County-year	State-quarter	County-year	State-year
a_{high}-a_{low}	13.47	-165.75**	5.98*	29.08	2.09
	(36.14)	(72.52)	(3.48)	(23.58)	(1.96)

Notes: Standard errors clustered by state or county are presented in parentheses. Significance levels of 0.10, 0.05, and 0.01 are denoted by *, **, and ***, respectively.

Table C.2: Available ED Data

State	Number of Quarters	Years
ARIZONA	40	2005-2014
ARKANSAS	8	2013-2014
CALIFORNIA	40	2005-2014
CONNECTICUT	40	2005-2014
FLORIDA	40	2005-2014
GEORGIA	40	2005-2014
HAWAII	40	2005-2014
ILLINOIS	24	2009-2014
INDIANA	40	2005-2014
IOWA	40	2005-2014
KANSAS	40	2005-2014
KENTUCKY	28	2008-2014
MAINE	36	2006-2014
MARYLAND	40	2005-2014
MASSACHUSETTS	40	2005-2014
MINNESOTA	40	2005-2014
MISSOURI	40	2005-2014
MONTANA	4	2014
NEBRASKA	40	2005-2014
NEVADA	32	2007-2014
NEW HAMPSHIRE	40	2005-2014
NEW MEXICO	40	2005-2014
NEW YORK	16	2011-2014
NORTH CAROLINA	20	2010-2014
OHIO	40	2005-2014
RHODE ISLAND	36	2006-2014
SOUTH CAROLINA	40	2005-2014
SOUTH DAKOTA	25	2007-2014
TENNESSEE	40	2005-2014
UTAH	40	2005-2014
VERMONT	40	2005-2014
WISCONSIN	40	2005-2014