Stanford | Institute for Economic Policy Research (SIEPR)

WHAT DRIVES PRESCRIPTION OPIOID ABUSE? EVIDENCE FROM MIGRATION

Amy Finkelstein
MIT and NBER

Matthew Gentzkow
Stanford and NBER

Heidi Williams
MIT and NBER

August, 2018

Working Paper No. 18-028

What Drives Prescription Opioid Abuse?
Evidence from Migration

Amy Finkelstein
MIT and NBER

Matthew Gentzkow
Stanford and NBER

Heidi Williams
MIT and NBER[*]

August 2018

Abstract

We investigate the role of person-specific and place-specific factors in the opioid epidemic by analyzing cross-county migration of disabled Medicare recipients and its relationship with prescription patterns associated with opioid abuse. We find that movement to a county with a 20 percent higher rate of opioid abuse (equivalent to a move from a 25th to 75th percentile county) increases rates of opioid abuse by 6 percent, suggesting that roughly 30 percent of the gap between these areas is due to place-specific factors. These effects are particularly pronounced for prior opioid users, who experience an increase in opioid abuse four times larger than the increase for opioid naives.

Keywords: Opioids; opioid epidemic; disability; geographic variation
JEL: H51, I12

[*]We thank the National Institute on Aging (Finkelstein R01-AG032449), the National Science Foundation (Williams, 1151497), and the Stanford Institute for Economic Policy Research (SIEPR) for financial support. We are grateful to Alden Cheng, Paul Friedrich, Ken Jung, Geoffrey Kocks, Abby Ostriker, and Anna Russo for excellent research assistance, and to Jonathan Gruber, Ellen Meara and Molly Schnell for helpful discussions.

1 Introduction

Opioid abuse in the United States has reached crisis proportions. In 2015, 90 Americans died each day from opioid overdoses (Rudd et al. 2016). Deaths from opioids rose five-fold between 1999 and 2015, and have continued to grow since then (NCHS 2017; CDC 2017c). In 2016, deaths from opioids were more than double the number of homicide deaths, and were an order of magnitude higher than cocaine-related deaths at the height of the 1980s crack epidemic (Frieden and Houry 2016; Rudd et al. 2016; GAO 1991). The intensity of the crisis varies significantly across geographic areas, with opioid prescription rates per capita (measured in morphine milligram equivalents) almost four times higher for the 75th percentile county than the 25th percentile county (McDonald et al. 2012).

In this paper, we analyze changes in opioid prescription patterns as individuals move across counties to identify the relative importance of person-specific and place-specific factors in driving opioid abuse. Person-specific factors include mental health status and prior substance abuse, characteristics whose importance has been emphasized in the medical literature (Ives et al. 2006; Sullivan et al. 2010; Fischer et al. 2012). They also include individual economic circumstances such as employment and income that have been highlighted in influential work by Case and Deaton (2015; 2017) documenting the rise of what they term "deaths of despair." Place-specific factors include the propensity of local physicians to prescribe opioids for what they believe to be legitimate reasons (Barnett et al. 2017; Schnell and Currie 2017; Doctor et al. 2018), the availability of unscrupulous providers and "pill mill" pain clinics (Lyapustina et al. 2016; Rutkow et al. 2015), and the presence of policies such as prescription monitoring programs intended to limit abuse (Meara et al. 2016; Kilby 2015), as well as area-specific peer effects and social learning. Person-specific factors generally correspond to what we would think of as "demand" and place-specific factors to what we would think of as "supply," though cases like peer effects imply that correspondence is imperfect.

The relative importance of person-specific and place-specific factors is important for understanding both the causes of the opioid crisis and the likely impacts of policies designed to combat it. If, for example, the supply of prescription opioids from unscrupulous doctors is a key contributor to opioid abuse, policies that effectively restrict the supply of opioids could potentially

have a large impact. If, on the other hand, opioid abuse is primarily determined by person-specific characteristics such as mental health or poor economic prospects, the implications for effective policy would be very different. Though existing studies suggest important roles for features of both people and places, quantitative evidence on their relative magnitudes is limited.

Our analysis focuses on prescription opioids, which are typically prescribed by doctors to treat moderate to severe pain (CDC 2017a); prescription opioids include oxycodone (Oxycontin), hydrocodone (Vicodin), and morphine. Our analysis does not speak directly to the use of illegal sources of opioids such as heroin.

The key intuition behind our empirical strategy is that supply conditions and other place factors change discretely on move, whereas person-specific factors such as long-term mental health do not. If geographic differences in opioid abuse mainly reflect a supply constraint like the availability of unscrupulous physicians, a migrant's rate of abuse should jump discretely on move to a level similar to others in her destination. If geographic differences instead mainly reflect demand differences, her rate of abuse should not change systematically on move. This strategy is similar to those we have used to understand the determinants of geographic variation in brand preferences (Bronnenberg et al. 2012) and healthcare utilization (Finkelstein et al. 2016).[1]

We focus on individuals enrolled in the Supplemental Security Disability Program (SSDI). This population is attractive to study for several reasons. First, opioid use is especially prevalent in this population. Roughly half of SSDI recipients receive an opioid prescription each year (Meara et al. 2016). Although they account for less than 5 percent of the adult population, we estimate that adult SSDI recipients account for about 13 percent of all opioid prescriptions (Autor 2015; SSA 2017; CDC 2017d; authors' calculations[2]). Second, the SSDI population has a relatively fixed level of government benefits and extremely tight limits on additional earnings, meaning we can rule out large changes in individual income or employment around moves. Third, all SSDI recipients enroll

[1]These papers in turn draw on earlier work in the healthcare space exploiting patient migration (Song et al. 2010) and physician migration (Molitor 2018). More broadly, a growing literature has used mover-based designs to study, among other things, the sources of differences across firms in wages (Card et al. 2013), differences across teachers in value-added (Chetty et al. 2014), and differences across geographies in tax reporting (Chetty et al. 2013) and in inter-generational mobility (Chetty and Hendren 2018a,b). Most closely related to the current paper, Laird and Nielsen (2016) exploit individual moves across municipalities in Denmark to generate quasi-random matches between individuals and physicians and estimate that treatment by a physician with a higher rate of opioid prescribing is associated with both more use of prescription opioids and a decline in labor market activity.

[2]SSDI recipient opioid use was calculated by multiplying the average opioid use per SSDI recipient on Medicare Part D (observed in our data) by the total number of SSDI recipients.

in Medicare - the federal public health insurance for the elderly and disabled in the United States; as a result, we are able to take advantage of rich panel data on prescription drug use (and residency changes) among a 20 percent random sample of SSDI recipients enrolled in Medicare from 2006 to 2014. Unlike Medicare for the elderly, enrollment in SSDI is not limited to those over 65 years of age, so our data includes a wide range of ages. We analyze about 1.5 million SSDI recipients who are enrolled in Medicare Part D, the federal prescription drug benefit program for elderly and disabled Medicare enrollees that began in 2006.

We analyze several commonly used measures of opioid abuse, defined at the enrollee-year level: whether the individual fills prescriptions from four or more prescribers ("Many Prescribers"); whether the individual fills prescriptions that result in a daily morphine-equivalent dose (MED) of more than 120 mg in any calendar quarter ("High MED"); and whether the individual fills a new prescription before a previous one has run out ("Overlapping Prescriptions"). These measures are frequently used in the literature as proxies for potential opioid abuse (Hall et al. 2008; Larrick 2014; Meara et al. 2016; Morden et al. 2014; Sullivan et al. 2010; Logan et al. 2013; Jena et al. 2014; Rice et al. 2012) and are correlated with rates of opioid overdose (Braden et al. 2010; Bohnert et al. 2011; Dunn et al. 2010; Jena et al. 2014). To summarize results using different measures, we develop a summary index of opioid abuse ("abuse index") that combines these commonly used indicators as well as more flexible functions of observed prescriptions; the index weights are derived from a multivariate regression of an indicator for poisoning events (i.e. emergency room visits or inpatient hospital admissions for poisoning) on the prescription measures.

Our main findings are summarized in event-study analyses of changes in these outcomes around a move. The results show that individuals moving to higher-abuse areas immediately begin abusing at higher rates following the move, and those moving to lower-abuse areas immediately begin abusing at lower rates. We estimate that moving from a county at the 25th percentile of the abuse index to the 75th percentile - a 20 percent increase in the index - is associated with a 6 percent increase in the migrant's abuse index. These results are consistent with a large role for place factors such as the prescribing behavior of local physicians or pill mills. At the same time, they also leave a large share of variation to be explained by person factors. We see qualitatively similar patterns when we examine the individual measures High MED, Overlapping Prescriptions, and Many Prescribers separately, with a somewhat larger relative change on move for the third measure.

The impact of moving differs markedly when comparing migrants who have not used opioids in the previous year ("opioid naives") and migrants who have used opioids in the previous year ("prior users"). The effect of moving to a high-opioid area on the abuse index is four times larger for prior users than opioid naives, and the 4:1 ratio is similar when we break out the individual abuse measures separately. This is consistent with prior users having excess demand for opioids relative to the available supply. It may also suggest a larger elasticity of prior users' behavior with respect to local supply-side factors.

An important question is whether the place factors we measure capture effects on total opioid abuse, or if they only capture changes in prescription opioid abuse, with offsetting substitution to illegal forms of opioids such as heroin. Our data do not permit us to examine this directly, which is an important limitation of our study. We present some suggestive evidence that is consistent with moves to areas with different rates of prescription opioid abuse affecting total opioid use, although the results are noisy and do not preclude the existence of quantitatively important substitution.

Finally, we use our empirical strategy to estimate county-specific place effects - i.e. the impact of being in a given county on the probability of opioid abuse - and examine how these place effects correlate with various observable county characteristics. Consistent with our strategy isolating important supply-side determinants of abuse, we find that demographic, health, and economic characteristics of the county are much less correlated with our estimated place effects than with the overall opioid abuse rate in the county. Areas with more physicians per capita, with no laws regulating pain management clinics, with lower Medicare spending per capita, and with higher scores on a healthcare quality index all tend to have higher county-specific place effects for opioid abuse.

Our results speak to an ongoing debate over the relative importance of different factors in driving the opioid crisis. Much of this work has been based on examination of the correlations between the geographic patterns of rising opioid abuse and changes in other factors. Case and Deaton (2015; 2017) and others have pointed to the important role of individuals' economic circumstances and related demand-side factors. By contrast, Ruhm (2018) and Currie et al. (2018) have argued that the patterns of increased drug deaths and opioid abuse across counties suggest an important role for the availability of drugs and little, if any, role for economic decline. Charles et al. (2018) conclude that both economic conditions and opioid supply have played a role in the geographic patterns of

rising local opioid use and deaths. We use a different empirical strategy, based on individuals moving across counties, to separate person-specific and place-specific factors. Qualitatively, our results suggest that all of the factors emphasized in past work may play an important role, but sharpen our estimate of their relative quantitative importance.

2 Data and Empirical Strategy

2.1 Opioids in the SSDI Population

The US Social Security Disability Insurance (SSDI) program is a federal program that provides income and medical insurance (Medicare) to workers experiencing long-term disability. SSDI expenditures are large and rising; in 2010 they comprised over seven percent of federal non-defense spending. About one-third of SSDI expenditures reflect Medicare costs (Autor 2015).

Individuals enrolled in SSDI qualify for Medicare after a two-year waiting period. About three-quarters of SSDI recipients are also enrolled in Medicaid - the public health insurance program for qualifying low income adults - and Supplemental Security Income (SSI), an income assistance program that covers low-income disabled adults; such individuals have a sufficient work history that they qualify for SSDI and Medicare, but sufficiently low earnings prior to disability that they also qualify for SSI and Medicaid. Dual eligibility for Medicaid and Medicare ensures that beneficiaries face no out of pocket expenses for covered healthcare.

In 2010, over 8 million disabled workers (and 2 million spouses and dependents) received SSDI benefits. Average monthly benefits were $1,165 in 2015 (SSA 2017). Individuals enrolled in SSDI are not allowed to have labor market earnings above a certain level, which was $1,090 in 2015 (SSA 2017). It is rare for individuals to exit SSDI and return to the labor force; most exits are either due to death or to reaching age 65 and so qualifying for Social Security old-age benefits (Autor and Duggan 2006).

Opioids are drugs that act on the nervous system to relieve pain, and they include both legally prescribed opioids and illegally manufactured and dispensed heroin. We focus in this study on prescription opioid use, as measured by the Medicare claims data. Prescription opioid use in the SSDI population is high; over half of disabled Medicare beneficiaries under the age of 65 receive

opioids annually (Morden et al., 2014). This presumably reflects this population's high rates of health problems - including pain - for which opioids might be prescribed, but may also reflect their limited economic circumstances and prospects as well.

The large role of such legal prescription drugs is a distinguishing feature of the opioid epidemic. About two-fifths of opioid deaths involve a prescription opioid (CDC 2017b). Rates of opioid prescribing have grown dramatically over the last two decades (Pualozzi et al. 2011; Guy et al. 2017), an increase which coincided with the rise in opioid abuse and deaths (Dart et al. 2015). Estimates suggest that the amount of opioids prescribed each year is enough for every American to be medicated around the clock for a month (Doctor and Menchine 2017). Despite public policy efforts aimed at curbing opioid prescriptions, both prescription opioid abuse and prescription opioid drug overdoses continue to rise (Meara et al. 2016).

2.2 Data and Definitions

Medicare Data We analyze a 20 percent random sample of Medicare enrollees from 2006-2014. To measure opioid use and abuse, we rely on Medicare Part D prescription drug claims data. Other papers that have used these data to analyze opioid use in the SSDI population include Morden et al. (2014) and Meara et al. (2016); in addition, Buchmueller and Carey (2018) use the Medicare Part D data to analyze opioid use for all Medicare recipients.

Medicare Part D is a federally subsidized but privately provided prescription drug insurance program for Medicare recipients that began in 2006. Enrollment in Part D is voluntary. About 75% of SSDI recipients enroll in Medicare Part D either through a stand-alone Part D plan or through Medicare Advantage—a set of private insurance plans that offer an alternative form of health insurance to traditional Medicare. This Part D enrollment rate is slightly higher than the Part D enrollment rate of about 60% among elderly Medicare recipients (Cubanski et al., 2016).

For individuals enrolled in Part D, our detailed, claim-level data allow us to observe which prescriptions are filled (including the dosage), as well as who the prescriber is. We are able to follow Part D enrollees in a panel over time, and to observe basic demographic information including gender, age, race, Medicaid enrollment, and zip code of residence, defined as the address on file for Social Security payments as of March 31st of each year. To match the timing with which we

observe enrollees' residence, we define all outcome variables for year t to be aggregates of claims from April 1 of year t through March 31 of year $t+1$; we thus analyze data for 8 years: t=2006 through t=2013. Finally, we also observe hospital and outpatient (i.e. Medicare Parts A and B) claims for the SSDI enrollees.

Our baseline geographic unit of analysis is a county. There are about 3,000 counties in the United States. Prior work has documented substantial geographic variation in opioid prescribing rates across counties (Guy et al. 2017). We show below that our main results are robust to analysis at other levels of geography such as state or commuting zone. On average, for our SSDI population, we estimate that about three-fifths of opioid prescriptions filled are prescribed by a doctor who practices within the individual's county of residence.

Sample construction From our original 20 percent sample of all Medicare enrollees from 2006-2014 (14.8 million enrollees, 91 million enrollee-years) we limit ourselves to the 3.2 million enrollees, or 18.5 million enrollee-years, whose original Medicare coverage was due to SSDI. We further restrict to enrollee-years with 12 months of Medicare Part A and Part B coverage, which excludes 6.5 million enrollee-years (35% of the enrollee-years in the sample), primarily due to enrollment in Medicare Advantage.[3]

We define individuals to be "non-movers" if their county of residence is the same throughout our sample period. We define individuals to be "movers" if their county of residence changes exactly once during this period. Of the sample of both movers and non-movers with 12 months of Medicare Part A and Part B coverage, we exclude the 6 percent of the sample (125,841 enrollees, 717,204 enrollee-years) who changed their county of residence more than once between April 2006 and March 2013; this represents about one-third of the sample who changed their county at least once during our study period. Finally, we follow the approach developed in Finkelstein et al. (2016) and exclude about half of the remaining "movers" whose share of claims in their destination county, among claims in either their origin or destination county, is not higher by at least 0.75 in the post-move years relative to the pre-move years.[4] Appendix Figure A.1 shows that this approach

[3]We exclude enrollee-years in Medicare Advantage because we do not observe their non-drug claims which, we will see below, are important for identifying movers.

[4]The change in claim share is not defined for movers who do not have at least one claim both pre- and post-move. We exclude these cases if: (i) they have no post-move claims and a pre-move destination claim share greater than 0.05; (ii) they have no pre-move claims and a post-move destination claim share less than 0.95. See Finkelstein et al. (2016)

successfully identifies the timing of moves.

Finally, for most of our analyses, we restrict to enrollee-years that are covered by Medicare Part D for all 12 months. This restriction eliminates about 35% of the remaining sample; we undertake robustness analysis below to show that our results are not affected by changes in sample composition as individuals may exit or enter Part D coverage. Our final sample contains 103,847 movers (493,361 enrollee-years) and 1,355,327 non-movers (6,069,460 enrollee-years).

Outcome measures Opioids are both a risky addictive drug and a critical source of relief for patients suffering acute pain. This makes it difficult to determine with certainty which prescriptions are consumed or diverted for non-medical purposes, and which are part of a careful treatment plan. Survey questions about prescription drug misuse suffer from response bias due to stigma surrounding addiction and drug abuse (Sullivan et al. 2010). Even in a clinical setting, physicians may struggle to identify opioid abuse when a patient may be feigning pain (Parente et al. 2004).

While there is no consensus gold standard measure of opioid abuse among clinicians or medical researchers (Sullivan et al. 2010; Turk et al. 2008), the medical literature studying the opioid epidemic has developed several proxies for likely opioid abuse based on prescription data (Hall et al. 2008; Larrick 2014; Logan et al. 2013; Meara et al. 2016; Morden et al. 2014; Sullivan et al. 2010; Jena et al. 2014; White et al. 2009; Rice et al. 2012). These measures identify patterns in prescriptions at the person-year level that are correlated with adverse drug-related outcomes such as opioid misuse (use for a non-medical purpose), dependence, emergency room visits, and overdose deaths. Each of the common hazardous prescribing indicators attempts to measure a different aspect of abuse; often, multiple indicators are used within the same study to provide a more complete picture of potential abuse. Broadly speaking, one class of measures tries to identify opioid abuse based on the supply of prescription opioids received, while another tries to capture the practice referred to as "doctor shopping" - i.e. collecting prescriptions from multiple prescribers or pharmacies simultaneously, and so exceeding the total amount any one prescriber would have knowingly prescribed on their own.

We draw on the existing literature to define and analyze three separate indicators for hazardous prescribing; these have been shown in prior work to be correlated with adverse outcomes such as

for more discussion.

poisoning-related emergency room visits or deaths (Braden et al. 2010; Bohnert et al. 2011; Dunn et al. 2010; Jena et al. 2014; Logan et al. 2013). First, following Larrick (2014), Meara et al. (2016), and Sullivan et al. (2010), we define an indicator for an individual filling prescriptions that result in an average daily morphine equivalent dosage (MED) of more than 120 mg in any quarter ("High MED"). Second, following Larrick (2014) and Meara et al. (2016), we define an indicator for the individual filling prescriptions from four or more prescribers ("Many Prescribers")[5]. Third, following White et al. (2009), Rice et al. (2012), Jena et al. (2014), and Logan et al. (2013), we define an indicator for an individual filling a new prescription before a previous prescription has run out ("Overlapping Prescriptions"). The existing literature has interpreted High MED as a measure of prescription supply, and has interpreted Many Prescribers and Overlapping Prescriptions as measures of doctor shopping. Appendix A provides more detail on the exact variable construction.

In addition to analyzing each of these three outcome measures separately, we also develop a summary index of opioid abuse ("abuse index") in order to present results that combine a larger and more flexible set of individual outcome measures. We first define an indicator for each enrollee-year equal to one if the enrollee had an emergency room or inpatient admission for poisoning in the following year. This indicator variable is intended to capture opioid overdose events, though due to limitations in the way the data are constructed, we cannot isolate these events from other poisonings. Our reliance on this measure as a welfare-relevant outcome resulting from opioid use follows previous work such as Meara et al. (2016). We then define our abuse index to be the predicted values from an enrollee-year level regression of this indicator on (i) indicators for bins of number of prescribers; (ii) indicators for bins of total MED; (iii) indicators for bins of the total number of opioid prescriptions; (iv) Overlapping Prescriptions; (v) High MED; and (vi) county fixed effects. We estimate this regression on the sample of all non-movers from 2006-2010, and compute predicted values for all movers. In constructing the predicted values, we use the observed values of (i)-(v) and fix (vi) at its mean value in the full sample, then multiply the values by 100. Appendix A describes the construction of the abuse index in more detail. Our abuse index can be interpreted as the predicted probability in percentage points of an overdose or other poisoning-related hospital visit given the pattern of prescribing we observe.

Figure 1 shows the distribution of the abuse index in our sample across counties. New England,

[5]Due to a data issue, we are currently missing the "Many Prescribers" measure for t =2012.

Appalachia, the Rust Belt and the Northwest are all particularly hard-hit. In Appendix A we also show that the geographic variation in the abuse index is highly correlated - across both time and space - with other measures of opioid abuse both within and outside the SSDI sample.

Summary statistics The first column of Table 1 reports summary statistics for our overall study sample. One-half are female, about three-quarters are white, and two-thirds receive Medicaid. The average age is 56, with about half aged 40-60 and about 40 percent over age 60. Consistent with the high rate of opioid use in the disabled population, we estimate that about two-fifths of our enrollee-years have at least one opioid prescription. More than 4 percent of enrollee-years include a quarter with an average daily morphine equivalent dosage of 120 mg or higher (High MED), about 6 percent of enrollee-years include prescriptions from four or more unique prescribers (Many Prescribers), and over 15 percent of enrollee-years have Overlapping Prescriptions. The average value of the abuse index in our sample is 1.1, which, consistent with its interpretation as a predicted probability of a poisoning hospital admission, matches the 1.1 percent rate of emergency department or inpatient admissions related to poisonings. Columns 2 and 3 show these summary statistics separately for non-movers and movers respectively. About 100,000 enrollees - or about 7 percent of our sample - move across counties exactly once over our 8 year study period. Compared to non-movers, movers are somewhat more likely to be female, white, on Medicaid, and younger. They also exhibit slightly higher rates of prescription opioid use and abuse.

We also briefly explore the nature of moves. One-third of moves are cross-state. The average distance moved (measured between the population-weighted county centroids based on the 2010 census) is 396 miles, with a median move of 116 miles and a standard deviation of 571 miles. The median county receives 12 movers and the mean county receives 32.[6]

2.3 Empirical Strategy

We focus on an event study analysis of changes in various measures of prescription opioid use associated with the timing and direction of moves. We estimate the event study on the sample of movers (Table 1, column 3). We characterize each mover i by δ_i, the difference in the abuse

[6] 171 counties - located predominantly in the Great Plains States and accounting for less than 0.25% of the population - receive no movers.

measure (y) between the mover's destination $d(i)$ and origin $o(i)$:

$$\delta_i = \bar{y}_{d(i)} - \bar{y}_{o(i)},$$

and we measure it with its sample analog $\hat{\delta}_i$, calculated based on the full sample of non-movers shown in Table 1, column 2.[7] For the sample of movers, we then estimate:

$$y_{ijt} = \alpha_i + \tau_t + \rho_{r(i,t)} + \theta_{r(i,t)}\hat{\delta}_i + x_{it}\beta + \varepsilon_{ijt} \tag{1}$$

where y_{ijt} is a measure of opioid abuse for individual i living in place j in year t, α_i are individual fixed effects, τ_t are calendar year fixed effects, x_{it} are five year age bins, $\rho_{r(i,t)}$ are dummies for year relative to move, $\theta_{r(i,t)}$ are relative-year-specific coefficients on $\hat{\delta}_i$, and ε_{ijt} is an error term. We define relative year $r(i,t)$ for a mover who moves during year t_i^* to be $r(i,t) = t - t_i^*$. Thus year 0 is the year of the move, relative year -1 is the last year in the origin, and relative year 1 is the first year in the destination. Appendix Figure A.1 shows that of relative year 0 claims located in either a mover's origin or her destination, 60 percent are in the destination. Including the relative year dummies $\rho_{r(i,t)}$ allows us to control for the possibility that the decision to move is correlated with changes in the propensity for prescription opioid abuse.

Our main estimates of interest are the event study coefficients $\theta_{r(i,t)}$. These can be interpreted as the average residual outcomes of movers in each relative year $r(i,t)$, scaled by the difference $\hat{\delta}_i$ between the average outcome in mover i's destination and the average outcome in her origin. If movers' outcomes looked just like the average in their origin in a given relative year, the estimated coefficient would be zero. If movers' outcomes looked just like the average in their destination, the estimated coefficient would be one. Where the coefficients fall between the zero and one provides a measure of the extent to which movers' behavior adjusts, and thus of the importance of place-specific factors (which change on move) relative to person-specific factors (which do not). Finkelstein et al. (2016) derive a similar event study specification from a fully specified economic model, and show formally a sense in which the event study coefficients quantify the share of variation explained by place factors. Coefficients prior to the move provide a diagnostic on the

[7] We calculate, for each area j, the simple average of the outcome within a year across non-movers, and then take the simple average of that across years in our sample.

extent of pre-trends or other differential selection.

Figure 2 shows the distribution of $\hat{\delta}_i$ for our abuse index. The mean value of $\hat{\delta}_i$ is close to zero and the distribution is roughly symmetric, implying that moves from low- to high-abuse counties are as common as moves from high- to low-abuse counties. A little over 15% of movers have a $\hat{\delta}_i$ of 0.2 or more in absolute value (equivalent to moving between a 25th and 75th percentile county).

The specification in equation (1) allows for migrant-specific differences in the level of the outcome, and for these differences to be correlated with the migrant's origin and destination county. It also allows for a flexible pattern of outcomes in each year leading up to and after the move. The key assumption needed to interpret our results as causal is that any such time patterns by relative year do not vary systematically with the migrant's origin and destination; for example, if migrants who are trending upward in their opioid use pre-move are more likely to move to higher opioid counties, this would violate our assumption. We will examine the pattern of $\theta_{r(i,t)}$ prior to move as one way to investigate the validity of this assumption.

3 Results

3.1 Main Event Study

Figure 3 shows our main event-study results, plotting the estimated coefficients $\hat{\theta}_{r(i,t)}$ from estimating equation (1) with the abuse index as the dependent variable. Since the event coefficients are only identified up to a constant term, we normalize the value for relative year -1 to zero. Recall that in relative year -1 an individual is in their origin and in relative year 1 the individual is in their destination. In relative year 0 the individual may be either in their origin or destination.

The results in Figure 3 show a sharp, discontinuous jump in abuse after relative year -1. There is little systematic trend pre-move, which is supportive of our identifying assumption. The impact of the effect appears to be immediate, and constant over the five years post-move that we observe. As discussed above, the magnitude of this jump—roughly 0.3—provides a measure of the share of the difference between a typical origin and destination attributable to place-specific factors.

Column 1 of Table 2 provides a more precise quantification, summarizing the values of $\hat{\theta}$ both one and four years post-move. The value one year post-move is 0.289 (standard error = 0.029).

This means that a move from a county at the 25th percentile of our abuse index to a 75th percentile county - i.e. a move from an abuse index of 1.0 to an abuse index of 1.2 - would be associated with an increase of 0.058 percentage points in the opioid abuse index. Put differently, the abuse index is 20 percent higher in the 75th percentile county than the 25th percentile county, and a move from the 25th to 75th percentile county would increase the abuse index by 6 percent, or about 30 percent of the cross-sectional difference. The effect remains quite similar four years post-move.

Figure 4 provides an alternative way to visualize changes in the abuse index around moves. We divide movers into ventiles according to the average destination-origin difference in outcomes $\hat{\delta}_i$. We then plot the average $\hat{\delta}_i$ in each ventile on the x-axis and the average differences in the movers' outcomes from 2-4 years pre-move to 2-4 years post-move on the y-axis. If outcomes were entirely driven by place factors, so that movers' outcomes were similar to their origins pre-move and similar to their destinations post-move, this plot would have a slope of one. If outcomes were entirely driven by demand factors, so that changes in movers' outcomes were unrelated to their destinations or origins, the plot would have a slope of zero. The results show a clear positive relationship, with the slope (0.35) approximately matching the size of the effects in the event study in Figure 3. This plot also suggests that the relationship between the size of the move and the on-impact change is roughly linear and symmetric for moves with negative and positive values of $\hat{\delta}_i$, consistent with the functional form imposed by equation (1).

We cannot say conclusively how the changes in opioid prescriptions we observe translate into changes in total abuse (including illegal opioids). However, two pieces of suggestive evidence are consistent with total use moving in the same direction as our measures. First, given the highly addictive nature of illegal opioids such as heroin, we might expect significant substitution toward them when supply conditions for prescription opioids are tightened, but little or no substitution away from them when these conditions are relaxed. This would suggest that changes on moves to higher abuse counties should more accurately capture changes in total abuse. Figure 4 suggests that if we focus on moves to higher abuse counties the place effects we estimate are, if anything, larger than when we focus on moves to lower abuse counties. Second, if we look at actual hospital visits for poisonings as an outcome (rather than the predicted value based on prescriptions), the results are imprecise but directionally consistent with effects in the same direction as our main results, particularly for prior users. These results are described in more detail in Appendix C.

3.2 Differential Effects for Opioid Naives and Prior Users

There is reason to expect the response to changing supply conditions to differ based on a migrant's prior opioid history. For example, because prior users may suffer from opioid dependence or be engaged in a long-term program of pain management, prescribing guidelines recommend different approaches for patients starting opioids versus continuing an on-going opioid treatment (Dowell et al. 2016). Other studies exploring the impacts of drug supply have recognized this important source of heterogeneity among patients by focusing exclusively on patients without any previous opioid use (Shah et al. 2017; Jeffery et al. 2018).

To explore heterogeneity by prior opioid history, we re-estimate equation (1) separately for movers with pre-move opioid utilization ("prior users") and movers without prior opioid use ("opioid naives"). Our construction of area characteristics $\bar{y}_{j(i)}$ and the origin-destination differences $\hat{\delta}_i$ is unaffected (and continues to be based on all non-movers in county j). Following the standard definitions in the literature (Deyo et al. 2017; Sun et al. 2016), we define an "opioid naive" as a migrant who filled no opioid prescriptions in the year before the move $(r = -1)$, and a "prior user" as a migrant who filled an opioid prescription in the year before the move. The roughly 21 percent of migrant-years who have no Part D data in $r = -1$ fall in neither group. We characterize about 55 percent of the sample that do fall into one of these two categories as naive, and 45 percent as prior users. The results are shown in Figure 5 and are summarized columns 2 and 3 of Table 2.

The change in the event study coefficients on move is much larger for prior users than for naives, with the effect for the latter roughly one quarter of the effect for the former. Considering the impact of moving from a 25th to 75th percentile county in terms of the abuse index, the results suggest that for opioid naives this would increase abuse by about 2 percentage points (0.11 x 0.2), while it would increase abuse for prior users by 8 percentage points (0.4 x 0.2).

Overall, these results seem consistent with the supply of prescription opioids playing an important role. Prior users who are addicted may already be up against the supply side constraint; movement to a higher opioid place immediately increases their rate of prescription opioid abuse, while movement to a lower opioid place immediately decreases it.

3.3 Components of Abuse Index

We report analogous results for the individual outcome measures High MED, Many Prescribers, and Overlapping Prescriptions. Table 3 summarizes these results. Appendix Figure A.2 shows the corresponding event studies. Event study changes using High MED and Overlapping Prescriptions are slightly smaller than our baseline index result, while the results for Many Prescribers are substantially bigger.

Again, it can be useful to think about the results in terms of the impact of a move from a 25th to 75th percentile county. The individual components are much more dispersed than the abuse index, for which the interquartile county range is from 1.0 to 1.2, as shown in Appendix Table A.1. By contrast, the interquartile county range for High MED is 1.8 to 5.3 percent, for Many Prescribers it is 3.1 to 6.4 percent, and for Overlapping Prescriptions it is 10.5 to 18.7 percent. The coefficients in Table 3 thus imply that moving from a 25th to 75th percentile county would increase the probability of High MED or Many Prescribers by about 50 percent, and the probability of Overlapping Prescriptions by about 17 percent.

3.4 Robustness

Table 4 explores the sensitivity of our findings to several alternative specifications. Panel A presents our baseline results for ease of comparison. Appendix Figure A.3 shows the corresponding event studies. Panel B defines the geographic area of interest—and the definition of a mover—at the commuting zone level rather than the county level. Commuting zones are collections of counties and there are approximately 700 of them in the United States. Panel C returns to the county-level analysis but limits the sample to the one-third of movers who move across state boundaries. Panels D-F restrict the sample to enrollees who had Part D coverage in all years, who are never on Medicare Advantage, and who do not die in our sample, respectively. The results are stable across these alternative specifications.

As additional evidence on differential selection into our sample, Appendix Figure A.4 estimates equation (1) using as the dependent variable whether the enrollee-year had 12 months of Part D coverage, whether the enrollee-year had 12 months of traditional Medicare coverage (rather than Medicare Advantage), and whether the enrollee died in that year. For all of these analyses $\hat{\delta}_i$ is

still defined based on the abuse index. The results show some evidence of selection into Part D and out of Medicare Advantage upon move; for example, a move from a 25th percentile county on the abuse index to a 75th percentile county is associated with a 3 percentage point increase in the probability of having traditional Medicare coverage (relative to a mean of 83%) and about a 0.8 percentage point increase in the probability of having Part D coverage (relative to a mean of 70%). Reassuringly, however, Table 4 shows that restricting attention to individuals whose selection along these margins does not change during the sample does not affect our main results.

3.5 Correlates of Place Effects

The place effects we estimate could reflect a range of underlying economic primitives. We argued above that our empirical strategy is designed to isolate place-specific factors, including supply or availability of opioids, from person-specific factors such as patient health or earnings potential. To provide additional support for this interpretation, as well as to shed some light on what characteristics of the local environment may be important, we recover separate estimates of the place effect of each county and investigate their observable correlates.

We estimate:

$$y_{ijt} = \alpha_i + \gamma_j + \tau_t + \rho_{r(i,t)} + x_{it}\beta + \varepsilon_{ijt} \tag{2}$$

where the various terms are defined as in equation (1) and γ_j are place fixed effects.[8] We estimate equation (2) on the combined sample of movers and non-movers; for all non-mover enrollee-years we set the relative year dummies to zero; we exclude enrollee-year observations with relative year zero for movers. Our objects of interest are the $\hat{\gamma}_j{}'s$ - which estimate the impact of county j on the outcome. We use the abuse index for our outcome measure.

We then analyze a series of bivariate regressions of the relationship between the $\hat{\gamma}_j{}'s$ and various county-level average characteristics. As potential demand-side factors, we consider demographic characteristics of the county (e.g. race, gender, and age distribution), health characteristics (share diabetic and share obese), and economic characteristics (share uninsured, share with some college, unemployment rate, exposure to competition from Chinese manufacturers, share in manufacturing,

[8] As we discuss in Finkelstein et al. (2016), the event study equation (1) can be straightforwardly derived from equation (2).

and median household income). As potential supply-side factors, we focus primarily on characteristics of the healthcare system and state laws. The healthcare characteristics we examine are the propensity to prescribe opioids after minor surgery ("MED after surgery"), primary care physicians (PCPs) per capita, the rate of hospitalization for ambulatory care sensitivity conditions (ACSC) as a measure of poor healthcare quality, and Medicare spending per capita. The state laws we examine are pain clinic laws (regulating pain management clinics), prescribing restrictions (restricting physician dispensing of opioids), and prescribing guidelines. We present detailed definitions, data sources, and summary statistics for these measures in Appendix B.

In Figure 6 we show both the raw correlations between opioid abuse and various county characteristics (left hand panel) and the correlations between our estimated place effects $\hat{\gamma}_j's$ and the same county characteristics (right hand panel). We emphasize that these bivariate correlations should be seen as suggestive descriptive evidence. Given the large set of factors that could affect opioid abuse and the likelihood that many of these are correlated at the place level, it would not be appropriate to interpret any of these as causal effects of the respective place characteristics.

Previous literature on geographic variation has primarily focused on documenting cross-sectional correlations between opioid abuse rates and area characteristics (McDonald et al. 2012; Guy et al. 2017). These analyses have noted positive correlations between the per capita amount of opioids prescribed in a county and demographic factors (the share of non-Hispanic whites), population health characteristics (the prevalence of diabetes and arthritis), the healthcare system (number of available physicians) and economic conditions (rates of unemployment and Medicaid enrollment). Some but not all of our findings in the left hand panels align with these prior results. Some of these differences may reflect the specific population we study.

In contrast, our results indicate a much smaller relationship - if any - between demand-side correlates and the causal effects of place in the right hand panel. For example, in the raw cross section, a one standard deviation increase in the share white is associated with a 0.25 standard deviation increase in the abuse index; this correlation is an order of magnitude smaller when we examine the relationship between the place effect and share white.

Correlations with supply-side factors suggest that areas with pain clinic laws, high Medicare spending per capita, and high rates of hospitalization for ambulatory-care sensitive conditions ("ACSC rate") are associated with places that reduce opioid abuse, while higher physicians per

capita are associated with places that increase opioid abuse.

4 Conclusion

Understanding the causes of opioid abuse is an important first step toward designing effective policies to combat the current opioid crisis. Our paper contributes to a growing literature exploring this question. Focusing on the Medicare disabled population - a population in which over two-fifths of individuals legally use prescription opioids each year - we document sharp changes in abuse when individuals move across areas, suggesting an important role for place-specific factors such as physician prescribing behavior in driving the epidemic.

Quantitatively, our estimates suggest that moving from a county at the 25th percentile of prescription opioid use rates to the 75th percentile - a 20 percent increase in the probability of opioid abuse according to our summary measure - increases the probability that the migrant is abusing opioids by 6 percent. These effects are particularly pronounced for prior opioid users, who experience a four times larger increase than opioid naives.

These results suggest that supply-side policies that restrict opioid availability could potentially reduce opioid abuse. For example, as of April 2018, Medicare will no longer reimburse for high-dose long-term opioid prescriptions (Hoffman 2018; CMS 2018). Our finding of sharp changes in opioid rates when individuals move across areas supports the idea that such policies may be effective in reducing opioid abuse. In contrast, if we had instead found little or no change in opioid behavior upon move, that would suggest such policies could be less effective, and policies that focus on health and economic prospects might be more fruitful.

However, our results raise at least three important questions that our paper does not answer. First, faced with supply-side constraints for prescription opioid prescriptions, do prior opioid users actually reduce their use of opioids or do they instead simply substitute to illegal sources of opioids such as heroin? Naturally the answer to this question has important implications for normative analyses of any policies that aim to reduce the supply of prescription opioids, such as the recent Medicare policy change discussed above. We present suggestive evidence that to the extent there is substitution to illegal opioids, there is no compelling evidence that it completely offsets the supply-side constraints on prescription opioids, but more work is needed here. Second, our results

do not address the underlying determinants of the place effects we estimate: how much is due to supply conditions related to the health system per se, and why do these conditions vary across counties? And third, while our results point toward a large role for supply factors, they suggest demand factors are important as well. Better understanding the determinants of demand is also an important topic for future work.

References

Autor, D. H. (2015). The Unsustainable Rise of the Disability Rolls in the United States: Causes, Consequences, and Policy Options. In Scholz, J. K., Moon, H., and Lee, S.-H., editors, *Social Policies in an Age of Austerity: A Comparative Analysis of the US and Korea*, pages 107–136. Edward Elgar Publishing, Northampton MA.

Autor, D. H., Dorn, D., and Hanson, G. H. (2013). The China Syndrome: Local Labor Market Effects of Import Competition in the United States. *American Economic Review*, 103(6):2121–2168.

Autor, D. H. and Duggan, M. G. (2006). The Growth in the Social Security Disability Rolls: A Fiscal Crisis Unfolding. *Journal of Economic Perspectives*, 20(3):71–96.

Barnett, M. L., Olenski, A. R., and Jena, A. B. (2017). Opioid-Prescribing Patterns of Emergency Physicians and Risk of Long-Term Use. *New England Journal of Medicine*, 376(7):663–673.

Billings, J. (2003). Using Administrative Data To Monitor Access, Identify Disparities, and Assess Performance of the Safety Net. Technical report, Agency for Healthcare Research and Quality.

Bohnert, A. S. B., Valenstein, M., Bair, M. J., Ganoczy, D., McCarthy, J. F., Ilgen, M. A., and Blow, F. C. (2011). Association between opioid prescribing patterns and opioid overdose-related deaths. *JAMA*, 305(13):1315–1321.

Braden, J. B., Russo, J., Fan, M.-Y., Edlund, M. J., Martin, B. C., DeVries, A., and Sullivan, M. D. (2010). Emergency department visits among recipients of chronic opioid therapy. *Archives of Internal Medicine*, 170(16):1425–1432.

Bronnenberg, B. J., Dubé, J.-P. H., and Gentzkow, M. (2012). The Evolution of Brand Preferences: Evidence from Consumer Migration. *American Economic Review*, 102(6):2472–2508.

Buchmueller, T. C. and Carey, C. (2018). The Effect of Prescription Drug Monitoring Programs on Opioid Utilization in Medicare. *American Economic Journal: Economic Policy*, 10(1):77–112.

Card, D., Heining, J., and Kline, P. (2013). Workplace Heterogeneity and the Rise of West German Wage Inequality. *The Quarterly Journal of Economics*, 128(3):967–1015.

Case, A. and Deaton, A. (2015). Rising morbidity and mortality in midlife among white non-Hispanic Americans in the 21st century. *Proceedings of the National Academy of Sciences*, 112(49):15078–15083.

Case, A. and Deaton, A. (2017). Mortality and Morbidity in the 21st Century. *Brookings Papers on Economic Activity, Spring 2017*.

CDC (2017a). Opioid Basics.

CDC (2017b). Opioid Overdose.

CDC (2017c). *Provisional Counts of Drug Overdose Deaths, as of 8/6/2017*.

CDC (2017d). U.S. Prescribing Rate Maps.

Charles, K. K., Hurst, E., and Schwartz, M. (2018). The Transformation of Manufacturing and the Decline in U.S. Employment. *NBER Working Paper #24468*.

Chetty, R., Friedman, J. N., and Rockoff, J. E. (2014). Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates. *American Economic Review*, 104(9):2593–2632.

Chetty, R., Friedman, J. N., and Saez, E. (2013). Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *American Economic Review*, 103(7):2683–2721.

Chetty, R. and Hendren, N. (2018a). The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects. *The Quarterly Journal of Economics*.

Chetty, R. and Hendren, N. (2018b). The Impacts of Neighborhoods on Intergenerational Mobility II: County-Level Estimates. *The Quarterly Journal of Economics*.

CMS (2018). 2019 Medicare Advantage and Part D Rate Announcement and Call Letter.

Compton, W. M., Jones, C. M., and Baldwin, G. T. (2016). Relationship between Nonmedical Prescription-Opioid Use and Heroin Use. *New England Journal of Medicine*, 374(2):154–163.

Cubanski, J., Neuman, T., and Damico, A. (2016). Medicare's Role for People Under Age 65 with Disabilities.

Currie, J., Jin, J. Y., and Schnell, M. (2018). U.S. Employment and Opioids: Is There a Connection? *NBER Working Paper #24440*.

Dart, R. C., Surratt, H. L., Cicero, T. J., Parrino, M. W., Severtson, S. G., Bucher-Bartelson, B., and Green, J. L. (2015). Trends in Opioid Analgesic Abuse and Mortality in the United States. *New England Journal of Medicine*, 372(3):241–248.

Deyo, R. A., Hallvik, S. E., Hildebran, C., Marino, M., Dexter, E., Irvine, J. M., O'Kane, N., Van Otterloo, J., Wright, D. A., Leichtling, G., and Millet, L. M. (2017). Association Between Initial Opioid Prescribing Patterns and Subsequent Long-Term Use Among Opioid-Naïve Patients: A Statewide Retrospective Cohort Study. *Journal of General Internal Medicine*, 32(1):21–27.

Doctor, J. N., Nguyen, A., Lev, R., Lucas, J., Knight, T., Zhao, H., and Menchine, M. (2018). Opioid prescribing decreases after learning of a patient's fatal overdose. *Science*, 361(6402):588–590.

Doctor, M. and Menchine, J. (2017). Tackling the opioid crisis with compassion, new ways to reduce use and treatment.

Dowell, D., Haegerich, T. M., and Chou, R. (2016). CDC Guideline for Prescribing Opioids for Chronic Pain–United States, 2016. *JAMA*, 315(15):1624–1645.

Dunn, K. M., Saunders, K. W., Rutter, C. M., Banta-Green, C. J., Merrill, J. O., Sullivan, M. D., Weisner, C. M., Silverberg, M. J., Campbell, C. I., Psaty, B. M., and Von Korff, M. (2010). Overdose and prescribed opioids: Associations among chronic non-cancer pain patients. *Annals of internal medicine*, 152(2):85–92.

Edlund, M. J., Martin, B. C., Russo, J. E., Devries, A., Braden, J. B., and Sullivan, M. D. (2014). The Role of Opioid Prescription in Incident Opioid Abuse and Dependence Among Individuals with Chronic Non-Cancer Pain: The Role of Opioid Prescription. *The Clinical journal of pain*, 30(7):557–564.

Finkelstein, A., Gentzkow, M., and Williams, H. (2016). Sources of Geographic Variation in Health Care: Evidence From Patient Migration. *The Quarterly Journal of Economics*, 131(4):1681–1726.

Fischer, B., Lusted, A., Roerecke, M., Taylor, B., and Rehm, J. (2012). The Prevalence of Mental Health and Pain Symptoms in General Population Samples Reporting Nonmedical Use of Prescription Opioids: A Systematic Review and Meta-Analysis. *The Journal of Pain*, 13(11):1029–1044.

Frieden, T. R. and Houry, D. (2016). Reducing the Risks of Relief - The CDC Opioid-Prescribing Guideline. *New England Journal of Medicine*, 374(16):1501–1504.

GAO (1991). *The Crack Cocaine Epidemic: Health Consequences and Treatment*.

Guy, G. P., Zhang, K., Bohm, M. K., Losby, J., Lewis, B., Young, R., Murphy, L. B., and Dowell, D. (2017). Vital Signs: Changes in Opioid Prescribing in the United States, 2006-2015. *MMWR. Morbidity and Mortality Weekly Report*, 66.

Hall, A. J., Logan, J. E., Toblin, R. L., Kaplan, J. A., Kraner, J. C., Bixler, D., Crosby, A. E., and Paulozzi, L. J. (2008). Patterns of abuse among unintentional pharmaceutical overdose fatalities. *JAMA*, 300(22):2613–2620.

Hill, M. V., McMahon, M. L., Stucke, R. S., and Barth, R. J. (2017). Wide variation and excessive dosage of opioid prescriptions for common general surgical procedures. *Annals of Surgery*.

Hoffman, J. (2018). Medicare Is Cracking Down on Opioids. Doctors Fear Pain Patients Will Suffer. *The New York Times*.

Ives, T. J., Chelminski, P. R., Hammett-Stabler, C. A., Malone, R. M., Perhac, J. S., Potisek, N. M., Shilliday, B. B., DeWalt, D. A., and Pignone, M. P. (2006). Predictors of opioid misuse in patients with chronic pain: a prospective cohort study. *BMC health services research*, 6:46.

Jeffery, M. M., Hooten, W. M., Hess, E. P., Meara, E. R., Ross, J. S., Henk, H. J., Borgundvaag, B., Shah, N. D., and Bellolio, M. F. (2018). Opioid Prescribing for Opioid-Naive Patients in Emergency Departments and Other Settings: Characteristics of Prescriptions and Association With Long-Term Use. *Annals of Emergency Medicine*, 71(3):326–336.e19.

Jena, A. B., Goldman, D., Weaver, L., and Karaca-Mandic, P. (2014). Opioid prescribing by multiple providers in Medicare: retrospective observational study of insurance claims. *BMJ*, 348:g1393.

Kilby, A. E. (2015). Opioids for the masses: Welfare tradeoffs in the regulation of narcotic pain medications. *Working Paper*.

Laird, J. and Nielsen, T. (2016). The Effects of Physician Prescribing Behaviors on Prescription Drug Use and Labor Supply: Evidence from Movers in Denmark. *Working Paper*.

Larrick, A. (2014). Request for Comments: Enhancements to the Star Ratings for 2016 and Beyond.

Logan, J., Liu, Y., Paulozzi, L., Zhang, K., and Jones, C. (2013). Opioid Prescribing in Emergency Departments: The Prevalence of Potentially Inappropriate Prescribing and Misuse. *Medical Care*, 51(8):646–653.

Lyapustina, T., Rutkow, L., Chang, H.-Y., Daubresse, M., Ramji, A. F., Faul, M., Stuart, E. A., and Alexander, G. C. (2016). Effect of a pill mill law on opioid prescribing and utilization: The case of Texas. *Drug and alcohol dependence*, 159:190–197.

McDonald, D. C., Carlson, K., and Izrael, D. (2012). Geographic variation in opioid prescribing in the U.S. *The Journal of Pain: Official Journal of the American Pain Society*, 13(10):988–996.

Meara, E., Horwitz, J. R., Powell, W., McClelland, L., Zhou, W., O'Malley, A. J., and Morden, N. E. (2016). State Legal Restrictions and Prescription-Opioid Use among Disabled Adults. *New England Journal of Medicine*, 375(1):44–53.

Molitor, D. (2018). The Evolution of Physician Practice Styles: Evidence from Cardiologist Migration. *American Economic Journal: Economic Policy*, 10(1):326–356.

Morden, N. E., Munson, J. C., Colla, C. H., Skinner, J. S., Bynum, J. P. W., Zhou, W., and Meara, E. (2014). Prescription opioid use among disabled Medicare beneficiaries: intensity, trends, and regional variation. *Medical Care*, 52(9):852–859.

Muhuri, P., Gfroerer, J., and Davies, C. (2013). Associations of Nonmedical Pain Reliever Use and Initiation of Heroin Use in the United States. *CBHSQ Data Review*.

NCHS (2017). *Health, United States, 2016: With Chartbook on Long-term Trends in Health*.

Parente, S. T., Kim, S. S., Finch, M. D., Schloff, L. A., Rector, T. S., Seifeldin, R., and Haddox, J. D. (2004). Identifying controlled substance patterns of utilization requiring evaluation using administrative claims data. *The American Journal of Managed Care*, 10(11 Pt 1):783–790.

Pualozzi, L. J., Jones, C. M., Mack, K. A., and Rudd, R. A. (2011). Vital Signs: Overdoses of Prescription Opioid Pain Relievers - United States, 1999-2008. *MMWR. Morbidity and Mortality Weekly Report*, 60.

Rice, J. B., White, A. G., Birnbaum, H. G., Schiller, M., Brown, D. A., and Roland, C. L. (2012). A model to identify patients at risk for prescription opioid abuse, dependence, and misuse. *Pain Medicine*, 13(9):1162–1173.

Rudd, R. A., Seth, P., David, F., and Scholl, L. (2016). Increases in Drug and Opioid-Involved Overdose Deaths - United States, 2010-2015. *MMWR. Morbidity and mortality weekly report*, 65(5051):1445–1452.

Ruhm, C. J. (2018). Deaths of Despair or Drug Problems. *NBER Working Paper #24188*.

Rutkow, L., Chang, H.-Y., Daubresse, M., Webster, D. W., Stuart, E. A., and Alexander, G. C. (2015). Effect of Florida's Prescription Drug Monitoring Program and Pill Mill Laws on Opioid Prescribing and Use. *JAMA internal medicine*, 175(10):1642–1649.

Schnell, M. and Currie, J. (2017). Addressing the opioid epidemic: Is there a role for physician education? *American Journal of Health Economics*, pages 1–37.

Scully, R. E., Schoenfeld, A. J., Jiang, W., Lipsitz, S., Chaudhary, M. A., Learn, P. A., Koehlmoos, T., Haider, A. H., and Nguyen, L. L. (2018). Defining optimal length of opioid pain medication prescription after common surgical procedures. *JAMA Surgery*.

Shah, A., Hayes, C. J., and Martin, B. C. (2017). Factors Influencing Long-Term Opioid Use Among Opioid Naive Patients: An Examination of Initial Prescription Characteristics and Pain Etiologies. *The Journal of Pain*, 18(11):1374–1383.

Song, Y., Skinner, J., Bynum, J., Sutherland, J., Wennberg, J. E., and Fisher, E. S. (2010). Regional variations in diagnostic practices. *The New England Journal of Medicine*, 363(1):45–53.

SSA (2017). *Annual Statistical Supplement to the Social Security Bulletin, 2016*.

Sullivan, M. D., Edlund, M. J., Fan, M.-Y., Devries, A., Brennan Braden, J., and Martin, B. C. (2010). Risks for possible and probable opioid misuse among recipients of chronic opioid therapy in commercial and medicaid insurance plans: The TROUP Study. *Pain*, 150(2):332–339.

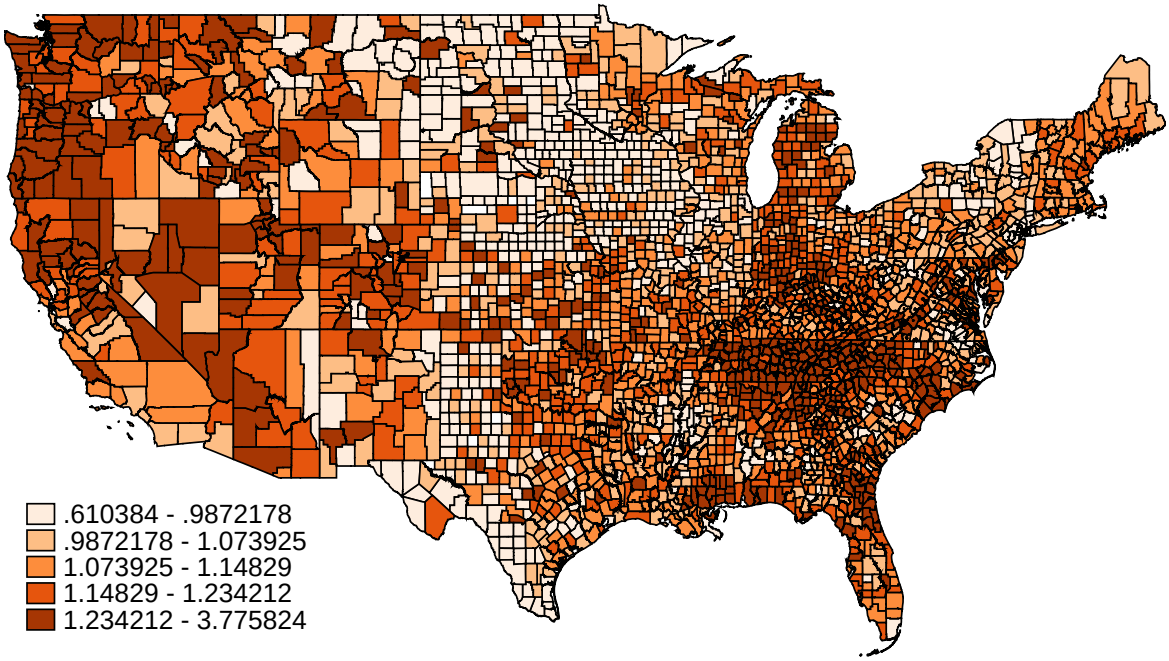
Sun, E. C., Darnall, B. D., Baker, L. C., and Mackey, S. (2016). Incidence of and Risk Factors for Chronic Opioid Use Among Opioid-Naive Patients in the Postoperative Period. *JAMA internal medicine*, 176(9):1286–1293.

Turk, D. C., Swanson, K. S., and Gatchel, R. J. (2008). Predicting opioid misuse by chronic pain patients: a systematic review and literature synthesis. *The Clinical Journal of Pain*, 24(6):497–508.

White, A. G., Birnbaum, H. G., Schiller, M., Tang, J., and Katz, N. P. (2009). Analytic models to identify patients at risk for prescription opioid abuse. *The American Journal of Managed Care*, 15(12):897–906.

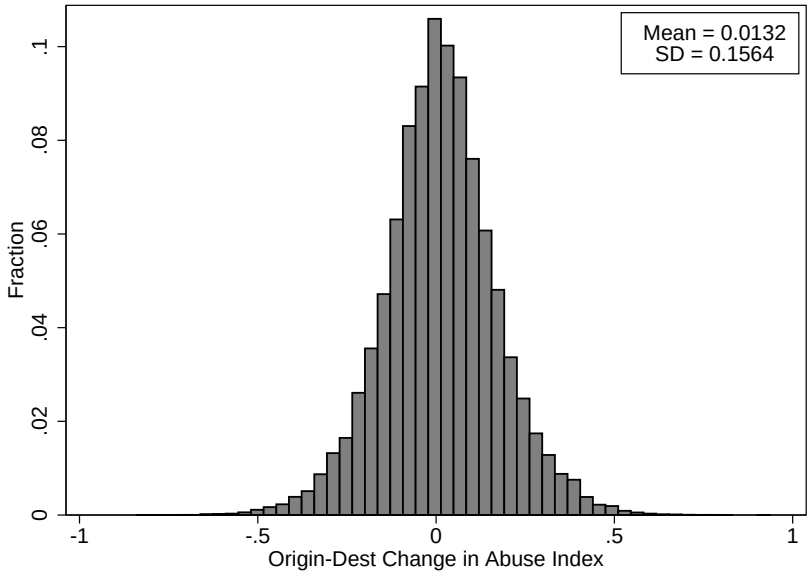
Yang, Z., Wilsey, B., Bohm, M., Weyrich, M., Roy, K., Ritley, D., Jones, C., and Melnikow, J. (2015). Defining risk of prescription opioid overdose: pharmacy shopping and overlapping prescriptions among long-term opioid users in medicaid. *The Journal of Pain: Official Journal of the American Pain Society*, 16(5):445–453.

Figure 1: Geographic Variation in Abuse Index



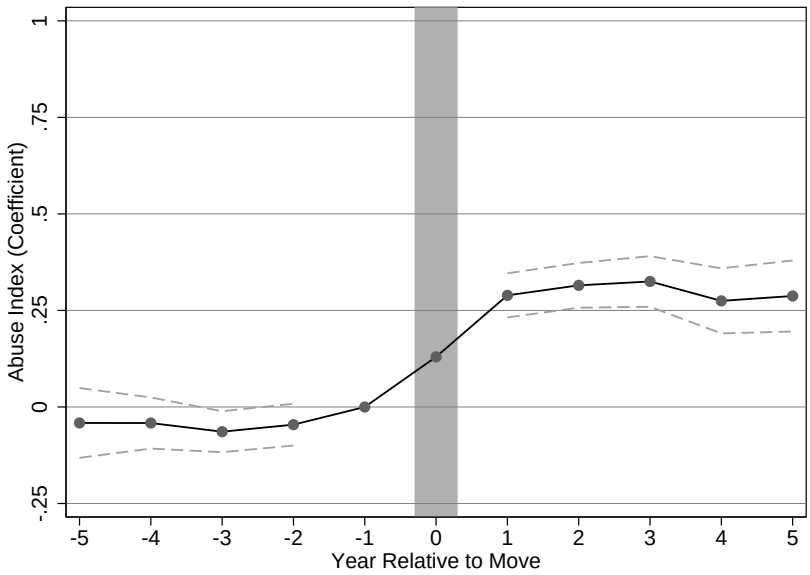
Notes: Figure reports county-level averages of our estimated abuse index (for movers and non-movers). Abuse index measures the predicted probability of a poisoning event in year t multiplied by 100, generated as a function of various measures of prescription opioid use and abuse in year $t-1$.

Figure 2: Distribution of Destination - Origin Difference in Abuse Index



Notes: Figure shows the distribution across movers of the difference in the average opioid abuse index between their origin and destination county ($\hat{\delta}_i$) The sample is all movers (N=102,649 enrollees).

Figure 3: Event Study



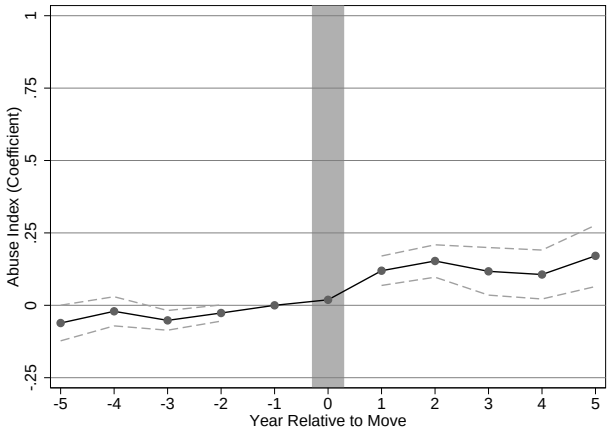
Notes: Figure shows the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the opioid abuse index between origin and destination counties. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is the opioid abuse index. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of its average opioid abuse index. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. The sample is all movers (N=461,614 enrollee-years).

Figure 4: Change in Abuse Index by Size of Move

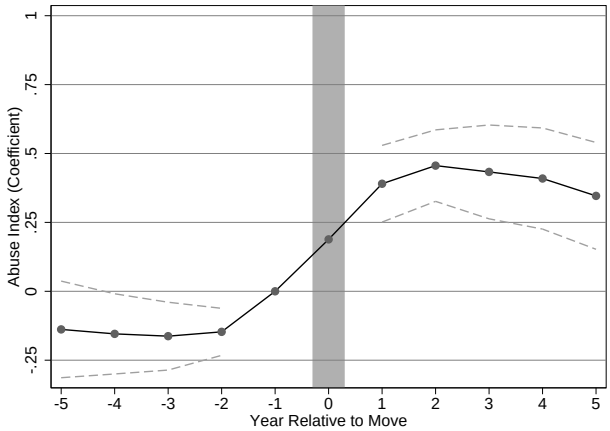
Notes: Figure shows change in abuse index before and after move. For each mover, we calculate the difference $\hat{\delta}_i$ in the abuse index between their origin and destination counties, then group the $\hat{\delta}_i$ into ventiles. The x-axis displays the mean of $\hat{\delta}_i$ for movers in each ventile. The y-axis shows, for each ventile, the average abuse index 2-4 years post-move minus the average abuse index 2-4 years pre-move, averaged within the ventiles. The line of best fit is obtained from a simple OLS regression using the 20 data points corresponding to movers, and its slope is reported in the graph. The sample is all mover-years between two and four years pre-move and two and four years post-move (N= 19,921 enrollee-years).

Figure 5: Event Studies - Naive and Prior Users

(a) Naive

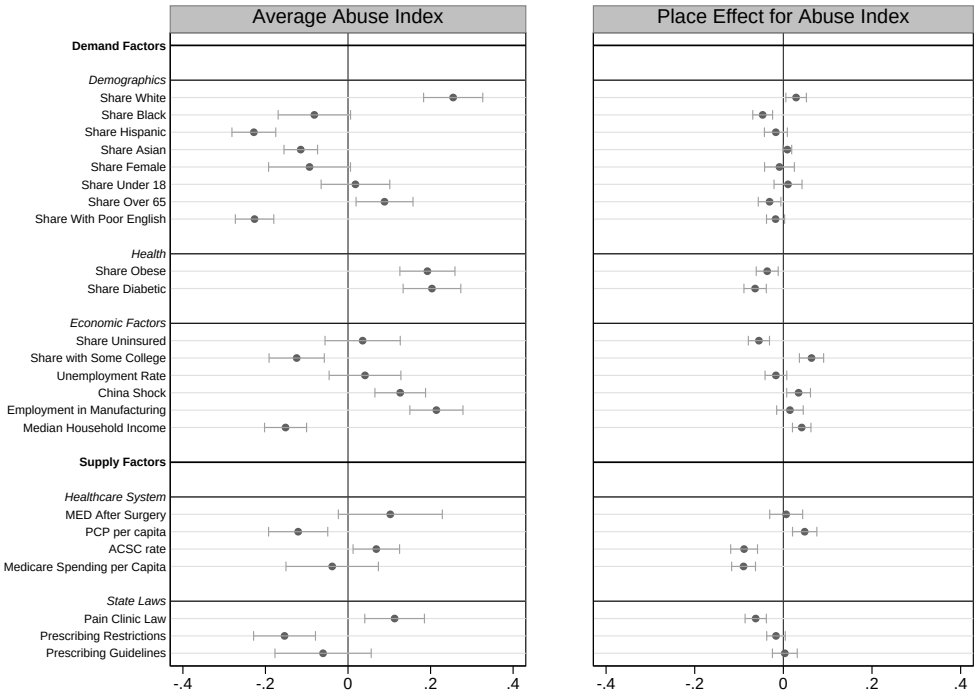


(b) Prior Users



Notes: Figure shows the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the abuse index between origin and destination counties - separately for opioid naive ("naive") and prior users. "Naives" are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. We omit the approximately 20% of enrollee-years with no observations in relative year -1. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is the opioid abuse index. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of its average opioid abuse index. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. Sample is 200,246 enrollee-years (naives) and 166,576 enrollee-years (prior users).

Figure 6: Correlates of Place Effects



Notes: Figure shows coefficients from bivariate regressions on the county-level average characteristic shown, with the regression weighted by the number of observations (movers and non-movers) in each county. Horizontal bars show the 95 percent confidence intervals, calculated with heteroskedasticity-robust standard errors. In the left-hand panel the dependent variable is the average abuse index in the county. In the right-hand panel the dependent variable is the estimated place effect for the abuse index in that county. Both outcomes are normalized to be in units of standard deviations, as are all the county-level average characteristics. "MED after surgery" refers to the average morphine equivalent dose (MED) of the prescriptions patients filled in the two weeks following a set of common surgical procedures. "ACSC rate" is the rate of hospitalizations for ambulatory-care sensitive conditions (ACSC); high ACSC rates are considered a measure of poor healthcare quality. Data for the following variables are missing for some counties: share of employment in manufacturing (79 counties), China Shock (22 counties), MED after surgery (964 counties), PCP per capita (137 counties), ACSC rate (176 counties), and Medicare Spending per capita (1 county).

Table 1: Summary Statistics

	(1) All	(2) Non-movers	(3) Movers
Female	52%	52%	54%
White	72%	72%	75%
Medicaid	66%	66%	71%
Age:			
< 40	13%	13%	16%
40 - 60	47%	47%	49%
> 60	40%	41%	35%
Average age	56.2	56.3	54.4
Region:			
Northeast	19%	19%	16%
West	16%	16%	19%
Midwest	24%	24%	24%
South	41%	41%	41%
Opioid use:			
Any Opioids	43.3%	43.0%	46.4%
High MED	4.2%	4.1%	4.7%
Many Prescribers	5.8%	5.6%	7.7%
Overlapping Prescriptions	15.0%	14.8%	17.1%
Abuse Index	1.1	1.1	1.2
Share with poisoning event	1.1%	1.1%	1.7%
Died in next year	3.4%	3.4%	2.8%
Share enrollees died during sample	19.4%	19.6%	17.2%
Number of enrollee-years	6,531,074	6,069,460	461,614
Number of enrollees	1,457,976	1,355,327	102,649

Notes: All rows, except for Abuse Index, "Share enrollees died during sample," Enrollee-years, and Enrollees, report the share of enrollee-years within the given population with the indicated characteristic. Specifically, "Region," "Medicaid," and "Age" are the average over all enrollee-years in the baseline sample. "Share with poisoning event" is calculated from 2006-2011.Enrollee-years within five years before and after the move year are included for movers. High MED is defined as having an average daily MED of at least 120 mg over at least one quarter in a year. Many Prescribers is defined as having at least four unique opioid prescribers. A poisoning event is defined as a non-fatal emergency room visit or inpatient admission with an ICD-9 diagnosis code corresponding to a poisoning.

Table 2: Event Study Coefficients - Abuse Index

	(1)	(2)	(3)
	All	Naive	Prior User
1 year post-move	0.289	0.119	0.390
	(0.029)	(0.026)	(0.071)
4 years post-move	0.275	0.106	0.409
	(0.043)	(0.043)	(0.094)
Enrollees	102,649	36,792	32,322
Enrollee-years	461,614	200,246	166,576
Abuse index 1 year pre-move	1.237	0.611	1.951

Notes: Table reports the coefficient and its bootstrapped standard error (in parentheses) in relative year 1 and relative year 4 for the baseline sample ("All"), naive enrollees, and prior users with the abuse index as the outcome variable. The omitted category is relative year -1. Opioid naive enrollees are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. Except in the baseline sample, we omit the ~20% of enrollee-years for enrollees with no observation in relative year -1. Standard errors are calculated using a bootstrap of 50 repetitions.

Table 3: Event Study Coefficients – Index Components

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline (Abuse Index)			
4 years post-move	0.275	0.106	0.409
	(0.043)	(0.043)	(0.094)
Enrollees	102,649	36,792	32,322
Enrollee-years	461,614	200,246	166,576
Panel B: High MED			
4 years post-move	0.255	0.040	0.583
	(0.051)	(0.040)	(0.116)
Enrollees	102,649	36,792	32,322
Enrollee-years	461,614	200,246	166,576
Panel C: Many Prescribers			
4 years post-move	0.483	0.185	0.783
	(0.093)	(0.095)	(0.217)
Enrollees	101,470	36,616	32,069
Enrollee-years	393,092	172,901	142,785
Panel D: Overlapping Prescriptions			
4 years post-move	0.220	0.128	0.316
	(0.048)	(0.048)	(0.095)
Enrollees	102,649	36,792	32,322
Enrollee-years	461,614	200,246	166,576

Notes: Table reports the coefficient and its bootstrapped standard error (in parentheses) in relative year 4 for the sample of all movers, naive enrollees, and prior users. Each panel reports results for a different outcome: baseline (abuse index), High MED, Many Prescribers, and Overlapping Prescriptions. The omitted category is relative year -1. Opioid naive enrollees are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. Except in the sample of all movers, we omit the ~20% of enrollee-years for enrollees with no observation in relative year -1. Standard errors are calculated using a bootstrap of 50 repetitions.

Table 4: Event Study Coefficients – Robustness

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline			
4 years post-move	0.275	0.106	0.409
	(0.043)	(0.043)	(0.094)
Enrollees	102,649	36,792	32,322
Enrollee-years	461,614	200,246	166,576
Panel B: Commuting Zone			
4 years post-move	0.361	0.130	0.538
	(0.060)	(0.069)	(0.132)
Enrollees	72,215	25,175	23,133
Enrollee-years	325,144	137,494	120,060
Panel C: State Movers Only			
4 years post-move	0.361	0.158	0.447
	(0.061)	(0.060)	(0.109)
Enrollees	35,525	14,149	13,875
Enrollee-years	173,755	78,375	74,263
Panel D: Part D Full Coverage			
4 years post-move	0.300	0.127	0.424
	(0.056)	(0.048)	(0.097)
Enrollees	63,276	25,002	22,011
Enrollee-years	317,610	146,256	121,271
Panel E: Never MA Coverage			
4 years post-move	0.213	0.128	0.311
	(0.046)	(0.045)	(0.100)
Enrollees	72,983	29,530	25,126
Enrollee-years	363,387	170,653	137,766
Panel F: Alive All Years			
4 years post-move	0.259	0.101	0.384
	(0.046)	(0.050)	(0.097)
Enrollees	88,478	31,618	27,103
Enrollee-years	412,757	179,813	146,619

Notes: Table reports the coefficient and its bootstrapped standard error (in parentheses) in relative year 4 for the sample of all movers, naive enrollees, and prior users. The table reports estimates for our baseline result, moves by commuting zone, restricting to cross-state movers, restricting to enrollees who had 12 months of Part D coverage all years 2006-2013, restricting to enrollees who were not on Medicare Advantage for any year 2006-2013, and restricting to enrollees who did not die in any year 2006-2014. There are 705 commuting zones in the sample. The omitted category is relative year -1. Opioid naive enrollees are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. Except in the baseline sample, we omit the ~20% of enrollee-years for enrolleees with no observation in relative year -1. Standard errors are calculated using a bootstrap of 50 repetitions.

Appendix A: Defining opioid abuse

Individual measures of opioid abuse

As discussed in the main text, clinicians and medical researchers have not come to a consensus on a gold standard measure of opioid abuse from claims data. However, the literature uses several proxies for likely opioid abuse based on apparent hazardous prescription patterns.

While the simplest measure of hazardous prescriptions is the number of opioid prescriptions a patient fills over a fixed time period (Rice et al. 2012), a more detailed measure of hazardous prescription behavior takes into account the strength, or morphine equivalent dose (MED), of the prescriptions. Several studies have found that patients with prescriptions that translate to a high average daily MED (usually above 100-120 mg) are at higher risk for diagnoses of opioid dependence (Sullivan et al. 2010; Edlund et al. 2014), ER visits associated with opioids (Braden et al. 2010), and overdoses (Bohnert et al. 2011). Other indicators for hazardous prescribing focus not only on the quantity of opioids prescribed, but also on a patient's method of obtaining the drugs. "Doctor shopping" and "pharmacy shopping," phenomena in which patients receive opioid prescriptions from multiple prescribers or pharmacies, also correlate with diagnoses of opioid dependence (White et al. 2009), hospitalizations (Jena et al. 2014), and overdose deaths (Yang et al. 2015; Hall et al. 2008).

We construct several measures of prescription opioid use and potential abuse. All the measures are at the enrollee-year level and are constructed using the Medicare Prescription Drug Events and the Pharmacy Characteristics files. The level of observation in the Prescription Drug Event file is an "event," or prescription fill, which we map to measures at the enrollee-year level. Each event is associated with an enrollee, the date filled, a national drug code (NDC), a prescribing physician, and the days of supply. We restrict to fills of drugs that contain at least one ingredient described in the MED conversion Table (Appendix Table A.2)

We define and separately analyze three indicator variables as proxies of abuse: Many Prescribers, High MED, and Overlapping Prescriptions. Many Prescribers and High MED are defined following Meara et al. (2016), and Overlapping Prescriptions is defined following Jena et al. (2014) and Logan et al. (2013). "Many Prescribers" is an indicator for whether an enrollee filled prescriptions associated with four or more unique physicians during the calendar year. "High MED" is an indicator for whether any quarterly MED is greater than 120 mg. To define it, we compute the MED for each quarter by multiplying the number of pills by their strength and the morphine equivalence, adding across all fills and ingredients, and then dividing by the number of days in each quarter. "Overlapping Prescriptions" is an indicator for whether the enrollee filled a new opioid prescription before her previous opioid prescription "ran out." To more effectively target hazardous overlaps, we use an approach similar to the prior studies and define this indicator so that it takes the value one only if the second opioid refill was either prescribed from a different doctor (indicating potential doctor shopping) or overlapped with the existing opioid prescription for more than one week (indicating potential use for non-medical purposes).

We also define two other indicators of prescription opioid use: an indicator for any opioid prescription ("any opioid") and an indicator for chronic opioid use. Following Morden et al. (2014), we define "chronic opioid use" as an indicator for whether the enrollee filled more than six prescriptions in one year.

These different opioid measures are highly correlated, as can be seen in Appendix Table A.3

which presents pairwise rank correlations across areas in our sample between these five different measures. They are also all more prevalent among individuals who have a subsequent poisoning event. To see this, Appendix Table A.4 shows the prevalence of each measure among the full population, and among the approximately one percent of the population that experiences an ER visit or inpatient hospital admission for a poisoning ("poisoning event"). The three measures we analyze separately are all about three times higher (or more) for the population that has a subsequent poisoning event. For example, about 5 percent of the population uses Many Prescribers, but among those with a poisoning event the next year it is 20 percent.

Opioid abuse index

To create a summary index of the relationship between poisoning events and the various measures of prescription opioid use, we estimate the following model on poisoning event data from $t = 2006$ to $t = 2010$[9]:

$$y_{ijt+1} = \alpha + \beta X'_{ijt} + \gamma_j + \varepsilon_{ijt+1},$$

The dependent variable, y_{ijt+1} is an indicator for whether an individual i in county j had an inpatient admission or ED visit related to a poisoning ("poisoning event") in year $t+1$[10]. This measure is intended to capture opioid overdose events, though due to limitations in the way the data are constructed, we cannot isolate these events from other poisonings. Our reliance on this measure as a welfare-relevant outcome resulting from opioid use follows previous work such as Meara et al. (2016). We use the more general "poisonings" as opposed to "drug poisonings" or "opioid poisonings" because different hospitals may differ in the specificity of their codings. On average, the data indicate that 80% of poisonings are "drug poisonings" and 25% of drug poisonings are "opioid poisonings."

Our key covariates - X'_{ijt} - are a vector of variables describing individual i's prescription behavior in year t. The variables include the five indicator variables mentioned above and shown in Appendix Table A.3. In addition, in order to not limit ourselves to these somewhat ad hoc binary cutoffs, we also include indicator variables for the number of prescribers, the number of opioid fills, and different bins of MED. Finally, the γ_j are county fixed effects, which we include to account for the fact that different counties may have higher or lower propensities for ER visits or inpatient admissions for the same underlying population (Finkelstein et al. 2016).

Our abuse index is defined as $\hat{\beta} X'_{ijt}$ plus the average of the estimated $\hat{\gamma}_j$ across all observations in the sample of non-movers used to generate the abuse index; it can be interpreted as the predicted probability of overdose next year multiplied by 100, given observed prescription patterns this year, for an individual in a representative county. The prescription variables that comprise X'_{ijt} and the estimated coefficients, $\hat{\beta}$ (the weights in our index), are presented in Appendix Table A.5. Our vector of prescribing behavior variables includes hazardous prescribing indicators commonly used in the clinical and public health literature as well as a number of other cutoffs to more flexibly

[9]Claims related to drugs and alcohol were masked by CMS beginning in 2012, so we are currently only able to observe hospitalizations related to drug or alcohol through 2011.

[10]We define y_{ijt+1}, hospital visits related to poisonings, using the ICD-9 diagnosis codes associated with each patient's emergency department (ED) visit or inpatient claims.

model the relationship between adverse outcomes and prescribing behavior. Many of the common hazardous prescribing indicators, particularly High MED and Many Prescribers, are highly correlated with poisoning hospitalizations.

Appendix Table A.3 shows the county-level rank correlation between our abuse index and the common indicators of hazardous prescribing that were inputs into the index's creation. As expected, our abuse index is highly correlated with the common indicators of hazardous prescribing that are inputs to it.

For some of our analyses we report results in terms of the impact of moving from a 25th to a 75th percentile county. Although our abuse index is highly correlated with each of the individual abuse measures we analyze, it is - not surprisingly given its construction - less geographically dispersed. Appendix Table A.1 shows the interquartile range for the abuse index and the individual abuse measures. The interquartile range for the abuse index is 0.2. By contrast, for the individual abuse measures the interquartile range is 3.5 to 8.2 percentage points.

Correlations with other populations

The foregoing results suggest that our various measures of opioid abuse are highly correlated within our population. We examine how geographic patterns in our population compare to the overall US population. We use 2006-2014 state-level data from the CDC Multiple Cause of Death (MCD) File[11] to construct a measure of opioid-related overdose deaths - i.e. deaths due to drug poisoning from heroin, other opioids, methadone, or other synthetic narcotics (such as fentanyl). We define the opioid death rate as these opioid-related deaths as a share of the population. We also measure self-reported opioid misuse rates in the 2006-2013 National Survey on Drug Use and Health (NSDUH)[12] as the share of the adult population who responded "yes" to "non-medical use of pain relievers in the past year." At the state level, both measures have a high rank correlation with our opioid abuse index; this rank correlation is 0.54 for the opioid-related death rate, and 0.72 for the self-reported opioid misuse rate.

We also compare national trends and geographic patterns of opioid prescription rates in our population to the general US population. To do so, we obtain data on opioid prescription fills per capita from county- and state-level averages of QuintilesIMS opioid prescription data, which are made publicly available by the Centers for Disease Control. QuintilesIMS collects data on prescriptions based on a sample of 59,000 retail pharmacies, which collectively dispense nearly 88% of all prescriptions in the U.S (CDC 2017d). The aggregated QuintilesIMS data set contains the number of opioid prescriptions per capita in each year.[13] We define an identical variable in our data, the number of opioid prescription fills per capita (Opioid Fills). Both are defined over the 2006-2013 time period.

Appendix Figure A.5 shows that national trends in opioid prescriptions per capita have evolved similarly in our sample and in the general US population, although with a substantially higher level for the disabled population. Our measure of opioid fills for our SSDI population and the QuintilesIMS measure of opioid fills for the general population are also highly correlated across geographies, with a correlation coefficient is 0.65 at the county level (weighted by 2010 census population) and 0.80 at the state level.

[11] Available at https://wonder.cdc.gov/mcd-icd10.html.

[12] Available at http://datafiles.samhsa.gov/study-series/national-survey-drug-use-and-health-nsduh-nid13517.

[13] Available at https://www.cdc.gov/drugoverdose/maps/rxrate-maps.html.

Appendix B: County-level characteristics

We construct measures of county-level characteristics from several sources.We briefly describe the measures and their sources here. Appendix Table A.6 shows the mean and standard deviation across counties (with each county weighted equally).

Demographics

Race (share white, black, Hispanic, Asian), gender, and age (share under 18 and share over 65) in each county are all from the 2012 Census population estimates. The percentage of the county population that is not proficient in English comes from the 2008-2012 American Community Survey (ACS). All of these demographics are population-wide measures.[14]

Health

Share Obese is defined as the share of adults in each county that report a BMI greater than or equal to 30; share diabetic is defined as the share of adults who self-reported being diagnosed with diabetes in 2010. Both obesity and diabetes were determined from the Behavioral Risk Factor Surveillance System (BRFSS), a yearly state-based telephone survey of the non-institutional adult population. The data are compiled and maintained by the CDC Division of Diabetes Translation.[15]

Economic Factors

Share uninsured is the estimated share of the population without insurance, taken from the Census Bureau's 2011 Small Area Health Insurance Estimates.[16] Share with some college is the percent of adults over 25 with some post-secondary education in each county, taken from the 2008-2012 American Community Survey.[17] The unemployment rate is the percent of the population age 16+ unemployed but seeking work in 2012, published by the Bureau of Labor Statistics.[18] China Shock is Autor et al.'s (2013) measure of exposure to Chinese imports per worker. The variable is defined at the commuting zone (CZ), which are aggregates of counties, and uses variation in community zone employment share in industries that are more or less affected by Chinese imports. Specifically, the China Shock is defined by:

$$\Delta IPW_{uit} = \sum_j \frac{L_{ijt}}{L_{ujt}} \frac{\Delta M_{ujt}}{L_{it-1}}$$

where i indexes CZs, j indexes industries, and t indexes time periods. L_{ijt} is CZ i's employment in industry j, L_{ujt} is the total U.S. employment in that industry, and L_{it} is the total employment in CZ i, all in time period $t = 2000$. ΔM_{ucjt} is the realized imports from China to the US in industry j from time periods t (2000) to $t+1$ (2007). Autor et al. (2013) explains this variable in more detail. In essence, the variable measures the changes in Chinese imports exposure per worker in

[14]These data can be downloaded at https://www.census.gov/programs-surveys/acs/data/summary-file.html.

[15]These data can be downloaded at https://www.cdc.gov/diabetes/data/countydata/countydataindicators.html.

[16]These data can be downloaded at https://www.census.gov/data/datasets/time-series/demo/sahie/estimates-acs.html.

[17]These data can be downloaded at https://factfinder.census.gov/ (Table S1501).

[18]These data can be downloaded at https://www.bls.gov/lau/#tables.

each CZ, where imports are apportioned to the region according to its share of national industry employment. We assign each county the China shock associated with that county's CZ.

Employment share in manufacturing is defined in 2000 and derived from the County Business Patterns Data.[19]

Median household income is measured in 2012 from the Census Bureau's Small Area Income and Poverty Estimates Program.[20]

Healthcare System

"MED after surgery" refers to the the average morphine equivalent dose (MED) of the prescriptions patients filled in the two weeks after a set of common general surgical procedures. We construct this measure ourselves from the 2006-2014 Medicare claims data for patients (both SSDI and over-65 eligibles) who had not filled an opioid prescription in the 1-30 days prior to the surgery. Our choice of procedures follows Hill et al. (2017), who documented wide variation in opioid prescriptions following several common outpatient surgical procedures at an academic medical center. To identify these surgeries in the claims data we use CPT codes identified by Scully et al. (2018). The surgeries we study are therefore limited to the overlap of procedures studied in the two papers, and consist of laparoscopic cholecystectomy, laparoscopic inguinal hernia repair, and open inguinal hernia repair. Under the (reasonable) assumption that patients do not demand unnecessary surgeries in order to obtain opioids, this measure provides us with a proxy for opioid supply in common surgical settings.

"PCP per capita" measures the number of primary care physicians (PCPs) practicing in the county as a share of the county's total population in 2011, taken from the Area Health Resources Files from information provided by the American Medical Association Masterfile.[21]

"ACSC Rate" is the rate of hospitalizations for ambulatory-care sensitive conditions (ACSC) per Medicare enrollee reported in the 2011 Dartmouth Atlas.[22] ACSCs are preventable conditions (with good outpatient care); high ACSC rates are generally associated with poor healthcare quality (Billings 2003).

"Medicare Spending per Capita" is the total Part A and Part B Medicare spending per Medicare enrollee at the county level, reported by the 2011 Dartmouth Atlas of Health Care.[23]

State Laws

We examine three types of state laws related to opioid prescribing: pain clinic laws, prescribing restrictions, and prescribing guidelines.[24]

Pain Clinic Law is an indicator that takes the value one if a county's state had passed a law regulating pain management clinics by December 1, 2016.

Prescribing Restrictions is an indicator that takes the value one if a county's state had passed a law restricting physician dispensing of opioids (i.e. restricting the amount the physician can

[19]These data can be downloaded at https://www.census.gov/programs-surveys/cbp/data/datasets.html.

[20]These data can be downloaded at https://www.census.gov/programs-surveys/saipe.html.

[21]These data can be downloaded at https://datawarehouse.hrsa.gov/data/datadownload.aspx#MainContent_ctl00_gvDD_lbl_dd_topic

[22]These data can be downloaded at http://www.countyhealthrankings.org/.

[23]These data can be downloaded at http://www.countyhealthrankings.org/.

[24]These data are available at http://www.pdaps.org/.

dispense, which patients a physician can dispense to, and restricting how physicians may charge for dispensed drugs) as of October 1, 2017.

Prescribing Guidelines is an indicator that takes the value one if a county's state had written a set of prescribing guidelines for when to prescribe opioids in emergency room or acute care settings as of July 1, 2017. In many but not all cases, these guidelines are not accompanied by an associated penalty for failure to comply.

Appendix C: Substitution to illegal opioids and adverse events

We cannot directly examine illegal opioid use in our data. However we conduct two suggestive exercises which are consistent with an impact of moving to a higher prescription opioid abuse county on total opioid use. This of course does not preclude the existence of quantitatively important substitution from prescription opioids to illegal forms of opioids, but it is suggestive that any such substitution is not fully offsetting.

First, the existing literature has documented substitution from prescription opioids to heroin, but little evidence of substitution from heroin to prescription opioids (Muhuri et al. 2013; Compton et al. 2016). Therefore, if substitution was an important factor we would expect to see bigger effects on prescription opioid abuse from moves down - from higher opioid abuse counties to lower opioid abuse counties - as users substitute towards heroin. We would expect more muted effects on prescription opioid abuse from moves up - from lower opioid abuse counties to higher opioid abuse counties. However, the results in Appendix Table A.7 suggest the opposite, if anything, with bigger impacts on prescription opioid abuse for moves up than moves down.

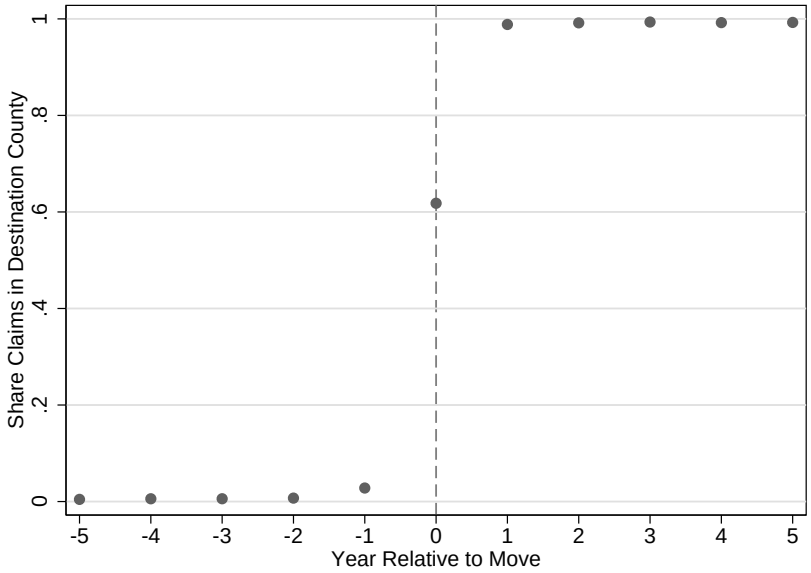
Second, we can look at whether moves across counties with different rates of prescription opioid abuse are associated with changes in hospital admissions and ER visits that are related to opioids.[25] To do so, we re-estimate the event study equation (1) with the outcome defined as a "poisoning event" - specifically a non-fatal inpatient admission or ED visit related to a poisoning (based on ICD-9 diagnosis codes) - and the measure of the "size of the move" - $\hat{\delta}_i$ - based on the abuse index. The analysis thus asks whether moving to a county with a higher rate of prescription opioid abuse is associated with a higher likelihood of a poisoning event. We use the more general "poisonings" as opposed to "drug poisonings" or "opioid poisonings" because different hospitals may differ in the specificity of their codings. On average, the data indicate that about 80% of poisonings are "drug poisonings" and about one-quarter of drug poisonings are are "opioid poisonings".

Data constraints currently allow us to perform this analysis through 2011, although we hope to be able to extend it in future work. The results - shown in Appendix Table A.8 and Appendix Figure A.6 - are fairly noisy and inconclusive; presumably due to the low frequency of poisoning events as well as the limited years of data availability. Nonetheless, the point estimates are consistent with moves to higher prescription opioid counties being associated with higher rates of poisoning events, particularly for prior users. For example, a move from a 25th to 75th percentile county in the opioid abuse index increases the probability of a poisoning event by 0.25 percentage points (about 25 percent relative to the mean county poisoning event rate); for prior users, it increases the probability of a poisoning event by 0.73 percentage points.

[25] Opioid-related deaths are not possible to study with our approach: they are exceedingly rare, cannot be analyzed in a panel, and CMS stopped providing cause of death data after 2008.

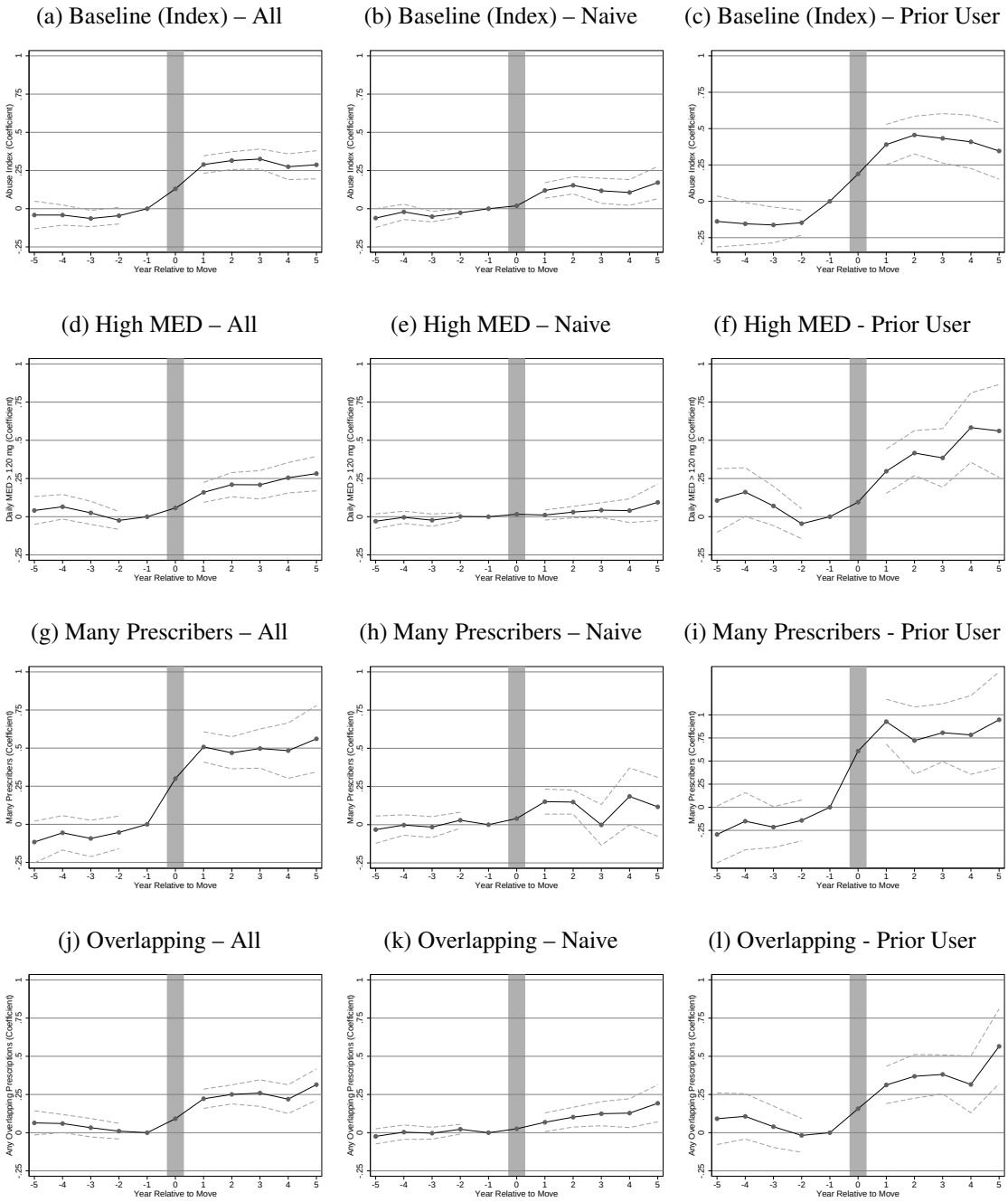
Appendix D: Appendix Figures and Tables

Figure A.1: Share of Claims in Destination County by Relative Year



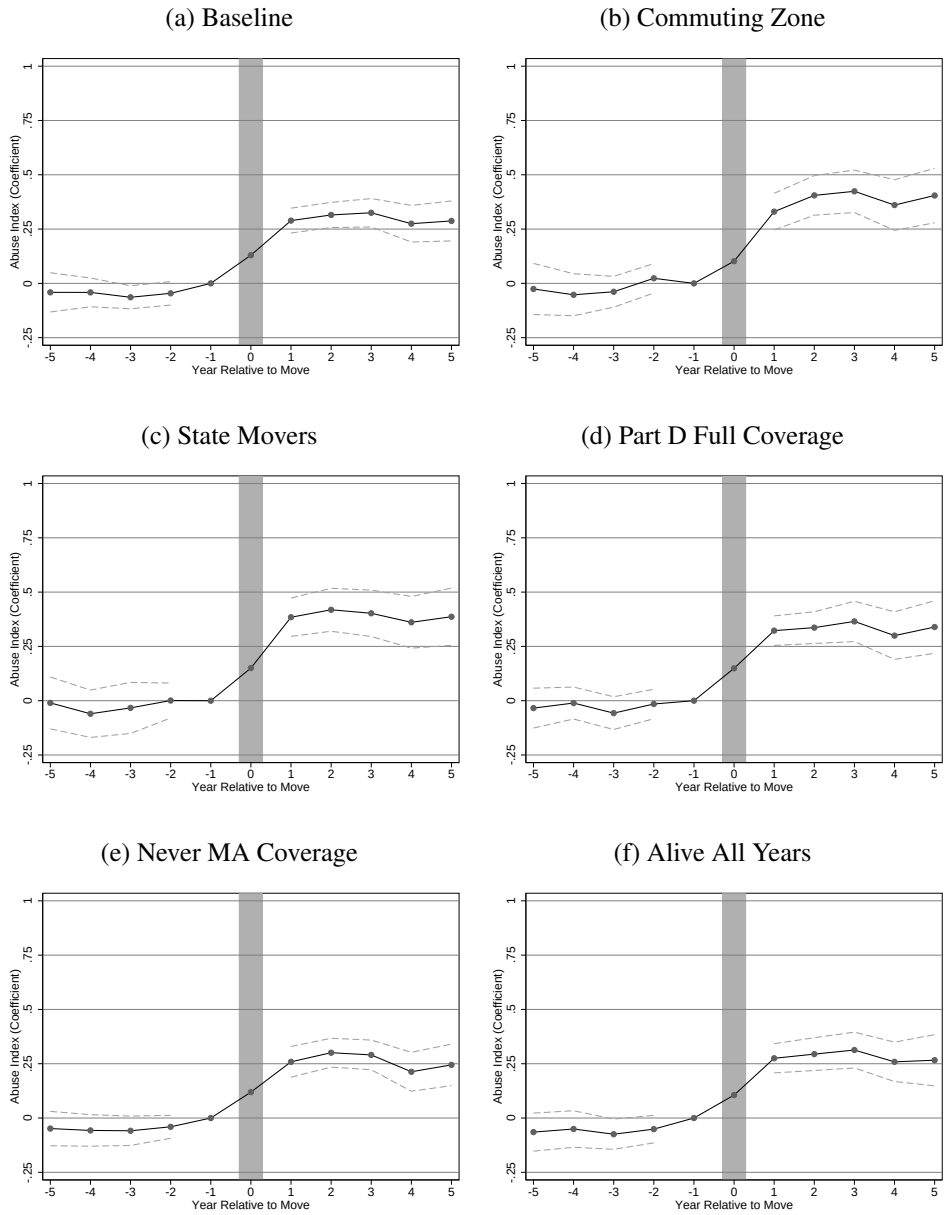
Notes: Figure presents the average fraction of claims in a mover's destination county (as a fraction of claims in either the origin or destination county) by year relative to move. Observations are at the mover-year level. The figure shows a sharp change in the year of the move, with only a small share of claims in the destination pre-move or in the origin post-move. The claim share in the year of the move (relative year zero) is close to 0.5, consistent with moves being roughly uniform throughout the year.

Figure A.2: Event Studies - Other Outcomes



Notes: Figures show the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the indicated opioid use measure between origin and destination counties. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is the indicated opioid use measure. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of the average opioid use measure. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. Panels (a) through (c) report the baseline specifications using the abuse index as an outcome for all movers, opioid naives, and prior users, respectively. Panels (c) through (l) report the results with commonly used abuse measures – High MED, Many Prescribers, and Overlapping Prescriptions. Sample sizes are given in Table 3.

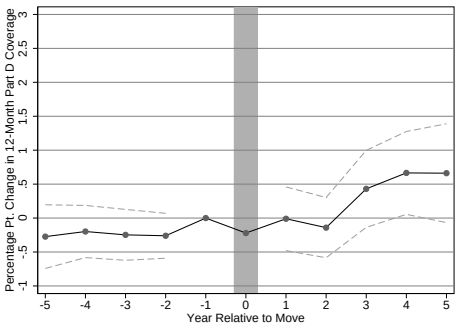
Figure A.3: Event Studies - Robustness



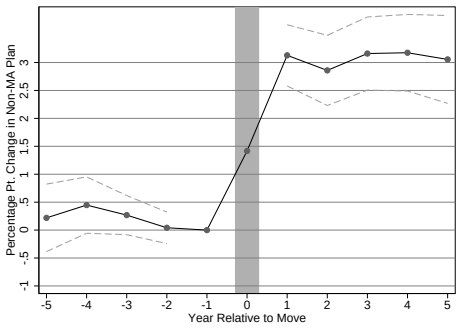
Notes: Figures show the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the abuse index between origin and destination counties. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is the opioid abuse index. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of its average opioid abuse index. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. Panel (a) is our baseline specification (N = 461,614 enrollee-years). Panel (b) uses commuting zones as the moving geography (N = 325,144 enrollee-years). Panel (c) restricts to movers who change states (173,755 enrollee-years). Panel (d) restricts to movers with 12 months of Part D coverage for each year they are observed in the sample (317,610 enrollee-years). Panel (e) restricts to all movers who had 12 months of traditional Medicare coverage for each year they are observed in the sample (N = 363,387 enrollee-years), and panel (f) restricts to all movers who did not die from 2006-2014 (N = 412,757 enrollee-years).

Figure A.4: Event Studies - Selection and Attrition

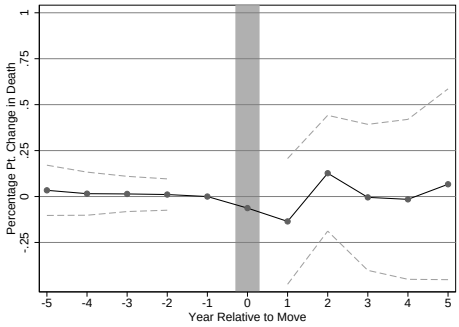
(a) Part D Extensive Margin



(b) Traditional Medicare Coverage

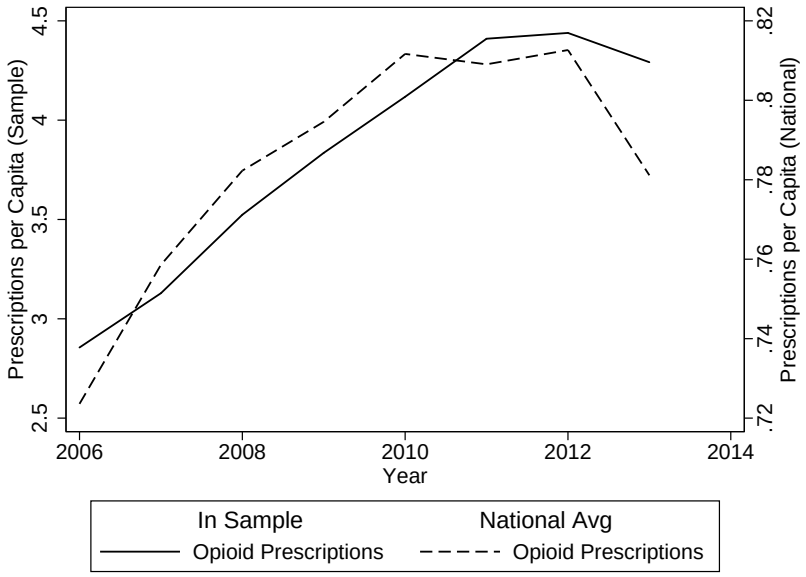


(c) Died



Notes: Figures shows the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the opioid abuse index between origin and destination counties. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is an indicator for having twelve complete months of Part D coverage during the year, an indicator for being on traditional Medicare coverage, and an indicator for death during the year. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of its average opioid abuse index. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. The y-axis is scaled to represent a 25th percentile to 75th percentile move in the abuse index. The sample in panel (a) is all movers, in addition to those without 12 complete months of Part D coverage (N = 593,869 enrollee-years). The sample in panel (b) is all movers, in addition to those not on traditional Medicare coverage (N = 550,926 enrollee-years). The sample in panel (c) is all movers, in addition to those who die during their year of death (N = 472,933 enrollee-years).

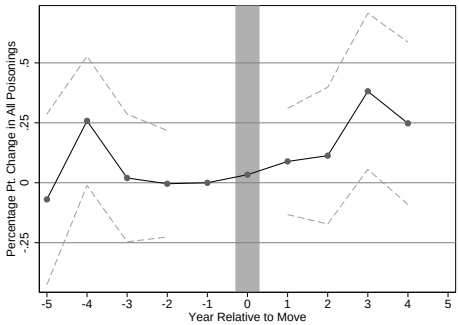
Figure A.5: Time Trends in Opioid Prescribing Rates



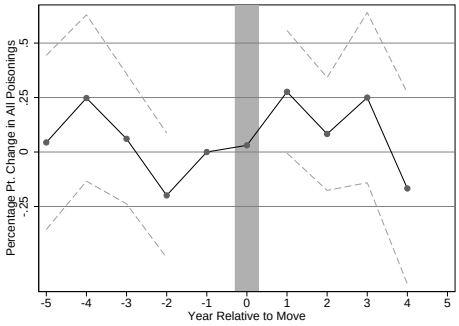
Notes: Figure presents averages over time for 2006-2013 of the number of prescriptions per capita in our sample (movers and non-movers) and from national QuintilesIMS data. Both prescription rates are calculated on a January-December calendar year.

Figure A.6: Event Study – Poisoning Event

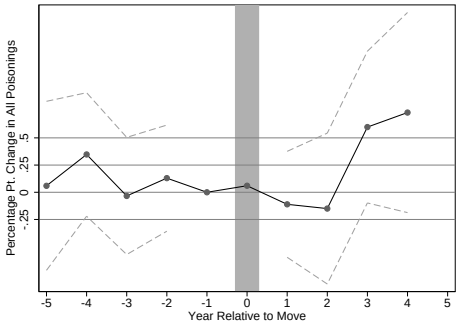
(a) All Users



(b) Naive Users



(c) Prior Users



Notes: Figures show the estimates from equation (1) of $\theta_{r(i,t)}$ - the coefficient on year relative to move interacted with the average difference in the abuse index between origin and destination counties. The coefficient for relative year -1 is normalized to 0. The dependent variable y_{it} is an indicator for whether an individual experienced a non-fatal poisoning event in that year, defined as an ER visit or inpatient admision with an ICD-9 diagnosis code indicating a poisoning. Values on the y-axis refer to the percentage point change in a poisoning event associated with a move from the 25th to 75th percentile county with respect to the opioid abuse index. The dashed lines are upper and lower bounds of the 95% confidence interval, constructed based on 50 repetitions of the boostrap following a two-step procedure. First, for each county, we construct the asymptotic distribution of its average opioid abuse index. In the second step, we bootstrap equation (1) with 50 repetitions drawn at the enrollee level, making a random draw from the asymptotic distibution for each mover's origin and destination to construct their $\hat{\delta}$ for each repetition. The sample is all movers between 2006-2011: 244,084 enrollee-years (all), 108,184 enrollee-years (naive), and 85,839 enrollee-years (prior users).

Table A.1: County-Level Opioid Prescribing Measure Summary Statistics

	25th Percentile	Median	75th Percentile	Interquartile Range
Abuse Index	1.000	1.103	1.208	0.208
High MED	0.018	0.035	0.053	0.035
Many Prescribers	0.031	0.046	0.064	0.033
Overlapping Prescriptions	0.105	0.144	0.187	0.082

Notes: Table presents the 25th percentile, median, and 75th percentile of county averages of the indicated opioid prescription measure. Each county average is determined by averaging the opioid prescription measure outcome within a year across non-movers and then taking a simple average of that across years in the sample. The final column presents the interquartile range of each opioid prescription measure to represent the difference between the 75th percentile and 25th percentile county.

Table A.2: Morphine Equivalents Conversion Table

Opioid Active Ingredient	Morphine Equivalents per Milligram
Codeine	0.15
Dihydrocodeine	0.25
Fentanyl (transdermal)	2.4
Fentanyl (oral)	0.1
Hydrocodone	1
Hydromorphone	4
Levorphanol	12
Meperidine	0.1
Methadone	4
Morphine	1
Oxycodone	1.5
Oxymorphone	3
Pentazocine	0.3
Propoxyphene	0.6
Tapentadol	0.367

Notes: This table identifies opioid conversion factors used in construction of the High MED variable. Adapted from Meara et al. (2016) supplementary material as well as http://www.agencymeddirectors.wa.gov/calculator/dosecalculator.htm.

Table A.3: Measures of Hazardous Opioid Prescriptions

	High MED	Many Prescribers	Overlapping Prescriptions	Any Opioid	Chronic Opioid Use
High MED	1.000				
Many Prescribers	0.546	1.000			
Overlapping Prescriptions	0.738	0.755	1.000		
Any Opioid	0.532	0.651	0.914	1.000	
Chronic Opioid Use	0.606	0.619	0.947	0.957	1.000
Abuse Index	0.718	0.908	0.938	0.854	0.843

Notes: Table presents pairwise rank correlations between the High MED, Many Prescribers, Overlapping Prescribers, Any Opioid, and Chronic Opioid Use variables for 2006-2013 (excluding 2012, since we do not have 2012 data for Many Prescribers), averaged across years at the county level. The last row presents the county-level correlation of the abuse index with the other measures of abuse in the sample from 2006-2013 (excluding 2012, since we do not have 2012 data for Many Prescribers). County level correlations are weighted by the 2010 Census population to account for noisy measures from small counties.

Table A.4: Hazardous Prescribing for Enrollees with Adverse Outcomes (Lagged)

	Overall	Poisoning Event
High MED	0.036	0.117
Many Prescribers	0.052	0.201
Overlapping Prescribers	0.136	0.359
Any Opioids	0.410	0.665
Chronic Opioid Use	0.191	0.424
MED	13.7	36.3
Prescribers	0.8	2.1
Opioid Fills	3.205	7.714
Share of Non-Movers		0.011
Number of Enrollee-Years	3,147,316	33,635

Notes: The first column of this table presents the average of each prescribing measure in year $t-1$ among all enrollee-years for non-movers observed in year t from 2007 to 2011. The second column presents the average of the same prescribing measures in year $t-1$ among all enrollee-years for non-movers who had a poisoning event in year t from 2007 to 2011. A poisoning event is defined as a poisoning-related emergency department or inpatient visit. The sample for this table consists of non-movers from t =2006 to 2011.

Table A.5: Multivariate Regression of Poisoning Event on Prescribing Measures

	Coef.	S.E.
Overlapping Prescriptions	0.003	(0.000)
High MED	0.010	(0.001)
MED:		
$(0,1]$	0.013	(0.000)
$(1,5]$	0.009	(0.001)
$(5,10]$	0.008	(0.001)
$(10,20]$	0.009	(0.001)
$(20,30]$	0.011	(0.001)
$(30,50]$	0.012	(0.001)
$(50,100]$	0.015	(0.001)
$(100,120]$	0.007	(0.001)
$(120,200]$	0.011	(0.001)
> 200	0.008	(0.001)
Prescribers:		
1	-0.009	(0.000)
2	-0.005	(0.000)
3	-	-
4	0.006	(0.001)
5	0.013	(0.001)
6	0.020	(0.001)
≥ 7	0.043	(0.001)
Opioid Fills:		
1	-	-
2	0.001	(0.000)
3	0.002	(0.001)
4	0.002	(0.001)
5	0.002	(0.001)
$(5,10]$	0.002	(0.001)
$(10,15]$	0.002	(0.001)
$(15,20]$	0.005	(0.001)
> 20	0.007	(0.001)

Notes: Table presents the coefficient, OLS standard errors, and the t-statistics for each dependent variable in a multivariate regression of poisoning events on hazardous prescribing outcomes from the previous year, based on the sample of all enrollee-years for non-movers from 2006-2011 (N = 3,147,316). A poisoning event is defined as a poisoning-related emergency department or inpatient visit. The omitted category for MED, prescribers, and opioid fills is zero. Estimates for the rows with dashes are omitted due to multicollinearity, and the regression includes a constant (that is not reported). Poisoning events are measured in year t and prescribing measures are measured in year $t-1$.

Table A.6: County-Level Place Covariate Summary Statistics

	Mean	S.D.
Demographics		
Share White	0.781	(0.198)
Share Black	0.089	(0.144)
Share Hispanic	0.085	(0.133)
Share Asian	0.012	(0.025)
Share Female	0.500	(0.022)
Share Under 18	0.229	(0.033)
Share Over 65	0.168	(0.043)
Share Bad English	0.018	(0.029)
Health		
Share Obese	0.306	(0.043)
Share Diabetic	0.108	(0.023)
Economic Factors		
Share Uninsured	0.179	(0.054)
Share with Some College	0.551	(0.117)
Unemployment rate	0.077	(0.028)
China Shock	2.812	(2.754)
Employment in Manufacturing	0.190	(0.147)
Median Household Income	44,786	(11384)
Healthcare System		
MED After Surgery	180	(154.0)
PCP per Capita (per 100,000)	55.5	(33.8)
ACSC Rate (per 1,000)	76.5	(29.9)
Medicare Spending per Capita	9,451	(1576.0)
State Laws		
State has pain clinic law	0.359	(0.480)
State has prescribing restriction law	0.254	(0.435)
State has prescribing guidelines	0.696	(0.460)

Notes: Table reports the mean and standard deviation (shown in parentheses) of each correlate of the place effects outlined in Appendix B.

Table A.7: Event Study Coefficients – Moves Up and Moves Down

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Moves Up			
4 years post-move	0.466	0.173	0.560
	(0.039)	(0.070)	(0.252)
Enrollees	54,967	19,984	16,822
Enrollee-years	246,573	108,769	86,788
Panel B: Moves Down			
4 years post-move	0.193	-0.003	0.126
	(0.043)	(0.093)	(0.219)
Enrollees	47,682	16,808	15,500
Enrollee-years	215,041	91,477	79,788

Notes: Table reports the coefficient and its bootstrapped standard error (in parentheses) in relative year 4 for the baseline sample ("All"), naive enrollees, and previous users. The table reports estimates for moves up and moves down based on the difference between the average non-mover opioid abuse index between the origin and destination county. The omitted category is relative year -1. Opioid naive enrollees are those with no opioid use in relative year -1, while previous users filled at least one opioid prescription in that year. Except in Column (1), we omit the ~20% of enrollee-years for enrollees with no observation in relative year -1. Standard errors are calculated using a bootstrap of 50 repetitions.

Table A.8: Event Study - Poisoning Event

	(1)	(2)	(3)
	All	Naive	Prior User
1 year post-move	0.089	0.276	-0.111
	(0.113)	(0.144)	(0.248)
4 years post-move	0.248	-0.167	0.733
	(0.173)	(0.224)	(0.469)
Enrollees	63,069	23,371	19,199
Enrollee-years	244,084	108,184	85,839

Notes: Table reports the coefficient and its bootstrapped standard error (in parentheses) in relative year 1 and relative year 4 for the baseline sample ("All"), naive enrollees, and prior users with $\hat{\delta}$ calculated using the opioid abuse index. The outcome is an indicator for whether an individual experienced any non-fatal poisoning event, defined as an ER visit or inpatient admission with an ICD-9 diagnosis code indicating a poisoning. Coefficients are scaled to represent a 25th to 75th percentile move in the abuse index. We restrict to the years 2006-2011. The omitted category is relative year -1. Opioid naive enrollees are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. Except in the baseline sample, we omit the ~20% of enrollee-years for enrollees with no observation in relative year -1. Standard errors are calculated using a bootstrap of 50 repetitions.