What Drives Risky Prescription Opioid Use?
Evidence from Migration

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Abstract

We investigate the role of person- and place-specific factors in the opioid epidemic by developing and estimating a dynamic model of risky prescription opioid use. We estimate the model using the relationship between cross-state migration and risky use among adults receiving federal disability insurance from 2006 to 2015. Event studies suggest that moving to a state with a 3.5 percentage point higher rate of risky use (roughly the difference between the 20th and 80th percentile states) increases the probability of risky use by 1.0 percentage point on-impact, followed by an additional increase of 0.30 percentage points per subsequent year. Model estimates imply large place effects in both the likelihood of transitioning to addiction and the availability of prescription opioids. A one standard deviation reduction in all place effects would have reduced risky use by two-thirds over our study period. Reductions in place effects on addiction have a larger cumulative effect than analogous reductions in place effects on availability. However, their relative efficacy is reversed in the first few years, suggesting a temporal tradeoff among policy options.

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1 Introduction

The opioid epidemic is one of the most important US public health crises of recent decades. In 2017, deaths from opioids were more than double the number of homicide deaths, and an order of magnitude higher than cocaine-related deaths at the height of the 1980s crack epidemic (United States General Accounting Office 1991; Kochanek et al. 2019; National Center for Health Statistics 2021). A distinguishing feature of the origin of the epidemic is the role played by legal prescription opioids such as oxycodone (Oxycontin) and hydrocodone (Vicodin). The opioid crisis emerged from physicians' increased willingness to prescribe legal opioids, which in turn was linked to pharmaceutical firms' marketing efforts and the medical profession's decision to recognize pain as a "fifth vital sign" (Quinones 2016; Case and Deaton 2020; Maclean et al. 2020; Alpert et al. 2022). There is wide geographic variation in the intensity of the opioid epidemic, with prescribing per capita varying by a factor of more than two between the 10th and 90th percentile states (McDonald, Carlson, and Izrael 2012; Schieber et al. 2019), and Appalachia, New England, and the West (Guy et al. 2017; Katz 2017) being particularly hard hit.

In this paper, we estimate a dynamic model of risky prescription opioid use ("risky use") that allows us to unpack the drivers of the prescription opioid epidemic along two key dimensions. First, we separate the roles of person-specific and place-specific factors. Person-specific factors include characteristics such as age, smoking, childhood adversity, mental health status, and prior substance use, whose importance has been emphasized in the medical literature (Ives et al. 2006; Sullivan et al. 2010; Fischer et al. 2012; Webster 2017). They also include individual human capital, which may be a contributor to what Case and Deaton (2015; 2017; 2020) 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, Olenski, and Jena 2017; Schnell and Currie 2018; Eichmeyer and Zhang, forthcoming), the availability of unscrupulous providers and "pill mill" pain clinics (Rutkow et al. 2015; Lyapustina et al. 2016), and the presence of policies such as triplicate prescription programs (Alpert et al. 2022).[1]

Second, we distinguish between place factors that impact the likelihood that individuals transition in and out of addiction and place factors that influence the ease with which addicted individuals can engage in risky use. The former, which we refer to as the *addiction channel*, would include factors like the willingness of physicians to give first-time prescriptions to patients suffering from pain, and it would be particularly influenced by policies like prescribing limits for such opioid "naive" patients (Sacks et al. 2021). The latter,

[1] Other examples of policies include naloxone access laws (Doleac and Mukherjee 2018; Rees et al. 2019), opioid prescribing limits (Sacks et al. 2021), and prescription monitoring programs intended to limit risky use (Kilby 2015; Meara et al. 2016; Buchmueller and Carey 2018; Kaestner and Ziedan 2019). Area-specific peer effects (Khan et al. 2019; Powell, Pacula, and Taylor 2020) will also be captured as place factors in our framework.

which we refer to as the *availability channel*, would include factors like the presence of pill mills, and it would be particularly affected by policies such as prescription monitoring programs (Kilby 2015; Meara et al. 2016; Buchmueller and Carey 2018; Kaestner and Ziedan 2019) and pill mill laws (Rutkow et al. 2015; Lyapustina et al. 2016). Decomposing the role of these different place-based factors is important for understanding how different public policies could have affected the epidemic.

In our model, an individual may transition in or out of addiction with probabilities that depend on both person- and place-specific factors. Whether or not she engages in risky use depends on her addiction status as well as place-specific availability factors. The model yields rich predictions for variation in the dynamics of the epidemic across space and time. It suggests that changes in the rates of risky use when people move across areas can identify the relative importance of person- and place-specific factors, while the time pattern of those changes helps distinguish between the two different types of place factors. In particular, sharp, on-impact effects of moves provide information about the role of place-specific availability factors, while the evolution of risky use rates following moves provides information about the role of place-specific addiction factors.

We estimate the model using data on prescription opioid use from 2006 to 2015 among adults enrolled in the Social Security Disability Insurance (SSDI) program, the federal public insurance program for the disabled. Opioid use is especially prevalent in this population. Roughly half of SSDI recipients receive an opioid prescription each year (Morden et al. 2014; Meara et al. 2016), a rate considerably higher than the rest of the population. We take advantage of rich panel data on prescription drug use for about 2.4 million SSDI recipients. The fact that the SSDI population has a relatively fixed level of government benefits and extremely tight limits on additional earnings lets us rule out large changes in individual income or employment as drivers of behavior change around moves. At the same time, it may also attenuate the variation in economic circumstances whose role in the opioid epidemic has been highlighted by Case and Deaton (2015; 2017; 2020).

To define risky prescription opioid use, we follow a standard approach in the literature (e.g., Larrick 2014; Meara et al. 2016; Ciesielski et al. 2017) and measure risky use for each individual-year with an indicator for prescriptions that result in a daily morphine-equivalent dose (MED) of more than 120 mg in any calendar quarter. This measure has previously been found to be strongly correlated with adverse outcomes such as opioid poisonings and opioid dependence (Sullivan et al. 2010; Bohnert et al. 2011), and we confirm this is true in our sample as well.

We begin our analysis by discussing how event study estimates of changes in risky use when individuals move across state lines can be interpreted within the context of our dynamic model. Using a potential

outcomes framework, we show that under certain assumptions, the event study coefficients can be cleanly written in terms of our model parameters. We then use this derivation to discuss how patterns in these event study coefficients may provide intuition for the relative importance of the addiction and availability channels in our model, as well as the relative importance of person- and place-specific factors within these channels.

We find that individuals' probability of risky use increases immediately when they move to areas with higher rates of risky use, and falls immediately when they move to areas with lower risky use rates. These effects then increase in the years post-move. Viewed through the lens of our model, the on-impact effects of moving are consistent with a large role for place-specific factors that affect availability. The continued post-move convergence in risky use rates additionally suggests an important role for place-specific factors that affect addiction transitions. Furthermore, our model predicts that when both types of place-specific factors play important roles, moving will affect addicted and non-addicted individuals differently. Consistent with these predictions, when we proxy for an individual's addiction status with her prior opioid use, we find that for prior opioid users, moving to an area with higher rates of risky use is associated with a sharp on-impact increase in risky use along with further increases post-move, whereas for opioid naive patients, there is little or no on-impact change, but a steady increase post-move.

We then estimate the model by Generalized Method of Moments, using the average rate of risky use for each year before and after a move for about 12,500 separate mover cohorts as moments. Each mover cohort is defined by its origin and destination state, and by the calendar year in which the move occurs. Our empirical implementation allows for the possibility that movers are non-randomly selected, with initial addiction status and addiction propensities that may be correlated with those of non-movers in both their origin and their destination; the key identifying assumption is that the pattern of selection does not change systematically with the time relative to the move, and in particular that it does not change discretely on move. Predicted moments from the model align well with their empirical counterparts, and event studies simulated based on the estimated parameters closely match the observed event studies.

We present two main sets of results from the estimated model. First, we evaluate the impact of different channels in contributing to risky prescription opioid use over our 10-year study period. We find quantitatively important roles for both person-specific and place-specific factors, and among place-specific factors for both the availability and addiction channels. A one standard deviation reduction in the person-specific factors that contribute to risky use (initial addiction shares and person-specific addiction propensities) would have reduced the average rate of risky use during this time period by 64 percent.[2] Similarly, a one standard

[2]These person-specific factors in turn may have been shaped by the long-run impacts of place-specific factors. In that sense, one can think of our estimates of the role of place-specific factors as a lower bound on their impact.

deviation reduction in the place-specific factors (availability and place-specific addiction propensities) would have reduced the rate of risky use by 65 percent. Among the two place-specific factors, we identify a role for policies that reduce place-specific addiction propensities that is more than twice as large as the role of policies that reduce place-specific availability. A one standard deviation reduction in place-specific addiction propensities would have reduced risky use by 53 percent over the ten-year period, while a one standard deviation reduction in place-specific availability would have reduced this rate by 24 percent.

Second, we compare the dynamic impact of policies that reduce the place-specific availability channels to those that reduce the place-specific addiction channel. We find that a one standard deviation reduction in place-based addiction propensities would have a larger impact than a one standard deviation reduction in place-based availability over most years of our sample, but that this comparison is reversed in the earliest years. This suggests a temporal tradeoff in which policies targeting availability may be relative more effective in the very short run.

One important limitation to these conclusions is that the impacts we measure only capture changes in prescription opioid use; our data do not permit a direct examination of potential substitution to illegal forms of opioids such as heroin. Further analysis of the determinants of illegal opioid use is an important area for future work, particularly as illegal opioids have started to play a larger role in the epidemic, especially in more recent years beyond our study period (Maclean et al. 2020).[3] A second important limitation is that our focus on the SSDI population means that variation in income will be smaller in our setting than in the general population, limiting our ability to pick up the role of both person- and place-specific drivers of individual economic circumstances.

Our analysis relates to a large literature on the causes and consequences of the opioid epidemic. Most closely related is work examining the importance of person and place factors, much of it focusing on correlations between risky use and changes in other factors. Some researchers, including Case and Deaton (2015; 2017; 2020), have pointed to the important role of individuals' economic circumstances and related demand-side factors (Carpenter, McClellan, and Rees 2017; Hollingsworth, Ruhm, and Simon 2017; Dean and Kimmel 2019; Venkataramani et al. 2020). Others have argued that the patterns of increased drug deaths and risky use across states suggest an important role for the availability of drugs and little, if any, role for economic decline (Ruhm 2018; Currie, Jin, and Schnell 2019; Currie and Schwandt 2021; Cutler and Glaeser 2021).[4] Still others, such as Charles, Hurst, and Schwartz (2019), conclude that both economic

[3]For important work on the consequences of the availability of illegal opioids, see Evans, Lieber, and Power 2019 and Schnell 2022.

[4]Other work suggests that economic decline may be a result of risky use itself (Laird and Nielsen 2016; Krueger 2017; Aliprantis, Fee, and Schweitzer 2019; Harris et al. 2020).

conditions and opioid supply have played a role in local opioid use and deaths. To complement this work, we develop an explicit model of opioid addiction, and we use a different empirical strategy based on individuals moving across states, to decompose causal drivers.

Our strategy is similar to one that we have used in prior work to understand the determinants of geographic variation in the elderly's health care utilization (Finkelstein, Gentzkow, and Williams 2016),[5] and fits more broadly into a large and growing literature using changes in location to separate the effects of individual characteristics from the effects of geographical or institutional factors on a wide range of economic outcomes (e.g. Card, Heining, and Kline 2013; Chetty, Friedman, and Rockoff 2014; Chetty and Hendren 2018a,b). Most of this prior literature has assumed separability between person-specific and place-specific factors in the outcome equation.[6] We extend the approach to allow for non-separability in the outcome equation of person-specific and place-specific factors, as well as for distinct place-specific channels. We also extend prior work by allowing for a specific form of selection of movers tailored to our setting.

Finally, our analysis of geographic variation in risky prescription opioid use relates to the large literature on the causes and consequences of geographic variation in health care utilization and treatment. Skinner (2011) provides a review of this literature. The important role we estimate for place-specific factors that affect addiction transitions is consistent with the existing widespread evidence of substantial geographic variation in treatment practices that involve tradeoffs across different objectives, such as whether to order a test (e.g. Moss et al. 2020) or whether to engage in aggressive treatment or "watchful waiting" in response to early detection of prostate cancer (e.g. Al Hussein Al Awamlh et al. 2021).

The rest of the paper proceeds as follows. Section 2 describes the setting and data. Section 3 introduces the model. Section 4 presents estimates from mover event studies and discusses how the resulting patterns can be interpreted in light of the model. Section 5 discusses the parameterization of the model and its estimation. Section 6 presents the model estimates and our analysis of the impacts of counterfactual place-based policies. The last section concludes.

2 Setting and Data

We study risky prescription opioid use over the ten-year period from 2006 to 2015. Our study period begins with the opioid epidemic already under way. Before and during our time period, the epidemic was primarily

[5] Finkelstein, Gentzkow, and Williams (2016) in turn draw on earlier work in the health care space that exploited patient migration (Song et al. 2010) and physician migration (Molitor 2018). 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.

[6] One exception is the study of habit formation in consumer purchases by Bronnenberg, Dubé, and Gentzkow (2012).

driven by prescription opioids (see Appendix Figure A.1).

We analyze prescription opioid use among adults enrolled in the Social Security Disability Insurance (SSDI) program, the federal public insurance program for the disabled. SSDI provides both income and medical insurance (Medicare) to workers experiencing long-term disability. Medical insurance coverage begins two years after eligibility for income payments. Over eight million disabled workers (and two million spouses and dependents) received SSDI benefits in 2010, and SSDI expenditures comprised over seven percent of federal non-defense spending. About one-third of SSDI expenditures reflect Medicare costs (Autor 2015). Once on SSDI, 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 and Medicare old-age benefits (Autor and Duggan 2006). While on SSDI, recipients' income and labor market status tend to be stable; they are not allowed to have monthly labor market earnings above a low limit, which was $1,090 per month in 2015 (Social Security Administration 2017).

Several features of this population make it particularly attractive for our analysis. First, the stringent limits on labor market activity allow us to abstract from potential changes in risky use driven by large changes in individual employment or income around the time of moves. Second, rates of prescription opioid use are high among this population (Morden et al. 2014; Meara et al. 2016). Third, we are able to observe detailed, individual-level panel data on prescription opioid use in the Medicare data.

We use data from SSDI recipients included in a 20 percent random sample of Medicare beneficiaries.[7] We focus on the approximately three-quarters of SSDI recipients who are also enrolled in Medicare Part D. This voluntary, heavily subsidized prescription drug benefit program has been available to Medicare enrollees since 2006. They can enroll in it 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. Adults with sufficiently low earnings prior to the onset of disability also quality for Medicaid - the public health insurance program for qualifying low income adults; dual eligibility for Medicaid and Medicare ensures that beneficiaries have Part D coverage, and face no out-of-pocket expenses for covered health care. The Part D enrollment rate among SSDI Medicare recipients is slightly higher than the 60 percent rate among elderly Medicare recipients (Cubanski, Neuman, and Damico 2016).

We are able to follow Part D enrollees in a panel over time, and observe basic demographic information including gender, age, race, Medicaid enrollment, and zip code of residence, which we define annually as the address on file for Social Security payments.[8] We use our detailed, claim-level data to observe the drug and

[7] Unlike Medicare for the elderly, enrollment in SSDI is not limited to those over 65 years of age.

[8] We obtain data on zip code of residence from the Medicare Denominator File for years 2006-2008 and Beneficiary Summary File (BSF) for years 2009-2015. In the Medicare Denominator File, a beneficiary's zip code of residence each year is determined

dosage of filled prescriptions; prior work has also used the Medicare Part D prescription drug claims data to measure prescription opioid use (e.g. Morden et al. 2014; Meara et al. 2016; Buchmueller and Carey 2018). For the approximately three-quarters of our enrollee-years who are not enrolled in Medicare Advantage, we also observe inpatient and outpatient claims which we use to develop various measures of adverse opioid outcomes.

Sample Construction We define all outcome variables at the yearly level and analyze data for 10 years from 2006 through 2015. From our original 20 percent sample of all Medicare enrollees (15.6 million enrollees; 103 million enrollee-years) we limit ourselves to the approximately one-fifth of enrollees whose original source of Medicare eligibility was from SSDI, and then further restrict to the approximately three-fifths of enrollee-years with 12 months of Medicare Part D coverage. We exclude from this sample person-years in which the enrollee is older than 99. This produces a sample of about 2.5 million enrollees (14.8 million enrollee-years).

Our baseline geographic unit of analysis is a state, although we show robustness of our descriptive analysis to other levels of geography such as county and commuting zone. We define individuals to be "non-movers" if their state of residence is the same throughout our sample period. We define individuals to be "movers" if their state of residence changes exactly once during this period.

Starting from a sample of 215,000 enrollees who change their state of residence at least once, we impose several additional restrictions to arrive at our final sample of movers. We exclude the approximately 25,000 movers who moved during the last year in the sample, since we cannot observe their post-move behavior. We exclude about 65,000 movers who changed their state of residence more than once between April 2007 and December 2015. We limit analysis to at most five years pre-move, the move year, and five years post-move, thus excluding about 90,000 enrollee-years outside of this window. Finally, we follow the approach developed in Finkelstein, Gentzkow, and Williams (2016) and exclude about one-quarter of the remaining movers whose share of prescription claims in their destination state, among prescription claims (for any drug) in either their origin or destination state, is not at least 0.75 higher in the post-move years relative to the pre-move years.[9]

by her address on file as of March 31st of the following year; in the BSF, a beneficiary's zip code of residence is determined by her address on file as of December 31st that year. See https://www.resdac.org/articles/medicare-eligibility-and-enrollment-files-rif-versions/ for more information.

[9]Appendix Figure A.2 shows that our approach successfully identifies the timing of moves, with about 50 percent of origin or destination claims in the move year located in either a mover's origin or her destination. Note that we do not directly observe the location of Part D claims, but we do observe the identification number of the prescriber. We define the prescriber's location in a given year as the state where they have at least 60 percent of their inpatient, outpatient, and carrier claims for that year. We do not define a location for any prescriber that does not have at least 60 percent of their annual claims within a single state. On average, for our non-mover SSDI population, we estimate that about 97 percent of opioid prescriptions filled are prescribed by a doctor who

Our final sample contains 90,890 movers (521,523 enrollee-years) and 2,325,094 non-movers (13,349,773 enrollee-years).

Measuring Risky Prescription Opioid Use 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 medically appropriate treatment plan. Even in a clinical setting, physicians may struggle to identify misuse (Parente et al. 2004).

While there is no consensus gold standard measure of misuse among clinicians or medical researchers (Turk, Swanson, and Gatchel 2008; Sullivan et al. 2010), the medical literature studying the opioid epidemic has developed several proxies for risky prescription opioid use based on prescription data (Hall et al. 2008; White et al. 2009; Sullivan et al. 2010; Cepeda et al. 2012; Rice et al. 2012; Logan et al. 2013; Jena et al. 2014; Larrick 2014; Morden et al. 2014; Meara et al. 2016). These measures identify patterns in prescriptions at the person-year level that are correlated with adverse drug outcomes such as opioid dependence, emergency room visits, and overdose deaths (Braden et al. 2010; Dunn et al. 2010; Bohnert et al. 2011; Logan et al. 2013; Jena et al. 2014; Ciesielski et al. 2017; Brat et al. 2018; Klimas et al. 2019; Rough et al. 2019; Wei et al. 2019).

Our primary measure of risky use is 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 of the year. This dosage is above the 96th percentile of enrollee-quarter observations in our data (and above the 93rd percentile among enrollees who are prescribed any opioids in the year); it is about six times higher than the average quarterly dosage in our sample, and three and a half times higher than the average quarterly dosage among those with any opioid use in the year. Using the typical Vicodin dosage, it would correspond to 24 Vicodin pills each day during a three-month period. Our use of this measure follows Sullivan et al. (2010), Larrick (2014), Meara et al. (2016), and Ciesielski et al. (2017).

We also show robustness of our descriptive results to two other measures used in the literature: an indicator for filling prescriptions with four or more unique physicians in a year, and an indicator for filling a new opioid prescription before the end of a previous prescription. Appendix A provides more detail on the construction of each measure and their correlations with each other and with subsequent adverse outcomes in our data. We selected our primary measure of risky use based on the observation that it is the most

practices within the individual's state of residence. Note that our measure of the change in the claim share in a given location 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 prescription claims and a pre-move destination prescription claim share greater than 0.05; (ii) they have no pre-move prescription claims and a post-move destination prescription claim share less than 0.95. See Finkelstein, Gentzkow, and Williams (2016) for a more detailed discussion.

predictive of subsequent adverse outcomes in our data (see Appendix Table A.1).

Finally, for some of the descriptive analyses, it will be useful to distinguish among patients based on their prior opioid history. Following the standard definitions in the literature (Sun et al. 2016; Deyo et al. 2017), we define a mover as "opioid naive" if she filled no opioid prescriptions in the year before the move, and as a "prior user" if she filled an opioid prescription in the year before the move.

Summary Statistics Table 1 reports summary statistics for our study population of movers (column 1). Slightly over one-half are female, and approximately three-quarters are white. Almost three-fifths receive Medicaid, and their average age is 55. In just under half of enrollee-years there is at least one opioid prescription, which is consistent with the previously-documented high rate of opioid use in the disabled population (Morden et al. 2014; Meara et al. 2016). About five percent of enrollee-years meet the definition for risky use. In the year prior to move, those whom we can characterize are roughly evenly divided between opioid naives and prior users; almost one-fifth of mover-years have no Part D data in the year before move and therefore cannot be characterized as an opioid naive or a prior user.[10] Relative to non-movers (column 2), movers are slightly more likely to be female, white, on Medicaid, and younger. Movers also exhibit somewhat higher rates of prescription opioid use and of risky use.

Among our movers, we estimate that the average distance between their origins and destinations (measured between the population-weighted state centroids based on the 2010 census) is 797 miles, with a median move of 638 miles and a standard deviation of 617 miles. The median state receives 1,462 movers and the mean state receives 1,779. Florida is the most common destination state (about 13% of movers) and also the most common origin state (8.7% of movers). The least common destination is the District of Columbia (0.2% of movers) and the least common origin is North Dakota (0.2% of movers).

Figure 1 shows the distribution of the rates of risky prescription opioid use across states for our full sample of non-movers. New England, Appalachia, the Southwest, and the Northwest are all particularly hard-hit. There is also considerable variation within regions. For example, within New England, the average risky use rate in New Hampshire is more than twice that of the neighboring state of Massachusetts, while in the Midwest, the risky use rate in Montana is more than two and a half times higher than that of the neighboring state of North Dakota.[11] These differences will be prove useful for generating variation in

[10] This is due to the steady expansion of Part D coverage during our sample period and largely reflects movers who enrolled in Part D during the year of their move or afterwards. As we previously noted in footnote 9, we label someone as a mover even when no prescription drug claims exist before the move if they have a post-move destination prescription share of over 95%. We show in Appendix B that our results are robust to the exclusion of these types of movers by restricting to a balanced panel of individuals with Part D coverage for 3 years pre- and post-move.

[11] We cannot compare the patterns of risky use as we measure it in our population to that of the general population. However, in Appendix B we show that national trends and state-level variation in opioid prescriptions per capita in our population are similar to

place-based factors when individuals move across states.

3 Model

We build a dynamic model of opioid addiction and risky opioid use. We consider a population of people i living in locations j in years t, with $j(i,t)$ denoting i's location in year t. Each person is either a non-mover who stays in a single location in all periods or a mover who changes location exactly once. We denote the set of non-movers in location j by $\mathscr{I}_j$. We define a *cohort* of movers by their origin o, destination d, and move year m, and index these cohorts by c. The set of movers in cohort c is $\mathscr{I}_c$, and we let $m(c)$, $o(c)$, and $d(c)$ denote cohort c's move year, origin, and destination, respectively.

In each year t, person i may be either addicted to prescription opioids or not addicted. We denote this addiction state by $a_{it} \in \{0,1\}$, with $a_{it} = 1$ indicating that i is addicted in year t. The addiction state evolves stochastically with probabilities of transitioning into or out of addiction that depend on both person- and place-specific factors. The probability of transitioning to addiction is given by $\pi_j^+ + \eta_i^+$ and the probability of transitioning out of addiction is given by $\pi_j^- + \eta_i^-$, where the π and η terms represent place- and person-specific factors respectively.[12] Thus,

$$Pr(a_{i,t} = 1 | j(i,t) = j) = \begin{cases} 1 - \pi_j^- - \eta_i^- & \text{if } a_{i,t-1} = 1 \\ \pi_j^+ + \eta_i^+ & \text{if } a_{i,t-1} = 0. \end{cases}$$

We let $\pi_j = \begin{bmatrix} \pi_j^+ & \pi_j^- \end{bmatrix}$ and $\eta_i = \begin{bmatrix} \eta_i^+ & \eta_i^- \end{bmatrix}$.

The expected share $\bar{a}_{jt}$ of non-movers in location j who are addicted at time t is defined recursively as

$$\bar{a}_{jt} = \frac{1}{\left|\mathscr{I}_j\right|} \sum_{i \in \mathscr{I}_j} \left[\left(1 - \pi_j^- - \eta_i^-\right) a_{i,t-1} + \left(\pi_j^+ + \eta_i^+\right)(1 - a_{i,t-1}) \right],$$

where the initial addiction states a_{i0} are parameters to be estimated and $\left|\mathscr{I}_j\right|$ denotes the number of non-movers in location j.

Our main observed outcome of interest is risky prescription opioid use. We treat this as binary, letting $y_{it} \in \{0,1\}$ be an indicator for whether person i engages in risky use in year t, and we let $\bar{y}_{ct}$ denote the mean of y_{it} among movers in cohort c. We assume that the probability an individual who is addicted in a given

that in the general population, although the level of prescriptions per capita is substantially higher for our disabled population.

[12] We restrict the domain of these parameters to values where addiction transitions can be interpreted as probabilities (i.e. [0,1]). The domain restrictions applied in our estimation are discussed in Appendix D.

year engages in risky use is given by γ_{jt}, a term that may depend on both place- and time-specific factors, including local supply-side conditions. As a shorthand, we refer to the parameter γ_{jt} as "opioid availability" in location j and time t. Our baseline model assumes that non-addicted individuals never exhibit risky use; in Appendix D we show that that our counterfactuals are robust to a relaxation of this restriction.

The probability of risky use is thus given by

$$Pr(y_{it} = 1 | j(i,t) = j) = \begin{cases} \gamma_{jt} & \text{if } a_{it} = 1 \\ 0 & \text{if } a_{it} = 0. \end{cases}$$

Expected risky use conditional on the addiction state is $a_{it}\gamma_{jt}$.

In the empirical analysis, we will observe risky use y_{it} and the location of each individual, but not their underlying addiction state a_{it}. We cannot directly observe the addiction state a_{it}, since not all addicted individuals engage in risky use in a given year. The key identification challenge will therefore be to separately pin down the opioid availability parameters γ_{jt}, the place-specific and person-specific addiction parameters π_j and η_i, and the initial shares of addiction $\overline{a}_{j0}$. As we will discuss, the patterns of risky use around moves to and from different locations will be crucial for identification.

A main focus of the analysis will be characterizing the component of risky use that can be attributed to the causal effect of place. For an individual who is addicted ($a_{it} = 1$), the causal effect of moving in period t from an original location $o(i)$ to a new location $d(i)$ on risky use in period t is $\left(\gamma_{d(i)t} - \gamma_{o(i)t}\right)$. The causal effect for a non-addicted individual is zero. Thus, the average causal effect of such a move in period t for a population in which share $\overline{a}$ is addicted is $\overline{a}\left(\gamma_{d(i)t} - \gamma_{o(i)t}\right)$. Over time, the share addicted $\overline{a}$ will also be affected by the move via place-specific addiction parameters.

To define the causal effect in full generality, we can define a set of potential outcomes $y_{it}(\mathbf{h})$, where $\mathbf{h} = (j_1, j_2, , , j_t)$ is a vector indicating the history of locations in which i lived in each year $1, ..., t$, and where $y_{it}(\mathbf{h})$ is the outcome y_{it} that would occur under history $\mathbf{h}$. We similarly define $\overline{a}_{ct}(\mathbf{h})$ to be the share of individuals in cohort c at time t who would be addicted were all of those individuals to have location history $\mathbf{h}$. We assume that a mover is in their origin location for $t < m$, is in their destination for $t > m$, and may be in either their origin or their destination in $t = m$. Note that our formulation implicitly rules out anticipatory effects of moving, as potential outcomes only depend on current and past locations.

We can then define the period-t average treatment effect on movers in cohort c of moving in year $m(c)$

relative to remaining in their origin as:

$$T_{ct} = E_{i \in \mathscr{I}_c}\left[y_{it}\left(\mathbf{h}_{ct}\right) - y_{it}\left(\mathbf{h}_{ct}^0\right)\right] \tag{1}$$

where $\mathbf{h}_{ct}^0$ is the t-history where all elements are equal to $o(c)$ and $\mathbf{h}_{ct}$ is the t-history in which the first $m(c)$ elements are $o(c)$ and the remaining elements are $d(c)$. It is straightforward to show that

$$T_{ct} = \begin{cases} 0 & t < m(c) \\ \overline{a}_{ct}\gamma_{d(c)t} - \overline{a}_{ct}^0\gamma_{o(c)t} & t > m(c) \end{cases} \tag{2}$$

where $\overline{a}_{ct} = \overline{a}_{ct}\left(\mathbf{h}_{ct}\right)$ and $\overline{a}_{ct}^0 = \overline{a}_{ct}\left(\mathbf{h}_{ct}^0\right)$. If we assume that movers are as-good-as-randomly selected from non-movers in their origin, we have $\overline{a}_{ct}^0 = \overline{a}_{o(c)t}$; our empirical implementation below will allow for the possibility of non-random selection.

The period-t average treatment effect on movers in equation (2) consists of two distinct channels, reflecting the impact of place on both the share of addiction $(\overline{a}_{ct})$ and on the rate of risky use conditional on addiction. To see this more clearly, we can rewrite the term for $t > m(c)$ as

$$T_{ct} = \left(\overline{a}_{ct} - \overline{a}_{ct}^0\right)\gamma_{d(c)t} + \overline{a}_{ct}^0\left(\gamma_{d(c)t} - \gamma_{o(c)t}\right),$$

where the first term can be interpreted as the treatment effect due to the addiction channel and the second term can be interpreted as the treatment effect due to the availability channel.

4 Descriptive Evidence

We present descriptive evidence of how the propensity for risky use changes with the timing and direction of moves, and how these patterns vary across individuals with different probabilities of addiction. We discuss how these patterns can be interpreted in light of our model.

4.1 Event Study Specification

We specify an event study regression that recovers an average of the cohort and period-specific average treatment effects defined in Section 3. Our approach follows recent work emphasizing the importance of allowing for heterogeneity in treatment effects across groups and periods (e.g. de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021; Sun and Abraham 2021; Wooldridge 2021). We allow

for fixed individual differences in risky use rates, arbitrary trends in those rates correlated with observables, and arbitrary trends in those rates around moves that are the same for all mover cohorts. These adjustments correct for selection of movers in a way that is flexible, although not directly tied to our model. When we parameterize our model in Section 5 below, we will correct for selection in a way that has a precise structural interpretation.

To focus on event time, we let $r(c,t) \equiv t - m(c)$ index years relative to a cohort's move year. We use the subscript cr as shorthand for $c,t(c,r)$, where $t(c,r)$ is the calendar year corresponding to relative year r for cohort c. Thus, for example, $T_{cr} = T_{c,t(c,r)}$. We similarly use the subscript ir as shorthand for $i,t(c(i),r)$ when referring to movers. Thus, for example, for any mover i we have $y_{ir} = y_{i,t(c(i),r)}$. We define X_i to be a vector of observables for mover i consisting of the interaction between five-year age bin (as measured by birth year), race (White, Black, or other), and gender. Finally, for each mover cohort c, we define a sample of matched non-movers consisting of all non-movers who are observed in cohort c's pre-move year $r = -1$ in the cohort's origin $o(c)$.

We let $\hat{m}^X_{cr}$ denote the average of y_{ir} among the subset of c's matched non-movers with characteristics X. We then define $\hat{T}^X_{cr}$ to be the average of the difference in differences $(y_{ir} - y_{i,-1}) - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)$ among i belonging to the set $\mathscr{J}^X_c$ of movers in cohort c with characteristics X. We let $\hat{T}_{cr}$ denote the average of $\hat{T}^X_{cr}$ across X, weighting by the number of movers in c with each set of characteristics X. Defining $\hat{T}^X_{cr}$ as a difference in difference adjusts for any fixed individual differences in propensity for risky use, and defining it conditional on X before averaging across X to obtain $\hat{T}_{cr}$ allows for arbitrary calendar year trends in y that are specific to each value of X. We show below that under an appropriate parallel trends assumption, $\hat{T}_{cr}$ is a consistent estimator for the treatment effect T_{cr}, possibly shifted by a relative year fixed effect.

Finally, in the spirit of Finkelstein, Gentzkow, and Williams (2016), we scale these estimated treatment effects by a measure of the gap in risky use between a mover's origin and destination, allowing us to interpret the event study coefficients as the share of geographic differences closed by moving. Specially, we define δ_{cr} to be the difference $\bar{y}_{d(c)r} - \bar{y}_{o(c)r}$ in the average rates of risky use among matched non-movers in period $t(c,r)$ in the cohort's destination $d(c)$ and origin $o(c)$. To simplify the interpretation of our estimates in Section 4.2, we normalize δ_{cr} by subtracting the mean of this difference across all movers.[13] We let $\hat{\delta}_{cr}$ denote the sample analogue of δ_{cr}. We then estimate:

[13]This term is formally defined by $\bar{y}_{d(c)r} - \bar{y}_{o(c)r} - \mathbb{E}_{i \in \cup_c \mathscr{J}^c}\left[\bar{y}_{d(i)r} - \bar{y}_{o(i)r}\right]$. This demeaning eases the formal interpretation of the event study coefficients. It makes little difference in practice since, as we will see in Figure 3 below, the mean of $\hat{\delta}_{cr}$ in our sample is very close to zero.

$$\hat{T}_{cr} = \rho_r + \mu_r \hat{\delta}_{cr} + \varepsilon_{cr}, \tag{3}$$

where ρ_r is a set of relative year fixed effects that control for any changes associated with moves that are common across origin-destination pairs. We weight each observation (c,r) by the total number of movers $|\mathscr{I}_c|$ in cohort c. We compute confidence intervals via 50 bootstrap iterations of the Bayesian bootstrap procedure.

4.2 Interpretation

We show that under the following assumptions, the event study coefficients $\hat{\mu}_r$ in equation (3) have a precise interpretation as the weighted average of the scaled treatment effects T_{cr}/δ_{cr}.

Assumption 1. (Conditional Parallel Trends with a Constant Relative Year Effect)

$$\mathbb{E}_{i \in \mathscr{I}_c^X}\left[y_{ir}\left(\mathbf{h}_{cr}^0\right) - y_{i,-1}\left(\mathbf{h}_{cr}^0\right)\right] = \mathbb{E}\left[\hat{m}_{cr}^X - \hat{m}_{c,-1}^X\right] + \rho_r$$

for all c, X, and $r > -1$.

Assumption 1 is a standard (implicit) assumption in much of the prior work conducting panel mover analyses (e.g., Finkelstein, Gentzkow, and Williams 2016). It adapts the conditional parallel trends assumption often found in differences-in-differences settings (e.g., Heckman, Ichimura, and Todd 1997; Abadie 2005) to an event study setting, weakening that assumption to allow for an additional level difference between mover and non-mover outcomes in each relative year. Assumption 1 will hold under our model if the distribution of both η_i and a_{i0} is as good as random conditional on our covariates X. It is weaker than that, however, as it also allows for relative year fixed effects and arbitrary individual fixed effects in risky use. In our structural analysis, we will allow explicitly for selection in the distributions of η_i and a_{i0}.

Two other assumptions are implicit in our formulation. First, we rule out anticipatory effects by assuming that potential outcomes $y_{ir}(\cdot)$ only depend on current and past locations. Second, we also make a stable unit treatment value assumption by allowing $y_{ir}(\cdot)$ to depend only on the location history of individual i.

Proposition 1. *Under Assumption 1,* $\hat{T}_{cr}$ *is an unbiased and consistent estimator of* $T_{cr} + \rho_r$. *Furthermore, the event study coefficients* $\hat{\mu}_r$ *from equation* (3) *are consistent estimators of*

$$\mu_r = \sum_c w_c \frac{T_{cr}}{\delta_{cr}}, \tag{4}$$

where $w_c \equiv \frac{|\mathscr{I}_c| \cdot \delta_{cr}^2}{\sum_{c \in \mathscr{C}_m} |\mathscr{I}_c| \cdot \delta_{cr}^2}$ *is the weight given to cohort c and* $\sum_c w_c = 1$.

Proof. See Appendix C.

The event study coefficients μ_r thus capture a weighted average of cohort treatment effects T_{cr} relative to the average difference in outcomes between their origin and destination. Scaling by the denominator δ_{cr} gives the event study coefficients an interpretation as the average share of the gap in risky use between origin and destination that is closed as a result of the treatment effect. The weights w_c are all between 0 and 1; they are increasing in the number of individuals in a cohort and the difference in rates of risky use between their origin and destination. Weighting by the difference in risky use rates addresses extreme heteroskedasticity that would otherwise arise due to some cohorts having small values of the denominator δ_{cr}. If relative treatment effects were homogeneous across all individuals (i.e., $\frac{T_{cr}}{\delta_{cr}}$ was equal to a constant R_r) and errors in risky use were *i.i.d.*, $\hat{\mu}_r$ would be the efficient estimator of R_r.[14] □

To build intuition for what we can learn from the pattern of event study coefficients, we consider a population consisting of a single cohort c with origin location $o(c)$ and destination $d(c)$, and we assume that movers are as good as randomly selected from among individuals in their origin, conditional on X. Recall from equation (2) that T_{cr} must be zero in years prior to the move ($r < 0$), and thus μ_r must be equal to zero as well in those years. In years following the move we have $T_{cr} = \overline{a}_{cr}\gamma_{d(c)r} - \overline{a}_{o(c)r}\gamma_{o(c)r}$ and $\delta_{cr} = \overline{a}_{d(c)r}\gamma_{d(c)r} - \overline{a}_{o(c)r}\gamma_{o(c)r}$. Thus, we can rewrite μ_r for $r > 0$ as

$$\mu_r = \frac{\overline{a}_{cr}\gamma_{d(c)r} - \overline{a}_{o(c)r}\gamma_{o(c)r}}{\overline{a}_{d(c)r}\gamma_{d(c)r} - \overline{a}_{o(c)r}\gamma_{o(c)r}}. \tag{5}$$

We consider four special cases.[15]

Case 1: Only Person Effects Suppose, first, that neither availability effects γ_{jr} nor transition probabilities π_j vary between the origin and destination. Then all geographic variation is due to person effects. Rates of risky use differ across places only because of differences in the distribution of initial addiction states a_{i0} and

[14] To see this, suppose $T_{cr}/\delta_{cr} = R_r$ for all c. We can then write

$$\hat{T}_{cr} = \rho_r + R_r\delta_{cr} + \varepsilon_{cr},$$

where the errors $\varepsilon_{cr} = \hat{T}_{cr} - \mathbb{E}_{i \in \mathscr{I}_c}\left[\hat{T}_{cr}\right]$ are conditionally mean zero by Proposition 1. If the underlying individual-level errors in risky use are *i.i.d.*, the covariance matrix of ε_{cr} will have zero off-diagonal elements and diagonal elements equal to $1/|\mathscr{I}_c|$. Thus, the weighted OLS regression in equation 3 is the efficient estimator of the coefficient R_r.

[15] As discussed in Section 2, a mover is in their origin in relative year -1 and in their destination in relative year 1. In relative year 0, however, the individual may be either in their origin or destination. In discussing how the model can be used to interpret these event study coefficients, we abstract away from this empirical reality and instead assume that movers are in their origin during relative year 0 and instantaneously move before relative year 1. In the visualization of our various examples in Figure 2, we replicate uncertainty around the mover's location in relative year 0.

addiction propensities η_i. In this case, we have $\gamma_{d(c)r} = \gamma_{o(c)r}$ and also $\overline{a}_{cr} = \overline{a}_{o(c)r}$ (since addiction rates do not depend on location). Thus, $\mu_r = 0$ for all $r > 0$, and the event study plot will be flat at zero with no jump on move. Panel A of Figure 2 illustrates this case.

Case 2: Only Availability Place Effects Next, consider the case where availability effects γ_{jr} differ between the origin and destination but the transition probabilities π_j and the distributions of a_{i0} and η_i do not. Now rates of risky use differ across places only because of differences in the ease with which individuals who are addicted can engage in risky use, and all geographic variation is due to variation in place effects on availability. In this case, we have $\overline{a}_{cr} = \overline{a}_{o(c)r} = \overline{a}_{d(c)r}$, since the evolution of addiction rates does not depend on location, and thus, $\mu_r = 1$ for all $r > 0$. The event study plot will jump from zero to one on move and remain flat at one thereafter. Panel B of Figure 2 illustrates this case.

Case 3: Person and Availability Effects Next, we combine the above two cases and allow both γ_{jr} and the distributions of person-specific factors a_{i0} and η_i vary between the origin and destination, but continue to assume the transition probabilities π_j do not vary across locations. Then we have $\overline{a}_{cr} = \overline{a}_{o(c)r}$ but $\overline{a}_{o(c)r} \neq \overline{a}_{d(c)r}$. In this case, we can write the event-study coefficient for $r > 0$ as

$$\mu_r = \frac{\overline{a}_{o(c)r}\left(\gamma_{d(c)r} - \gamma_{o(c)r}\right)}{\left(\overline{a}_{d(c)r} - \overline{a}_{o(c)r}\right)\gamma_{d(c)r} + \overline{a}_{o(c)r}\left(\gamma_{d(c)r} - \gamma_{o(c)r}\right)}.$$

Here, the first term in the denominator is due only to differences in person-specific (addiction) factors between origin and destination, and the second term is due only to differences in place-specific availability. The event-study coefficient gives the share of the overall difference in outcomes due to the latter. Provided that the sign of the availability effect $\left(\gamma_{d(c)r} - \gamma_{o(c)r}\right)$ is the same as the sign of the difference in addiction rates $\left(\overline{a}_{d(c)r} - \overline{a}_{o(c)r}\right)$, this share will lie between zero and one. The event study plot will show an on-impact jump equal to this share and, provided that the ratio of addiction shares $\frac{\overline{a}_{d(c)r}}{\overline{a}_{o(c)r}}$ and availability effects $\frac{\gamma_{d(c)r}}{\gamma_{o(c)r}}$ are approximately constant, have little or no trend following the move. Panel C of Figure 2 illustrates this case.

Case 4: Adding Place-Specific Transitions to Addiction Finally, we generalize the previous case to now also allow transition probabilities π_j to also vary between the origin and destination. This means that $\overline{a}_{cr}$ will differ from both $\overline{a}_{o(c)r}$ and $\overline{a}_{d(c)r}$, and we cannot simplify the expression for μ_r beyond equation (5).

However, we can build some intuition by thinking about how $\overline{a}_{cr}$ will evolve. Consider, for simplicity, the case where the destination has higher risky use rates due to both availability and addiction: $\gamma_{d(c)r} > \gamma_{o(c)r}$, $\pi^+_{d(c)} > \pi^+_{o(c)}, \pi^-_{d(c)} < \pi^-_{o(c)}$, and $\overline{a}_{d(c)r} > \overline{a}_{o(c)r}$.

Note, first, that immediately after the move $\overline{a}_{cr}$ will be close to $\overline{a}_{o(c)r}$. This means that the on-impact jump in the event-study plot will have a similar interpretation to Case 3, with the jump roughly equal to the share of differences in outcomes that are due to place-specific availability factors. Next, consider the changes in $\overline{a}_{cr}$ post-move. If most variation in addiction rates is due to person-specific factors a_{i0} and η_i, $\overline{a}_{cr}$ will remain close to $\overline{a}_{o(c)r}$ and the event study plot will remain flat post-move as in Case 3. If a large share of variation in addiction rates is due to the place-specific terms π_j, $\overline{a}_{cr}$ will gradually increase following the move from a level close to $\overline{a}_{o(c)r}$ in the direction of $\overline{a}_{d(c)r}$. This positive post-move trend in the event study plot is a signature of variation in π_j playing an important role. If that variation is sufficiently important that the addiction rates among movers eventually converge to the destination level $\overline{a}_{d(c)r}$, the event study coefficients μ_r will eventually converge toward 1. Panel D of Figure 2 illustrates this case.

4.3 Results

Main Event Study

We begin by examining the distribution of the origin-destination gaps in risky use rates across individual movers and years. This is shown in Figure 3. The mean value is close to zero and the distribution is roughly symmetric, implying that moves from states with low-rates of risky use to states with high rates of risky use are as common as moves from states with high rates of risky use to states with low rates. The standard deviation is roughly two percentage points, which is large relative to the overall mean rate of risky use among movers of approximately five percentage points shown in Table 1.

Figure 4 shows our aggregate event-study results. The figure plots the coefficients $\hat{\mu}_r$ from equation (3). The plot shows little systematic trend pre-move, which is supportive of our identifying assumption that matched non-movers serve as valid controls. It also shows two distinct features that we might expect based on Case 4 in Section 4.2 above: an immediate jump in risky use upon move and gradual post-move convergence. As discussed, the magnitude of the jump—approximately 0.30—provides a rough measure of the share of the difference between a typical origin and destination attributable to differences in opioid availability to addicted individuals. This might reflect place-specific factors such as the availability of "pill mill" pain clinics or prescription monitoring programs intended to limit risky use. In addition, the significant post-trend that we see may suggest a significant role for place-specific factors that affect transitions in and out of addiction. This might reflect place-specific factors such as the propensity of local physicians to initiate opioid prescriptions. Together, these features suggest that both opioid availability and addiction transitions play a significant role in driving cross-sectional variation in risky use.

Column 1 of Table 2 provides a quantification of these estimates, summarizing the average values of $\hat{\mu}_r$ at various time horizons r relative to the move. One year after moving, we estimate that this coefficient is 0.288 (standard error = 0.041). Five years after moving, this estimate grows to 0.621 (standard error = 0.080). To illustrate these dynamics, consider locations with a stable 3.5 percentage point difference between non-movers in the rate of risky use—approximately equivalent to the difference between the 20th percentile state and the 80th percentile state in average rates of risky use over our full sample period. A move between these two locations would be associated with an immediate increase in the likelihood of risky use of approximately 1 percentage point in the year after moving, or about 30 percent of the cross-sectional gap between these areas. On average, the likelihood of risky use would continue increasing by about 0.30 percentage points each subsequent year after the move, with approximately 60 percent of the cross-sectional gap closed by five years after the move.

Figure 5 provides another way of visualizing how the propensity for risky use changes around moves. It shows a binscatter of the average change in risky use rates over the one to five years post-move compared to the one to five years prior to move against the origin-destination difference $\hat{\delta}_{ct}$ in risky use rates. The results show a clear relationship between the size of the move and changes in the prevalence of risky use, with a significant and positive slope (0.24). This plot also suggests that the relationship between the size of the move and the change is roughly linear and symmetric for moves between places with higher and lower risky use rates, consistent with an assumption of roughly constant scaled treatment effects $\frac{T_{cr}}{\delta_{cr}}$. We also find roughly symmetric effects when we estimate equation (3) for moves to areas with higher and lower risky use rates separately (Appendix Figure A.3).

Finally, we examine the event study estimates when we no longer adjust for any fixed individual differences in propensity for risky use. Specifically, instead of defining $\hat{T}^X_{cr}$ to be the average of the difference in differences $(y_{ir} - y_{i,-1}) - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)$ among the set $\mathscr{I}^X_c$ of movers in cohort c with characteristics X, we define the dependent variable as $\left(y_{ir} - \hat{m}^X_{cr}\right)$. Appendix Figure A.4 shows the results of this alternative "levels" specification. We continue to find little systematic pre-move trend, and a similar post-move pattern of coefficients. However, the level of the pre-move coefficients is now about 0.15, indicating that there is potential selection in where movers move. In particular, the results indicate that movers to destinations with a higher propensity for risky use than in their origin tend, prior to their move, to have a higher propensity for risky use than matched non-movers in their origin. We will parameterize our model below to to explicitly allow for such selection within the context of the model.

Prior Users and Opioid Naives

The model described in Section 3 predicts differential treatment effects by the addiction status of movers. Changes in availability, for example, would directly affect addicted individuals immediately upon move, but would have no immediate effects on those who are not addicted. Differences in place-based transitions to and from addiction would affect all individuals, and so lead to post-trends in the event study for both those who are addicted and those who are not. The aggregate event study in Figure 4 captures an average of these effects.

While we cannot directly observe addiction states in a given year, we explore these predictions empirically by proxying for addiction status with an individual's prior history of opioid use. This dovetails with medical literature studying how addiction varies with the opioid histories of patients (Paulozzi et al. 2012; Edlund et al. 2014), and exploring the impacts of drug supply among patients without any previous opioid use (Shah, Hayes, and Martin 2017; Brat et al. 2018; Jeffery et al. 2018). Figure 6 shows the results from estimating equation (3) separately for movers with at least some opioid utilization in the year prior to move ("prior users") and movers without opioid use in the year prior to move ("opioid naives"). These results are summarized in Table 2 (columns 2 and 3) and are consistent with the intuitions from the model. Prior users show a larger and immediate jump upon move while there is little or no discrete change upon move for opioid naives. In the subsequent post-move years, risky use rates for both prior users and opioid naives both increase gradually, consistent with high-risky-use areas causing more transitions to addiction and fewer transitions from addiction.

Robustness

In Appendix B, we show that these descriptive results on the changes in risky prescription opioid use around moves—both in aggregate and separately based on prior opioid use—are robust to several alternative specifications. These include using using county or commuting zone as the unit of analysis instead of state, using alternative measures of risky prescription opioid use, removing the relative year fixed effects (ρ_r), and removing our conditioning on covariates (X). They also continue to hold when we estimate the event study on a balanced panel of individuals whom we see with Part D coverage for three years before and after move, if we restrict to individuals who who had Part D coverage for the full year for all years they appear in the sample, or if we restrict to individuals who are alive throughout our sample period. They are also robust to removing the three most common destinations in our sample (i.e. Florida, Texas, and California) as well as excluding patients in hospice or being treated for cancer.

5 Model Parameterization and Estimation

The descriptive evidence in Section 4 suggests an important role for place-specific factors affecting both opioid availability and addiction transitions. To make quantitative statements about the relative role of different model parameters in driving risky opioid use, as well as counterfactual inferences about what rates of risky use would look like under alternative policies, we further specify and parameterize the dynamic model of opioid addiction and risky opioid use developed in Section 3.

5.1 Allowing for selection

Our event-study specification in the previous section allowed for selection of movers to different destinations based on their propensity for risky use relative to other people in their origin by allowing a constant level difference in risky use rates across individuals. However, the model in Section 3 suggests that selection on initial addiction state or on person-specific addiction parameters can generate differences in risky use rates that evolve dynamically, rather that ones that are constant over time.

We therefore model potential selection explicitly by allowing for the distribution of initial addiction status and person-specific addiction transitions to vary systematically with both the mover's origin and their destination. Although this type of selection does not nest constant level differences among individuals, we show below that it is able to reproduce the key patterns from our event study analysis.

Specifically, we assume that the distribution of person-specific parameters (a_{i0}, η_i) for each move cohort c is drawn from some convex combination of the distributions of person-specific factors in their origin state $o(c)$ and destination state $d(c)$. We denote the distribution of initial addiction statuses a_{i0} and the person-specific addiction parameters η_i among non-movers in each state by F_j^a and F_j^η respectively, and our assumption specifies that the distribution of these parameters in each move cohort is given by

$$F_c^a \sim (1-s) \cdot F_{o(c)}^a + s \cdot F_{d(c)}^a$$
$$F_c^\eta \sim (1-s) \cdot F_{o(c)}^\eta + s \cdot F_{d(c)}^\eta.$$

The selection parameter s governs the extent to which movers are selected to resemble individuals in their destinations.

5.2 Parameterization

Our sample includes 12,528 cohorts of movers defined by unique combinations of origin state, destination state, and move year. The descriptive evidence in the previous section suggested that the inclusion of demographic covariates did not affect the basic event study patterns; for computational ease, we therefore do not further separate cohorts of movers by demographics.

We omit observations from the move year ($r = 0$), since the enrollee is neither fully in her origin or destination in that year (see Appendix Figure A.2). We observe each cohort for an average of 6.5 years to generate 81,892 moments $\hat{y}_{cr}$, which are the sample analogues of the cohort risky use rates $\bar{y}_{cr}$.

We make two main simplifying assumptions in the estimation of our model. First, we assume that the temporal and geographic components of availability can be expressed as additively separable parameters γ_j and $\tau_{t(c,r)}$ along with relative year effects ρ_r. This ensures that geographic differences in opioid availability remain constant (in proportions) throughout our sample while allowing for shifting national attitudes and guidelines to affect availability across all states. Risky use among movers who are addicted during a given year is therefore given by

$$\gamma_{cr} = \begin{cases} \gamma_{o(c)} + \tau_{t(c,r)} + \rho_r & \text{if } r < 0 \\ \gamma_{d(c)} + \tau_{t(c,r)} + \rho_r & \text{if } r > 0. \end{cases}$$

The ρ_r extends the model in section 3 to allow for addicted movers to differ from non-movers through proportional shifts in risky-use rates that evolve arbitrarily in years around the move. These will capture factors such as temporary disruption in individuals' access to opioid supply following moves as well as correlation between the timing of moves and positive or negative shocks to opioid demand (provided these do not vary systematically depending on the origin or destination).

Second, we set aside within-location heterogeneity in the addiction transition parameter η_i among non-movers, so that $\eta_i = \eta_j$ for all i such that $i \in \mathscr{J}_j$. As we discussed in the previous section, the person-specific addiction transition factors for a cohort will be a convex combination of the distributions of person-specific factors in their origin state $o(c)$ and destination state $d(c)$. Thus, the predicted shares of addiction in our model for each cohort c evolve according to

$$\bar{a}_{cr} = \begin{cases} (1 - \pi^-_{o(c)} - \eta^-_c)\bar{a}_{c,r-1} + (\pi^+_{o(c)} + \eta^+_c)\bar{a}_{c,r-1} & \text{if } r < 0 \\ (1 - \pi^-_{d(c)} - \eta^-_c)\bar{a}_{c,r-1} + (\pi^+_{d(c)} + \eta^+_c)\bar{a}_{c,r-1} & \text{if } r > 0 \end{cases}$$

where $\eta_c = (1-s)\cdot\eta_{o(c)} + s\cdot\eta_{d(c)}$ and $\bar{a}_{cr} = \bar{a}_{c,r-1}$ if $r = 0$.[16] Because underlying drivers of addiction and risky use have changed dramatically over time, we do not focus on the steady state of this model but rather study the evolution of addiction patterns beginning from an initial state that we estimate. Finally, we define the initial probabilities of addiction in the first period and person-specific addiction transition factors for a cohort as

$$\bar{a}_{c,r(c,0)} = (1-s)\cdot a_{o(c),r(c,0)} + s\cdot a_{d(c),r(c,0)}$$

Average risky use for cohort c in time r is, as before, given by $\bar{y}_{cr} = \bar{a}_{cr}\cdot\gamma_{cr}$.

5.3 Estimation

We estimate the parameters of this model by Generalized Method of Moments (GMM), with moment conditions defined at the level of the cohort by relative year. We weight each cohort-relative-year moment by the number of mover-years in the sample used to construct the moment. We also restrict our parameter space so that all transition probabilities are bounded between zero and one and so that our combination of parameters produces a weighted global mean share of addiction of 10 percent.[17] Although the parameters ρ_r and s allow movers to differ from non-movers and to resemble non-movers in their destination locations, we estimate the parameters of the model using moments only for our sample of movers. We will therefore be able to examine the out-of-sample performance of our model using non-movers. Appendix D presents more details on our estimation and presents Monte Carlo results suggesting that the finite-sample properties of our estimator and key counterfactuals are reasonable.

5.4 Identification

We offer intuition for the key features of the data driving our estimates by highlighting the role of various model parameters in risky use of movers. This discussion builds on the intuition developed in Section 4.2

[16] This assumption—combined with omitting observations from the year of the move—allows us to essentially remove the year of the move from the estimation. We show in Appendix D that allowing for addiction transitions to occur during the year of the move does not substantially affect our results.

[17] Naturally, the share of the population who is addicted to opioids is not directly observed in the data. We chose 10 percent as a reasonable approximation; it is between the share of the population in the 2011 National Survey on Drug Use and Health that reports non-medical prescription pain reliever use over a 12 month period (4 percent) and the share that reports such as use 'ever' (13 percent) per authors' calculations. We show in Appendix D that other values do not substantially affect our results; indeed, the global mean share of addiction serves merely as a normalization because the relative overall magnitudes of addiction and availability parameters are not meaningful or well-identified in our model. For example, doubling the shares of individuals who are addicted and halving all availability parameters would leave predicted risky use at a point in time unchanged.

above.

Person-specific factors are constant throughout our model and do not change upon move. Therefore, the distribution of person-specific parameters across locations—a_{i0} and η_i— will be informed largely by patterns in risky use rates that are common to movers from a given origin regardless of their destination. Our estimate of the selection parameter s is driven by the way pre-move patterns of risky use differ across movers with different destinations.

In contrast, place-specific factors do change discretely upon move, and generate sharp changes in predicted risky use directly after moving. The estimation of differences in place-specific availability effects γ_{jt} will therefore largely be driven by the charges in risky use that occur immediately upon move. Similarly, place-specific addiction parameters π_j will be related to changes in the evolution of risky use rates in the years post-move.

A few other parameters are worth commenting on. To see how we identify relative year effects (ρ_r), consider two cohorts with the same origin and destination but different move years. During the same pre-move calendar year, the two cohorts will share the same origin, destination, and calendar year. Thus, they will differ only in their year relative to move, and the ratio of their risky use will drive our estimates of ρ_r.

Finally, to see how we separately identify the addiction transition parameters into (π^+ and η^+) and out of addiction (π^- and η^-) , it is useful to consider two hypothetical cohorts of movers: one cohort where the mover population is fully addicted and another cohort with no addiction. The post-move trends in the risky use rates of each cohort would initially each be driven by a separate set of addiction transitions. The fully addicted cohort would only initially be affected by transitions out of addiction, while the fully non-addicted cohort would only initially be affected by transitions into addiction. In the more general case, transitions into and out of addiction are related to local patterns of rising and declining risky use rates that cannot be explained by national trends in availability (τ_t).

6 Model Estimates

6.1 Fit

We simulate individual-level risky opioid use for movers and non-movers according to our estimated parameters to generate a corresponding simulated risky use panel to our Medicare data. We use this simulated data to evaluate model fit from several perspectives. Figure 7 compares the event study regression estimated on data simulated from our estimated model with the one estimated from the data. The model successfully matches the qualitative features of the data, and our model parameters closely replicate the magnitude of

the post-trends, although the magnitude of the jump is somewhat larger in the observed data than in the simulated data.

We also examine additional model predictions for non-movers, who were not used in our estimation. These predictions offer, in some sense, an out-of-sample examination of fit. We find that our estimated model successfully replicates the patterns of cross-sectional variation we observe for non-movers in the Medicare data (recall Figure 1). Specifically, Figure 8 shows that, across states, the predicted average risky use rates for non-movers from our model are highly positively correlated with the observed average risky use rates for non-movers. In addition, Appendix Figure A.5, Panel B suggests that simulated non-mover moments at the state-year level also closely reflect moments in the Medicare data.[18] The time series patterns of simulated average risky opioid use rates for non-movers in the 20th, 50th, and 80th percentile states also align reasonably well with the predictions from the model (Appendix Figure A.6).

6.2 Parameter estimates

To get a sense of our parameter estimates, the panels of Figure 9 show the distribution across states of factors affecting availability, place-based addiction transitions, and person-based addiction transitions, as well as the correlation of these factors with predicted risky use rates. These graphs are produced for non-movers.

The panels suggest that the three sets of parameters differ in their geographic distributions. Broadly speaking, they suggest that place-based factors (γ_j and π_j) play a relatively larger role in the West and Northeast, whereas person-based factors (η_i) play a larger role in Appalachia and the deep South. This could be consistent with individual factors like childhood adversity, mental health status, or broader health behaviors playing an especially important role in the latter areas. Within the place-based factors, the relative importance of availability (γ_j) is especially high in states like Michigan, Arizona, and California.

Figure 9 also shows that our estimated availability and place-based addiction parameters are highly correlated with overall predicted risky use rates. Panel A plots availability against average predicted risky use rates, a relationship with a correlation of 0.44. The correlation is even higher between place-specific addiction transitions and predicted risky use (0.51), shown in Panel B. In contrast, Panel C suggests a near-zero correlation between predicted risky use rates and our person-specific addiction parameters.

Finally, we note that our estimates imply a substantial amount of selection, with the person-based addiction parameters of movers resembling non-movers in their destination locations to a significant degree ($\hat{s} = 0.45$). This does not mean that the pre-move risky use rates of movers resemble non-movers in their

[18] Appendix Figure A.5, Panel A shows that likewise, for movers, our simulated moments at the state-year level also align closely with the corresponding empirical moments.

destination to the same degree, however, as availability parameters and place-based addiction parameters play significant roles in determining risky use rates.

6.3 Counterfactual rate of risky opioid use

To provide a quantitative estimate of the importance of various model parameters, Table 3 shows the impact of a one standard deviation reduction in different parameters (holding all others constant) on the average rate of risky use over our 10 year sample period. We show results for all states (column 1) and for the ten states which exhibited the highest rates of risky use at the beginning of our sample period (column 2). Results are quite similar for these two samples.

We start by considering the relative importance of place-based parameters and person-based parameters. We find (row 1) that about two-thirds of risky use over our ten-year period would have been eliminated by a one standard-deviation reduction in the place-specific effects of addiction (π_j) and availability (γ_j). Similarly, we find (row 2) that two-thirds of risky use would have been eliminated by a one standard deviation reduction in person-based parameters (factors affecting addiction transitions η_i and initial addiction states a_{i0}). Differences across areas in initial addiction states (a_{i0}) may in turn reflect both differences in population characteristics and also prior influences of place. These results are consistent with our event-study analysis, which suggested a sizable role for both person-specific and place-specific factors.

In practice, there may be more scope for policies to affect place-based channels than person-based channels. We therefore provide additional evidence on the effectiveness of reductions targeting different place-based channels. We compare the effectiveness of reducing place-specific availability (row 3) and place-specific addiction channels (row 4). These two targets mirror much of recent and historical policy aimed at curbing risky opioid use. Many initial policy efforts focused on policies such as prescription monitoring programs (Kilby 2015; Meara et al. 2016; Buchmueller and Carey 2018; Kaestner and Ziedan 2019) and pill mill laws (Rutkow et al. 2015; Lyapustina et al. 2016) which aimed to limit the availability of risky prescriptions for potential misuse. More recently, states have enacted opioid prescribing limits that specifically target opioid naive patients and are explicitly intended to limit transitions to addiction (Sacks et al. 2021). We find that policies targeting place-based addiction transitions would have resulting reductions more than twice as large compared to policies targeting availability.

The cumulative effects over our ten-year study period shown in Table 3 mask substantial variation in the time path of these impacts. Figure 10 illustrates this by showing how the effects of place-based policies targeting addiction or availability evolve over time. We find that in the first year of implementation, average risky use rates would have been 35 percent lower if availability were targeted compared to addiction. By the

final year of our study period, however, this relationship is reversed; average risky use rates are 55 percent lower when addiction is targeted. While the cumulative impact of policies targeting place-based addiction transitions is larger over the ten-year period, policies that affect place-based availability have a much larger initial effect, suggesting important policy tradeoffs in terms of near-term compared to longer-term impacts. These findings also suggest that, as emphasized by Cutler and Glaeser (2021), the persistence of addiction creates challenges to ending the opioid epidemic.

7 Conclusion

We explored the role of different factors in driving the prescription opioid epidemic in the decade between 2006 and 2015 for individuals enrolled in the Social Security Disability Insurance (SSDI) program, a population hit particularly hard by the opioid epidemic. Both the descriptive findings and the estimates from a dynamic model of risky opioid use highlight important roles for both person-specific factors and place-based factors. They also illustrate the distinct role played by two separate place-based channels: an addiction channel which affects transitions in and out of addiction, and an availability channel which affects the ease with which addicted individuals can obtain risky prescriptions.

We find that policies targeting place-based factors could have led to a substantial reduction in the rates of risky prescriptions, and that policies targeting different place-based channels may have both different overall impacts and different time paths for those impacts. Much of the policy attention has focused on restricting the availability of risky prescriptions, and our results suggest that these types of supply-side policies that restrict opioid availability among existing users—such as the 2018 Medicare policy to no longer reimburse for high-dose long-term opioid prescriptions (Centers for Medicare & Medicaid Services 2018; Hoffman 2018)—could have an immediate impact on rates of risky prescription opioid use. However, our findings also suggest that decreasing transitions into addiction and increasing transitions out of addiction may be quantitatively more important in reducing risky opioid use over a longer horizon. These include policies such as opioid prescribing limits specifically targeted to the opioid naive (Sacks et al. 2021) as well as policies designed to increase access to treatment facilities (Corredor-Waldron and Currie 2022).

The feasibility and desirability of policies targeting these place-based channels remains an important open question. In terms of the feasibility of targeting the addiction channel, recent work has documented substantial variation in physician propensity to prescribe opioids for what they believe to be legitimate reasons (e.g., Barnett, Olenski, and Jena 2017; Schnell and Currie 2018; Eichmeyer and Zhang, forthcoming), which is consistent with the broader evidence of substantial variation across individual physicians in other

aspects of practice style (e.g. Epstein and Nicholson 2009; Van Parys 2016; Currie and MacLeod 2020; Fadlon and Van Parys 2020; Chan, Gentzkow, and Yu 2022). A greater understanding of the roles that physician training (Schnell and Currie 2018), physician beliefs (Doctor et al. 2018), and organizational factors play in contributing to differences in physician propensity to prescribe opioids remains an important area for further work, as is further exploration of the tradeoffs created by these alternative practice styles. In terms of the desirability of targeting the availability channel, the potential for this to cause substitution to illegal forms of opioids (Alpert, Powell, and Pacula 2018; Evans, Lieber, and Power 2019; Schnell 2022) is an important area for further study.

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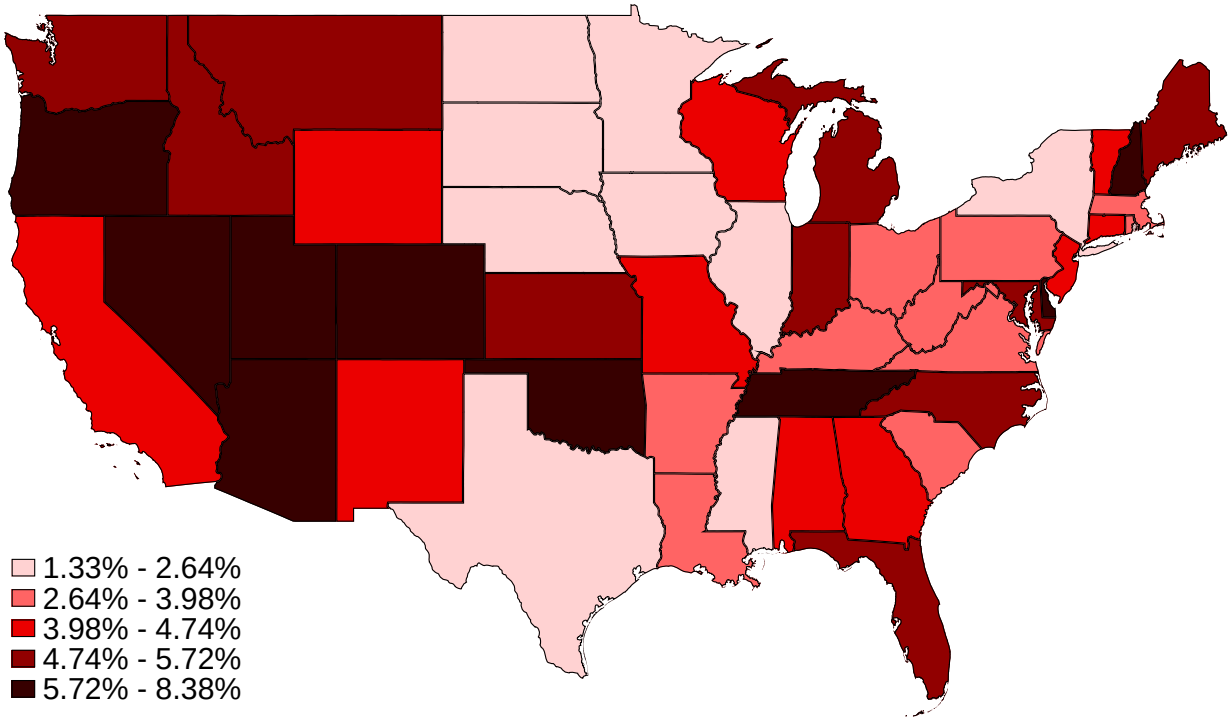
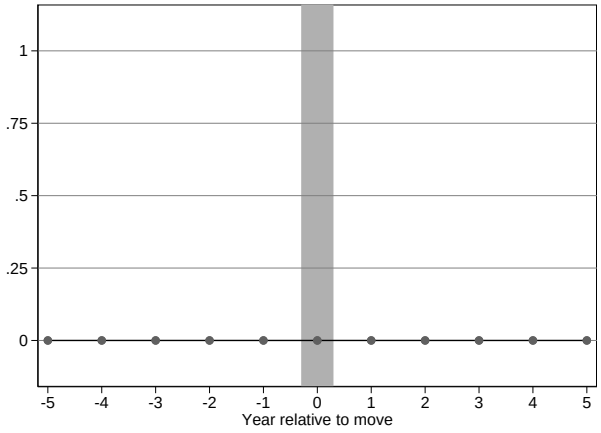
Figure 1: Geographic Variation in Rates of Risky Use



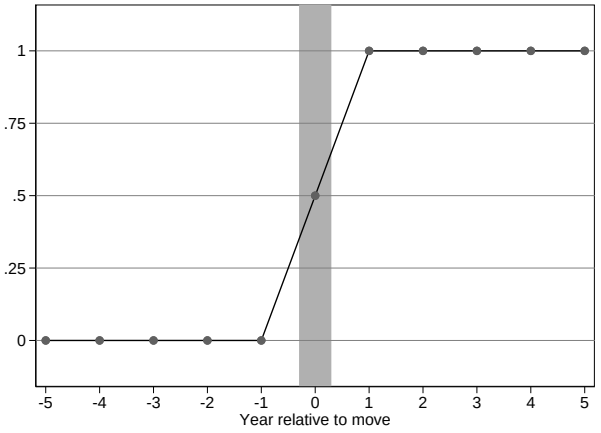
Notes: Figure reports state-level averages for the rate of risky opioid use among all non-mover years (N = 2,325,094 enrollee-years).

Figure 2: Interpreting Treatment Effects in Event Study Specification

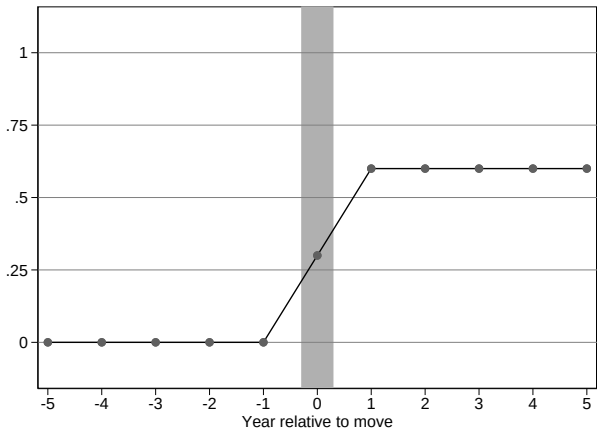
(a) Case 1: Only Person Effects Vary



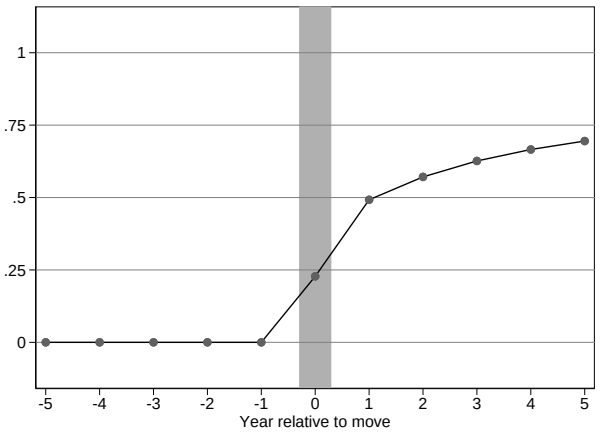
(b) Case 2: Only Availability Effects Vary



(c) Case 3: Both Person and Availability Effects Vary

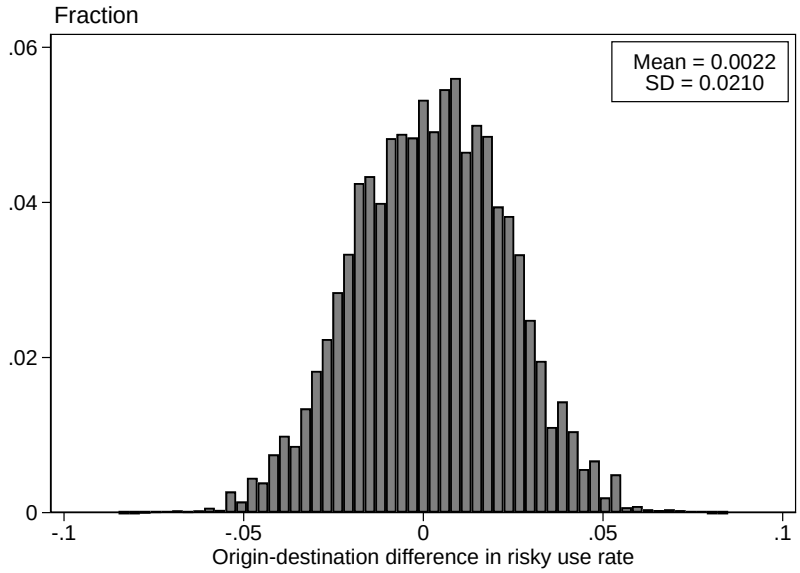


(d) Case 4: Person, Availability, and Addiction Effects Vary



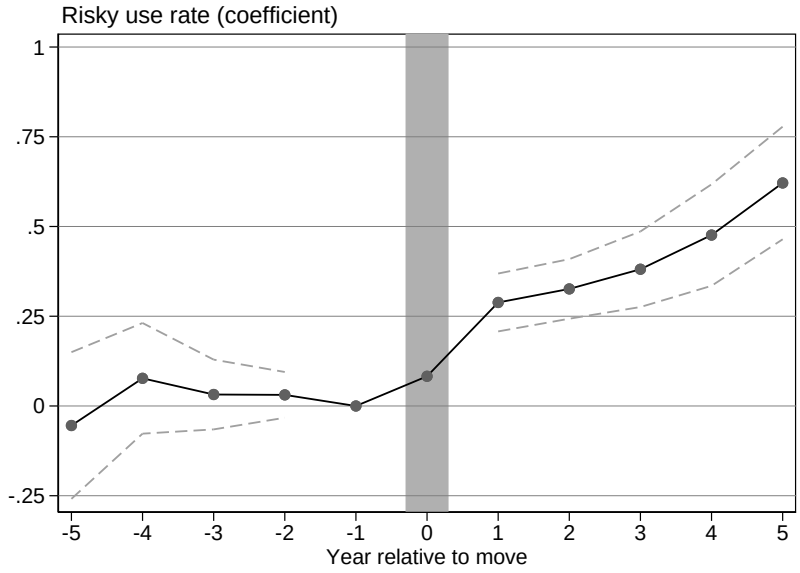
Notes: Figure shows the patterns of event study coefficients μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—as calculated according to equation (5) in a theoretical setting with a single cohort moving between two locations and the coefficients plotted on the y-axis. The cohort is randomly sampled from the origin, and risky use rates are determined according to the parameters described in Section 3. We choose an illustrative example set of parameters for each example. In addition, we assume that in relative year 0, there is a 50% chance that each individual is in their origin location and a 50% chance that they are in their destination location. In Panel A, only person-based factors differ between the two locations. In Panel B, only availability effects differ between the two locations. In Panel C, both person-based factors and availability effects differ between the two locations, and we choose a case where the ratio of shares addicted and availability effects in the origin and destination are exactly constant. Finally, in Panel D, the two locations differ in their person-based factors, availability effects, and their place-based effects on transitions to addiction. The parameters used in Panel C are $\pi^{+}_{o(c)} = \pi^{+}_{d(c)} = 0.01$, $\pi^{-}_{o(c)} = \pi^{-}_{d(c)} = 0.16$, $\gamma_{o(c)r} = 0.10$ and $\gamma_{d(c)r} = 0.40$ for $r \in [-5,5]$, $\eta^{+}_{i} = 0.01$, $\eta^{-}_{i} = 0.02$, and $a_{i0} = 0.10$ for all $i \in \mathscr{J}_{o(c)}$, and $\eta^{+}_{i} = 0.02$, $\eta^{-}_{i} = 0.01$, and $a_{i0} = 0.15$ for all $i \in \mathscr{J}_{d(c)}$. The parameters used in Panel D are $\pi^{+}_{o(c)} = 0.05$, $\pi^{+}_{d(c)} = 0.03$, $\pi^{-}_{o(c)} = 0.03$, $\pi^{-}_{d(c)} = 0.10$, $\gamma_{o(c)r} = 0.30$ and $\gamma_{d(c)r} = 0.26$ for $r \in [-5,5]$, $\eta^{+}_{i} = 0.05$, $\eta^{-}_{i} = 0.02$, and $a_{i0} = 0.10$ for all $i \in \mathscr{J}_{o(c)}$, and $\eta^{+}_{i} = 0.04$, $\eta^{-}_{i} = 0.05$, and $a_{i0} = 0.10$ for all $i \in \mathscr{J}_{d(c)}$.

Figure 3: Distribution of Origin - Destination Differences in Rate of Risky Use



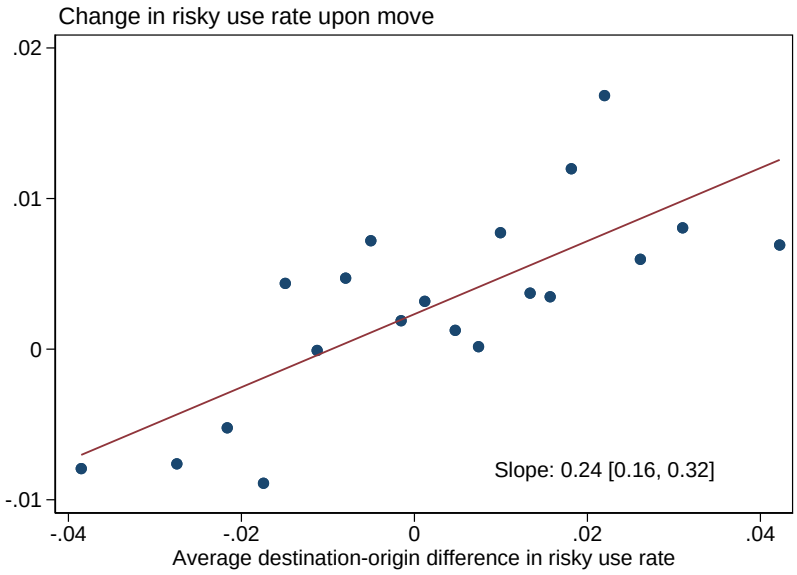
Notes: Figure shows the distribution across movers of the difference in the average rates of risky opioid use between their origin and destination states in a given year. The sample is all mover-years (N = 521,523 mover-years).

Figure 4: Event Study



Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction. The sample is all mover-years excluding movers unobserved in the year before move (N = 421,067 mover-years).

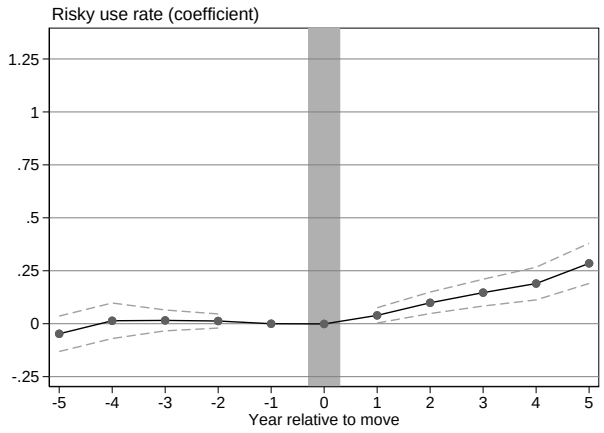
Figure 5: Change in Rate of Risky Use by Size of Move



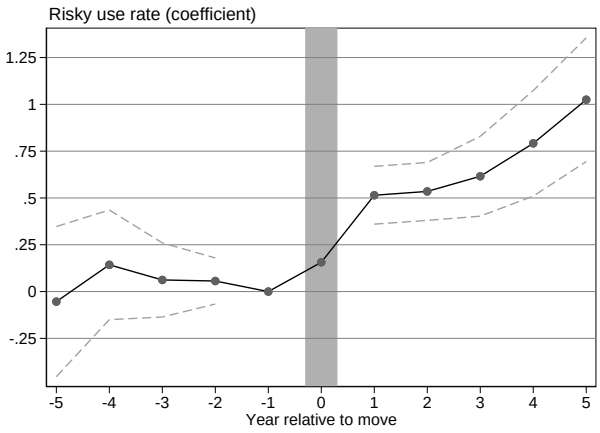
Notes: Figure shows changes in the rate of risky use from before to after move as a function of the size of the move ($\hat{\bar{\delta}}_{cr}$). For each mover, we calculate the average difference $\hat{\delta}_{cr}$ in the rate of risky use between their origin and destination states for all years they are observed between one and five years pre-move and one and five years post-move. We then group these average differences—$\hat{\bar{\delta}}_{cr}$—into ventiles. The x-axis displays the mean of $\hat{\delta}_{cr}$ for movers in each ventile. The y-axis shows, for each ventile, the average risky use rate one to five years post-move minus the average risky use rate one to five 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 with a 95% confidence interval in brackets. The sample is all mover-years between one and five years pre-move and one and five years post-move (N = 451,124 mover-years).

Figure 6: Event Studies - Naives and Prior Users

(a) Naives

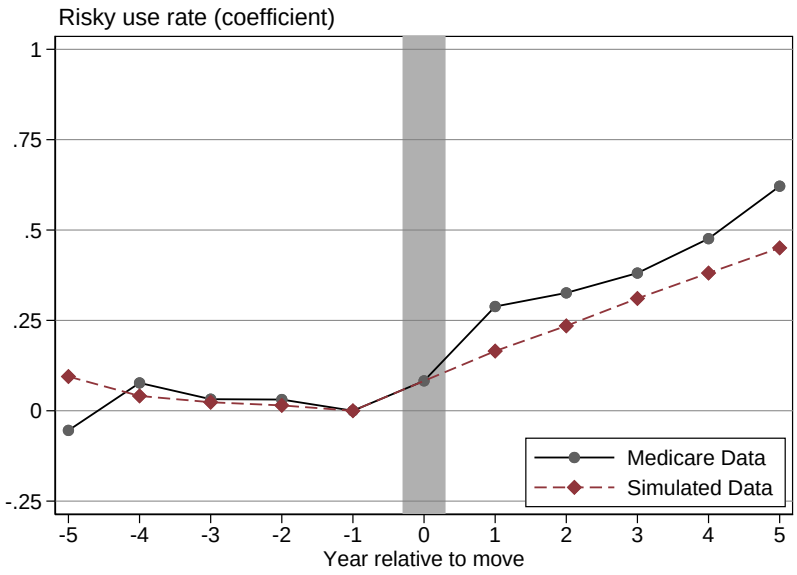


(b) Prior Users



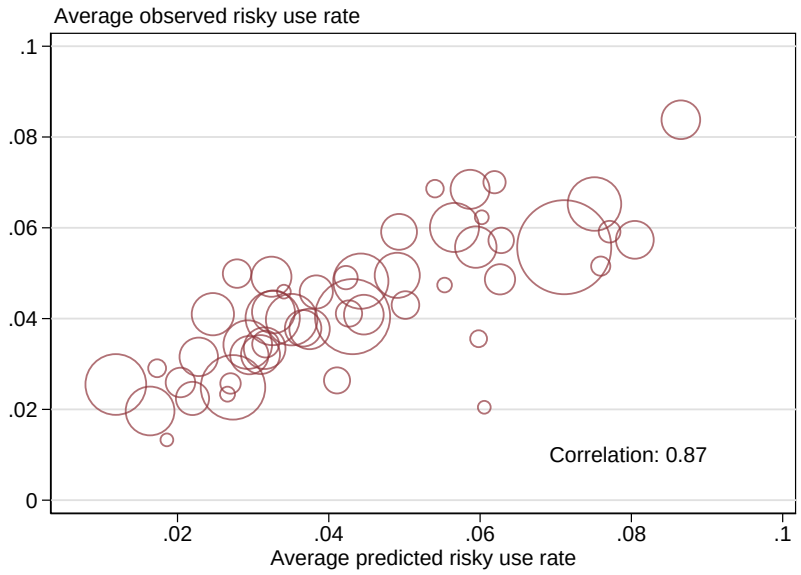
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. We show these estimates separately for opioid naive ("naive") and prior users. "Naives" are those with no opioid use in relative year -1, while prior users filled at least one opioid prescription in that year. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction. The samples are 217,922 mover-years (naives) and 202,807 mover-years (prior users).

Figure 7: Event Study in Simulation vs. Data



Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—as estimated on the simulated data and the Medicare data, where the weights are given by the number of movers observed in each cohort. The dashed line shows the aggregate coefficients estimated from our simulated event study, where the yearly rates of risky opioid use among movers and non-movers are simulated according to the model parameterizations described in Section 5.2. The solid black line is identical to Figure (4) and plots our aggregate event study coefficients as estimated on the Medicare data. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction.

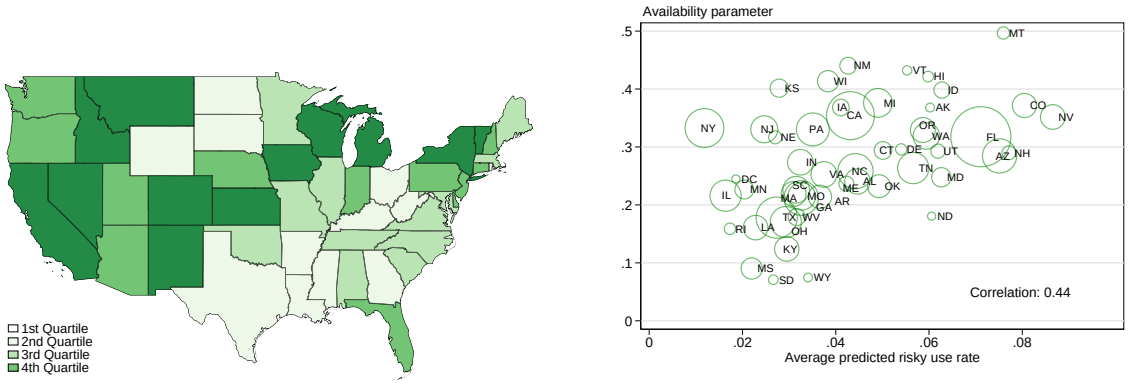
Figure 8: Simulated vs. Observed Risky Use Rates



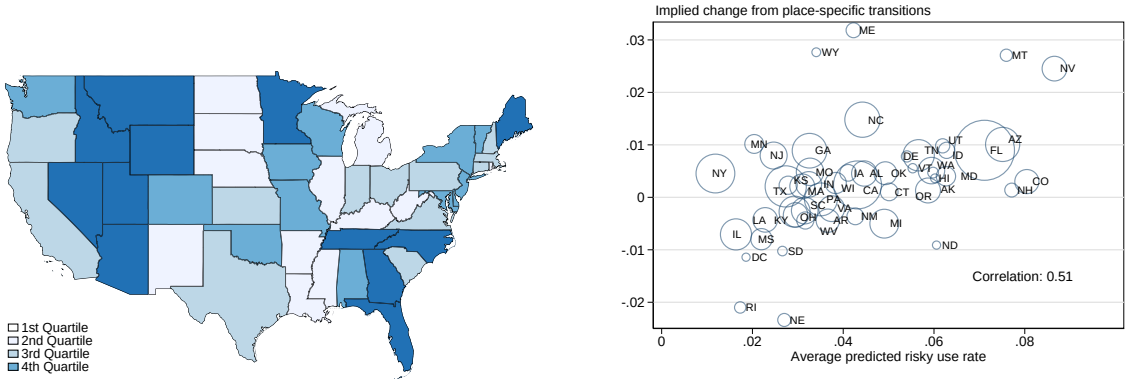
Notes: Figure presents a scatterplot of average state-level observed risky use rates against average state-level predicted risky use rates. Average risky use rates for each state are computed as an average of all non-mover years ($N = 2,325,094$ enrollee-years). The weighted correlation coefficient is presented in the bottom right corner, where weights are given by the number of mover-year observations in our estimation sample where a mover was observed in the state. Markers are scaled accordingly.

Figure 9: Geographic Variation in Calibrated Parameters

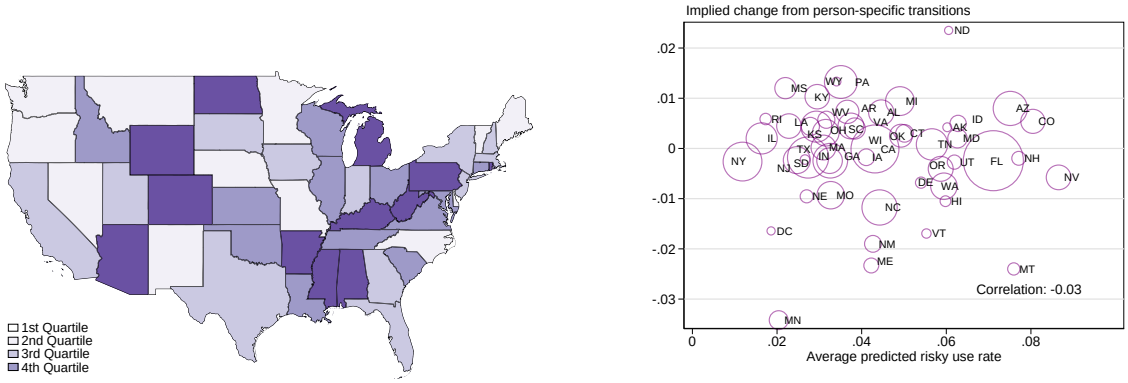
(a) Availability (γ_j)



(b) Place-Based Addiction Transitions (π_j)

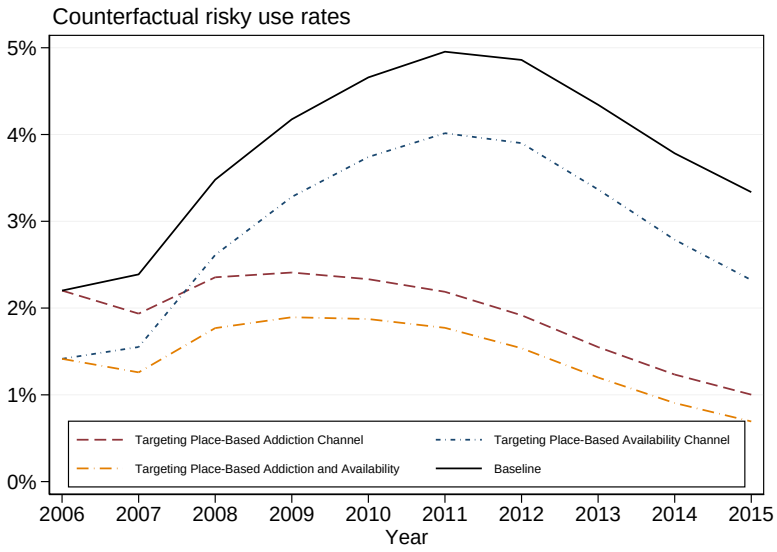


(c) Person-Based Addiction Transitions (η_i)



Notes: Figure presents three panels with both maps and scatterplots of parameter estimates for various channels in our model. Panel A presents the place-specific availability parameter, while Panels B and C present place-specific and person-specific factors affecting addiction transitions respectively. The weighted correlation coefficient is presented in the bottom right corner, where weights are given by the number of mover-year observations in the estimation sample where a mover was observed in the state. Markers are scaled accordingly. The magnitude of addiction transition parameters is measured by the change in shares addicted that would result after one year if all states shared an initial share of 0.10 and differential addiction transitions occurred only occurred according to variation in the relevant set of addiction transition parameters. The implied one-year change from place/person specific addiction transitions specifically computes $\Delta\bar{a} = \bar{a}_0\,(1-\pi^- - \eta^-) + (1-\bar{a}_0)\,(\pi^+ + \eta^+) - \bar{a}_0$, where $\bar{a}_0$ is set to the global share of addiction in our model calibration and the other set of parameters (place vs. person) are held to their median values. We discuss the calibration of a global share of addiction further in Appendix D.

Figure 10: Counterfactual Policies Targeting Risky Use



Notes: Figure shows the yearly effect of counterfactual policies that lower place-based addiction and place-based availability parameters. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The average yearly risky use rate is computed by taking a weighted average across simulated risky use outcomes in each state for our non-mover sample, weighting by the number of non-movers in each state-year. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary. The estimates shown in this figure (along with their standard errors) are reported in Appendix Table A.6.

Table 1: Summary Statistics

	(1) Movers	(2) Non-movers
Female	56%	51%
White	73%	72%
Medicaid	58%	57%
Age:		
< 40	14%	11%
40 - 60	47%	44%
> 60	38%	45%
Average age	55.3	57.4
Region:		
Northeast	20%	19%
West	22%	18%
Midwest	20%	22%
South	38%	41%
Opioid use:		
Any opioids	47.1%	41.8%
Prescriptions in year before move ("prior user")	38.8%	
No prescriptions in year before move ("opioid naive")	42.0%	
No observation in year before move	19.2%	
Risky use	4.8%	4.1%
Number of enrollee-years	521,523	13,349,773
Number of enrollees	90,890	2,325,094

Notes: All rows except for the number of enrollee-years and enrollees report the share of enrollees or enrollee-years within the given population with the indicated characteristic. "Any Opioids" and "Risky Use" are averaged over all enrollee-years, while all other statistics are averaged at the enrollee-level, with "Region," "Medicaid," and "Age" measured in a reference year. This reference year is the year before move for movers, and a randomly assigned year for non-movers such that the distribution of reference years for non-movers mirrors that of movers.

Table 2: Event Study Coefficients - Rates of Risky Use

	(1)	(2)	(3)
	All	Naive	Prior User
1 year post-move	0.288	0.039	0.515
	(0.041)	(0.019)	(0.078)
5 years post-move	0.621	0.285	1.025
	(0.080)	(0.048)	(0.169)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Average Risky Use Rate 1 year pre-move	0.048	0.000	0.099

Notes: Table reports the coefficients and their bootstrapped standard errors (in parentheses) one and five years post-move as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Estimates are shown for the baseline sample ("All"), naive enrollees, and prior users, for movers observed in the year before move for whom we are able to observe matched non-movers. 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. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table 3: The Effect of Counterfactual Reductions on Risky Use

Predicted reduction in rate of risky use from a one standard deviation reduction in...		All States	Ten Highest Risky Use States in 2006
(1)	All Place-based Parameters (γ_j, π_j)	64.6% [61.4%, 71.4%]	61.9% [56.4%, 73.7%]
(2)	All Person-based Parameters (a_{i0}, η_i)	64.1% [50.3%, 70.5%]	60.5% [46.1%, 73.8%]
(3)	Availability (γ_j)	23.8% [22.4%, 27.8%]	22.1% [20.5%, 27.5%]
(4)	Place-based Addiction Transitions (π_j)	53.1% [46.2%, 61.3%]	50.6% [44.8%, 62.4%]
(5)	Person-based Addiction Transitions (η_i)	44.1% [38.7%, 53.9%]	42.6% [36.2%, 57.5%]
(6)	All Addiction Transitions (π_j, η_i)	74.4% [66.4%, 79.7%]	68.4% [64.4%, 77.7%]
(7)	Initial Addiction States (a_{i0})	35.7% [19.0%, 40.7%]	32.2% [21.2%, 47.4%]
(8)	All Addiction Parameters (π_j, η_i, a_{i0})	87.9% [78.7%, 88.9%]	80.4% [71.6%, 88.1%]

Notes: Table reports the percent reduction in simulated overall risky use rates from 2006 to 2015 from a one standard deviation reduction in parameters. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation, except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The overall risky use rate is computed by taking a weighted average of simulated risky use over all state-years in our non-mover sample, weighting by the number of non-movers observed in each state-year. 95% confidence intervals are computed using 50 replications of the Bayesian bootstrap. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary. The ten states with the highest levels of risky use in 2006 examined in the right-hand column are Alaska, Arizona, Delaware, Maine, Montana, Nevada, New Hampshire, Oregon, Utah, and Washington.

Appendix A: Defining Risky Opioid Use

Measuring risky opioid use and adverse opioid events

As discussed in the main text, clinicians and medical researchers have not come to a consensus on a gold standard measure of risky opioid use from claims data. However, the literature uses several proxies for risky use 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 better 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 morphine equivalent dosage, or MED, (usually above 100-120 mg) are at higher risk for diagnoses of opioid dependence (Sullivan et al. 2010; Edlund et al. 2014) 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 (Hall et al. 2008; Yang et al. 2015).

We therefore construct several measures of potentially risky prescription opioid use. 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 opioid active ingredient described in the MED conversion table (Appendix Table A.4).

We define and separately analyze three indicator variables as proxies of risky use: High MED, Many Prescribers, and Overlapping Prescriptions. High MED and Many Prescribers are defined following Meara et al. (2016); Overlapping Prescriptions is defined following Logan et al. (2013) and Jena et al. (2014).

Our primary measure of risky use is "High MED". This is an indicator for whether average daily MED in any quarter of the year 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 the quarter. "Many Prescribers" is an indicator for whether an enrollee filled prescriptions associated with four or more unique physicians during the calendar year. "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 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). 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.

We also define two other indicators of prescription opioid use for an enrollee-year: Any Opioid Use and 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.

Finally, we define a number of adverse opioid outcomes. To do so, we must limit our analysis to the 75 percent of non-mover enrollee-years who are not enrolled in Medicare Advantage during the year, so that we can observe their full set of inpatient and outpatient claims. We define a "poisoning event" for each enrollee-year as an ER visit or inpatient hospital admission for poisoning. We define opioid poisonings as the subset of poisoning events that are so-labelled. We define opioid use disorder as the set of so-labelled diagnoses in the claims data.

Comparison across measures

Among non-movers, about four percent of our enrollee-years have a high average daily MED (High MED), six percent of enrollee-years include prescriptions from four or more unique prescribers (Many Prescribers), and about 15 percent of enrollee-years have overlapping opioid prescriptions (Overlapping Prescriptions). The pairwise rank correlations among these three proxies of risky opioid use are high (Appendix Table A.2) and all are positively correlated with our other indicators of prescription opioid use.

To compare our various proxies of risky opioid use, we limit our analysis to the non-mover sample with traditional Medicare for which we can fully observe observe opioid poisonings and opioid use disorders. We further restrict to non-mover enrollee years whom we observe the following year, so that we can examine the relationship between our measures of opioid use in year t and adverse outcomes in year $t+1$ (Appendix Table A.1).

In Panel A, we see that on average, one percent of our sample is diagnosed with an opioid use disorder each year and two-tenths of a percent of our sample is diagnosed with an opioid poisoning event. These adverse outcomes are significantly more prevalent among individuals with our measures of risky opioid use in the year preceding. For example, while only four percent of our overall population is classified as High MED, the share classified as High MED is about six and one half times more common among enrollees with observed opioid use disorder in the following year and about seven and one half times more common among enrollees with an opioid poisoning event in the following year.

In Panel B, we translate these rates into a measure of diagnostic accuracy by constructing the positive likelihood ratio. The positive likelihood ratio is defined for each opioid measure and each adverse outcome as the ratio of true to false positives when the opioid measure in year t is used as a diagnostic for the adverse outcome in year $t+1$. We chose High MED as our baseline measure of risky opioid use because, across all five opioid use measures we examined, it has the highest positive likelihood ratio for both poisonings and opioid use.

Finally, for our preferred measure of risky opioid use—High MED—we examine how geographic patterns of risky use in our population correlate with geographic patterns in adverse opioid outcomes in the overall US population. To do so, we use 2006-2014 state-level data from the CDC Multiple Cause of Death (MCD) File[19] 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 2006-2014 National Survey on Drug Use and Health (NSDUH)[20] 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, we calculate that both measures have a high rank correlation with our risky opioid use measure; the average yearly rank correlation is 0.52 for the opioid-related death rate, and 0.46 for the self-reported opioid misuse rate.

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

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

Appendix B: Additional Analyses

Comparison of patterns in our sample to overall U.S. population

We 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 and Prevention. QuintilesIMS collects data on prescriptions based on a sample of 59,000 retail pharmacies, which collectively dispense nearly 88 percent of all prescriptions in the U.S (CDC 2017). The aggregated QuintilesIMS data set contains the number of opioid prescriptions per capita in each year.[21] We define an identical variable in our data, the number of opioid prescription fills per capita (Opioid Fills). Both are defined over the 2006-2014 time period.

Appendix Figure A.7 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 of 0.80 at the state level.

Robustness of Descriptive Analysis

The descriptive results in Section 4 on risky opioid use are robust to several alternative specifications, including defining the geographic areas of interest at different levels, excluding individual and relative year fixed effects, and defining our sample in alternative ways to handle entry or exit by enrollees during our sample period.

Appendix Figure A.8 presents our event study results when estimated at various geographic levels (coefficients in Table A.5). Panel A shows our baseline results, with state as the geographic unit, for ease of comparison. We find that our event study results are similar if we instead define the geographic areas of interest—and hence redefine who is a mover—at the commuting zone level (Panel B) or the county level (Panel C), rather than the state level. Commuting zones are collections of counties; there are about 700 commuting zones and 3,000 counties in the United States.

We also consider the robustness of our analysis to alternative ways the literature has measured risky opioid use. As we discussed in Appendix A, two alternative measures of risky use in the literature are the Many Prescribers and Overlapping Prescriptions measures. We show here that the basic event study results from estimating equation 3 are similar if we instead use these alternative measures instead of our baseline measure, High MED. Appendix Table A.3 presents summary statistics on these measures, Appendix Table A.7 summarizes the results, and Appendix Figure A.9 shows the corresponding event studies. The immediate discrete jump upon move using Overlapping Prescriptions is similar to that of our baseline measure, while the jump for Many Prescribers is larger. The post-move convergence is somewhat less pronounced with these other measures.[22]

Appendix Figures A.4 and A.10 show robustness to modifications in our event study specification, summarized in Table A.8. In Figure A.4, we define the dependent variable in levels, rather than in differences; in other words, instead of defining $\hat{T}^X_{cr}$ to be the average of the difference in differences $(y_{ir} - y_{i,-1}) - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)$ among the set $\mathscr{I}^X_c$ of movers in cohort c with characteristics X, we define it as $\left(y_{ir} - \hat{m}^X_{cr}\right)$.

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

[22] A lack of post-trends is plausible within our model framework. In Section 4.2, we analyzed a case (Case 4) in which place-based addiction transitions were higher in higher risky use locations. However, if we relax this assumption—consider, for example, if high risky use rates were primarily driven by person-specific factors or availability rather than place-based addiction factors—the average risky use rates in a cohort could converge in the opposite direction of δ_{cr}. Thus, we could see zero or even negative post-trends.

As in our baseline specification, we then let $\hat{T}_{cr}$ denote the average of $\hat{T}_{cr}^{X}$ across X, weighting by the number of movers in c with each set of characteristics X. With this alternative specification, we no longer adjust for any fixed individual differences in propensity for risky use. The figure shows that we continue to find little systematic pre-move trend, and a similar post-move pattern of coefficients. However, the level of the pre-move coefficients is now about 0.15, indicating that there is potential selection in where movers move. In particular, the results indicate that movers to destinations with a higher propensity for risky use than in their origin tend, prior to their move, to have a higher propensity for risky use than matched non-movers in their origin.

Figure A.10, Panel A presents our results if we exclude relative year fixed effects from estimation of equation (3). Our baseline specification constructs the dependent variable $\hat{T}_{cr}$ in equation (3) by averaging across $\hat{T}_{cr}^{X}$ across movers in cohort c, where X consists of five-year age bins (as measured by birth year), race, and gender. Panel B presents our results when we omit any conditioning on covariates X in constructing $\hat{T}_{cr}$. We continue to observe the key post-move features of our event study: a discrete jump upon move and then post-trends.

Appendix Figure A.11 suggests that these features also hold in a balanced panel of individuals who are observed in our sample for three years pre- and post-move, indicating that these features are not simply driven by the changing composition of groups at each length of exposure to the new location.

We find that our results are also stable across alternative ways of defining our sample to handle entry or exit by enrollees during our sample period. We present these results in Appendix Figure A.12, and summarize them in Appendix Table A.9. Our baseline sample restricts to enrollee-years where enrollees are covered for the full year (i.e. have 12 months of Part D coverage). A given enrollee may therefore enter or exit the sample if she gains or loses just a few months of Part D coverage. Panel A therefore shows results if we further restrict the sample to enrollees who had Part D coverage for the full year (all 12 months) in all years they appear in our sample, while Panel B restricts to enrollees who are alive in all years during our study period. We also show that our results are not driven by only the most common moves or by patients who might be expected to have very high opioid use. In Figure A.12, Panel C, we exclude moves to or from the three most common destinations in our sample (Florida, Texas, and California), and in Panel D, we exclude from the sample all enrollees for whom we observe a cancer diagnosis or hospice stay.

Finally, to further explore the potential impacts of selection into our sample, Appendix Figure A.13 estimates equation (3) using as the dependent variable whether the enrollee-year had 12 months of Part D coverage. For all of these analyses, $\hat{\delta}_{cr}$ is still defined based on risky opioid use. The results show some evidence of selection into Part D upon move; for example, a move from a 20th percentile risky opioid use state to a 80th percentile state is associated with about a two percentage point increase in the probability of having Part D coverage (relative to a mean of 80 percent). Reassuringly, however, Table A.9 shows that restricting attention to individuals whose selection along this margin does not change during the sample does not affect our main results.

Appendix C: Proof of Proposition 1

We begin by showing that under Assumption 1, $\mathbb{E}_{i\in\mathscr{I}_c}\left[\hat{T}_{cr}\right] = T_{cr} + \rho_r$. To see this, we note that the average treatment effect among movers with characteristics X is:

$$\begin{aligned}
T^X_{cr} =& \mathbb{E}_{i\in\mathscr{I}^X_c}\left[y_{ir}\left(\mathbf{h}_{cr}\right) - y_{ir}\left(\mathbf{h}^0_{cr}\right)\right] \\
=& \mathbb{E}_{i\in\mathscr{I}^X_c}\left[\left(y_{ir}\left(\mathbf{h}_{cr}\right) - y_{i,-1}\left(\mathbf{h}^0_{c,-1}\right)\right) - \left(y_{ir}\left(\mathbf{h}^0_{cr}\right) - y_{i,-1}\left(\mathbf{h}^0_{c,-1}\right)\right)\right] \\
=& \mathbb{E}_{i\in\mathscr{I}^X_c}\left[y_{ir}\left(\mathbf{h}_{cr}\right) - y_{i,-1}\left(\mathbf{h}^0_{c,-1}\right) - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)\right] - \rho_r \\
=& \mathbb{E}_{i\in\mathscr{I}^X_c}\left[y_{ir} - y_{i,-1} - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)\right] - \rho_r \\
=& \mathbb{E}_{i\in\mathscr{I}^X_c}\left[\hat{T}^X_{cr}\right] - \rho_r
\end{aligned}$$

where the third line follows from Assumption 1 and the fourth line follows from the facts that $\mathbf{h}^0_{c,-1} = \mathbf{h}_{c,-1}$ and the fact that $y_{ir}\left(\mathbf{h}_{it}\right)$ is equal to the actual observed outcomes y_{ir} for all r. Averaging over X then implies $\mathbb{E}_{i\in\mathscr{I}_c}\left[\hat{T}_{cr}\right] = T_{cr} + \rho_r$. Next, to show that $\hat{T}_{cr}$ is also a consistent estimator for $T_{cr} + \rho_r$, we recall that $\hat{T}^X_{cr}$ is simply the sample average of $y_{ir} - y_{i,-1} - \left(\hat{m}^X_{cr} - \hat{m}^X_{c,-1}\right)$ among movers in cohort c with characteristics X. It follows that $\hat{T}^X_{cr} \to_p T^X_{cr} + \rho_r$, and averaging once again over X implies that $\hat{T}_{cr} \to_p T_{cr} + \rho_r$.

Next, we would like to show that the event study coefficients $\hat{\mu}_r$ from equation (3) converge in probability to

$$\mu_r = \sum_c w_c \frac{T_{cr}}{\delta_{cr}}.$$

Let $\hat{\bar{\delta}}_{cr}$ and $\hat{\bar{T}}_{cr}$ denote the $|\mathscr{I}_c|$-weighted means of $\hat{\delta}_{cr}$ and $\hat{T}_{cr}$ respectively across c within r. The residuals from respective weighted regressions of $\hat{\delta}_{cr}$ and $\hat{T}_{cr}$ on relative year fixed effects ρ_r are thus $\left(\hat{\delta}_{cr} - \hat{\bar{\delta}}_{cr}\right)$ and $\left(\hat{T}_{cr} - \hat{\bar{T}}_{cr}\right)$, and so

$$\hat{\mu}_r = \frac{\sum_c |\mathscr{I}_c| \left(\hat{\delta}_{cr} - \hat{\bar{\delta}}_{cr}\right)\left(\hat{T}_{cr} - \hat{\bar{T}}_{cr}\right)}{\sum_c |\mathscr{I}_c| \left(\hat{\delta}_{cr} - \hat{\bar{\delta}}_{cr}\right)^2} \to_p \frac{\sum_c |\mathscr{I}_c| \delta_{cr} T_{cr}}{\sum_c |\mathscr{I}_c| \delta^2_{cr}}.$$

where the first equality follows from the Frish-Waugh theorem and the convergence in probability follows from the facts that (i) $\hat{\bar{\delta}}_{cr} \to_p 0$ (since $\hat{\delta}_{cr}$ is demeaned), (ii) $\hat{\delta}_{cr} \to_p \delta_{cr}$, and (iii) $\hat{T}_{cr} \to_p T_{cr} + \rho_r$ as shown above.

Appendix D: Estimation Details

Generalized Method of Moments

The model as parameterized in Section 5.2 includes six sets of parameter which vary by location—$\bar{a}_{j0}$, γ_j, π^+_j, π^-_j, η^+_j, η^-_j—as well as the level of selection s, the separable temporal component of availability τ_t for each of the ten years of our sample, and the impacts of moving upon availability ρ_r for the five years before and five years after the move.

In order to avoid collinearity in the sums that are used in the computation of addiction ($\pi_j + \eta_j$) and availability ($\gamma_j + \tau_t + \rho_r$) in our model, we normalize three sets of values to zero as references: the first calendar year availability parameter, the effect of the fifth-year pre-move on availability, and an arbitrary state's person-specific addiction transition effects. The vector of parameters to be estimated therefore consists of 323 values and is given by $\theta = \left\{\bar{a}_{j0}, \gamma_j, \pi^+_j, \pi^-_j, \eta^+_j, \eta^-_j, s, \tau_t, \rho_r\right\}$.

We construct our score function as follows. We first construct the vector-valued function of average risky use rates by $M(\theta)$, where cr indexes each observed combination of cohort and relative year and each entry $M_{cr}(\theta)$ is given by the average cohort risky use rate $\bar{y}_{cr}$ as calculated according to Section 5.2 using the vector of parameters θ. Next, we allow the vectors $Z_i \coloneqq \{(Y_{ir}, X_{ir}^{'})^{'}\}_r$ to be the vector collecting the variables for individuals i across relative years r, where

$$Y_{ir} = \begin{cases} y_{ir} & \text{if } i \text{ is observed in relative year } r \\ 0 & \text{else} \end{cases}$$

denotes the risky use outcome of the individual in any given relative year and X_{ir} is a vector with a 1 in the index cr where the individual-year combination ir is a subset of cohort-year ct and a 0 in all other entries. Finally, for each of our observed combinations cr, we construct a corresponding function g_{cr} as

$$g_{cr}(Z_i, \theta) = [(Y_{ir} - M_{cr}(\theta))] X_{ir}^{'} W_{cr},$$

where W_{cr} is a vector with 1 in the entry corresponding to cr and a 0 everywhere else. We stack these functions g_{cr} into the score function $g(Z, \theta)$, where Z denotes a generic observation from the population.

Using $g(\theta) \equiv \mathbb{E}g(Z, \theta)$ to denote the expectation of this score function with respect to the generic observation from the population, we then define the corresponding empirical estimator $\hat{g}(\theta)$ as $\frac{1}{N}\sum_{i=1}^{N} g(Z_i, \theta)$ where Z_i now denotes an observation from our sample. The GMM estimator is therefore defined by

$$\hat{\theta} = \arg\min_{\theta} \hat{g}(\theta)^{'} \hat{A} \hat{g}(\theta),$$

where we choose the weighting matrix $\hat{A}$ to be the diagonal matrix where each diagonal entry is the number of individuals in our sample who were observed in the relevant cohort-year cr. We choose this weighting matrix because the typical two-step procedure is ill-suited for the structure of our problem, which includes moments that correspond to very few observations in our data.

Parameter Space Constraints

We constrain our parameter space in two ways. First, as our parameters describe probabilities and rates, we require that all addiction and availability parameters that an individual could be subject to in the model are bounded between 0 and 1. In practice, we find that this constraint does not bind for availability. The constraint binds at 0 for 2% of possible pairwise combinations of states for the probability of transitioning into addiction, and it binds at 0 for 0.3% of pairwise combinations of states for the probability of transitioning out of addiction. Second, we restrict our parameter space such that the combination of parameters generates a weighted global mean share of addiction equal to the specified value $\bar{a} = 0.10$. Specifically, we require our estimation to satisfy

$$\bar{a} = \sum_{m=2007}^{2014} \sum_{c \in \mathscr{C}_m} \hat{\bar{a}}_{cr} \frac{\hat{n}_{cr}}{N},$$

where $\hat{n}_{cr}$ denotes the number of enrollee-years used to construct the moment $\hat{y}_{cr}$, $\bar{a}_{cr}$ denotes the average share of addiction for a given cohort and relative year (as calculated in Section 5.2), and N is the total number of enrollee-years in the sample.

We view this constraint as a normalization because the multiplicative property of our outcome implies that the relative magnitudes of addiction and availability parameters are not meaningful or well-identified. For example, doubling the shares of addiction and halving all availability parameters would leave predicted risky use unchanged in our model. While our specific parameterizations mean that it is not always possible

to directly change all addiction or all availability parameters in such a way, this restriction is driven by our functional form assumptions. Thus, we instead focus on the meaningful distribution of parameters within the addiction and availability channels, and we simply set a global mean share of addiction.

We therefore draw our global mean share of addiction among all individual-years from estimates of non-medical prescription pain reliever (PPR) use in the Department of Health and Human Services' National Survey on Drug Use and Health (NSDUH). Estimates from the mid-year of our sample (the 2011 NSDUH) suggest the prevalence of non-medical PPR use was roughly 4% over a 12-month period and 13% over the lifetime of the individuals surveyed (estimates are nearly identical within the small set of SSDI recipients surveyed). Noting that our model implies that the share of addicted individuals must be greater than or equal to the observed rate of risky opioid use and that individuals transition in and out of addiction, we choose 10% as a rough anchor for addiction between annual and lifetime non-medical opioid use. We also show the robustness of our parameter estimates to various other values of $\bar{a}$ in the robustness subsection below.

Estimation Implementation

Our actual estimation occurs in three steps. In the first step of our estimation, we set various initial values for the optimization algorithm. Using sixteen sets of initial values, we set parameter values equal to a constant ranging between 0.00 and 0.15, including the end-points and evenly distributed between. Second, we minimize the weighted distance between the empirical and predicted moments by using the Levenberg-Marquardt algorithm separately for each of these initial values (Moré 1978). We use the penalty approach to non-linear optimization in order to enforce our parameter space constraints. The algorithm converges when the consecutive values of the sum of squares in the objective function are less than 10^{-10} apart. Finally, we select the globally optimal solution as our solution.

Monte Carlo Exercises

We examine the finite-sample properties of our estimators by conducting Monte Carlo simulations. Using the results from Section 5.3 as the true set of parameters, we simulate the risky use of the mover estimation sample one hundred times. We present visualizations of the bias in our estimators through scatterplots in Appendix Figure A.14. The results suggest that estimators for many of our key sets of parameter—initial share addicted and availability in particular—behave well in our finite estimation sample.

We also use the Monte Carlo simulations to assess the consistency of our key counterfactual results. We find evidence in Tables A.12 and A.13 that the bias in the estimators of these effects is limited and unlikely to affect our conclusions.

Robustness

In this section, we examine several extensions and modifications of our model and show in Table A.14 that our key counterfactual results are highly stable across these extensions and modifications. In particular, we conduct the counterfactual exercise from Table 3 with each of these, and we present our results from our baseline model in column (a) for ease of comparison.

In column (b), we examine the model specification which includes the year of move in the computation of shares of addiction among move cohorts. Thus, we allow movers to undergo another year of transitioning to and from addiction between move from their origins in relative year -1 to the destinations in relative year 1. In this extension, we assume that addiction among move cohorts evolves according to the place-specific addiction transition parameters of their origin during the move year. In column (c), we estimate our specification while restricting selection such that $s = 0$.

In column (d), we examine the specification where we allow for individuals who are not addicted to receive risky prescriptions with a constant probability across time and geographies. Denoting this probability as λ, we have that the average risky use rate for cohort c and relative year r becomes

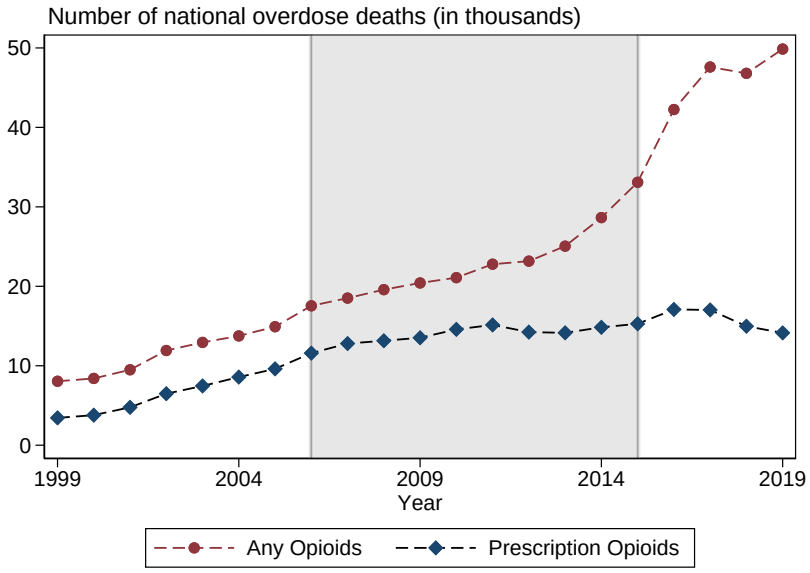
$$\bar{y}_{cr} = \bar{a}_{cr} \cdot \gamma_{cr} + (1 - \bar{a}_{cr}) \cdot \lambda.$$

We estimate this parameter λ as an additional parameter in our model estimation, and the probability of risky use among non-addicted individuals that we estimate is 1.1%.

Finally, in columns (e) and (f), we consider modifications of our baseline model which use alternative specifications of the global share of addiction $\bar{a}$.

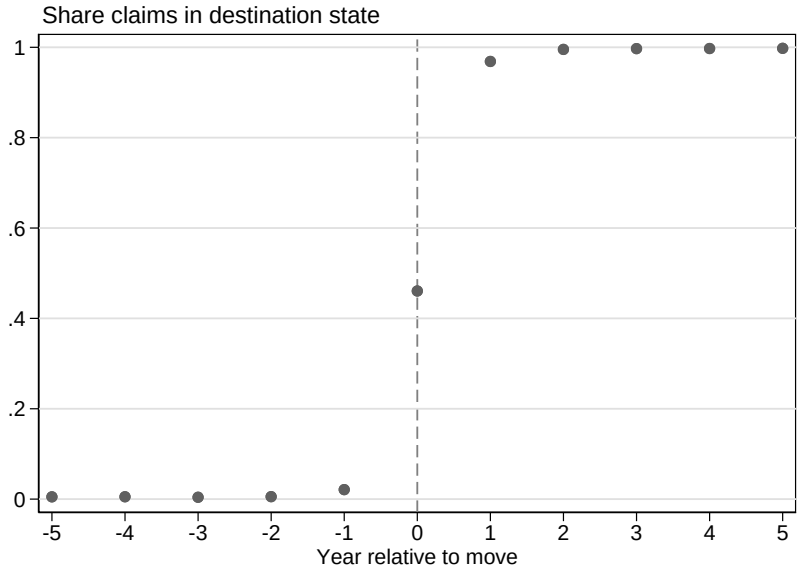
Appendix E: Appendix Figures and Tables

Figure A.1: Age-Adjusted Opioid Overdose Death Rates



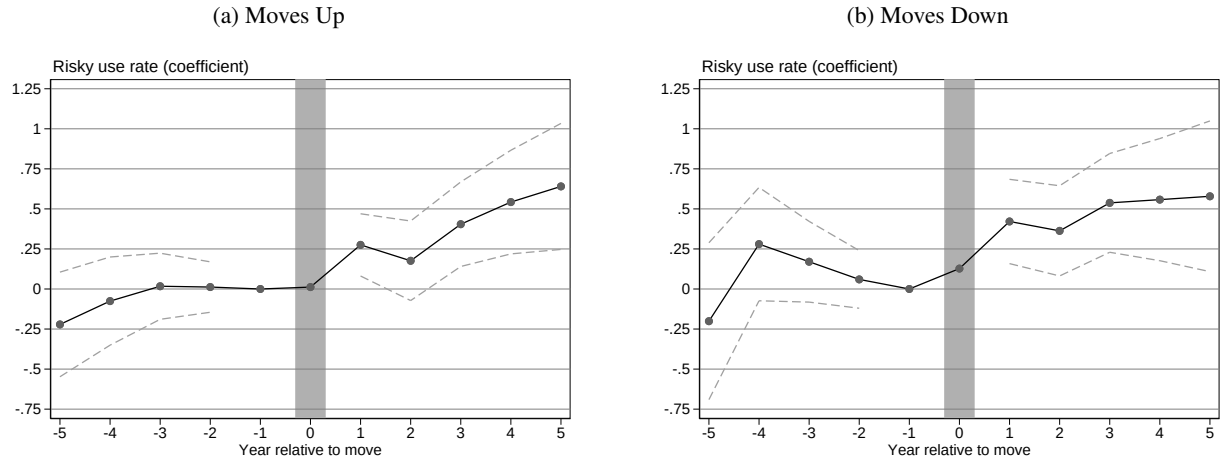
Notes: Figure presents the number of national opioid overdose deaths and the subset of those which involved prescription opioids. The years of our sample – 2006 through 2015 – are shaded. The data was directly obtained from the CDC WONDER Multiple Cause of Death (MCD) Files (National Center for Health Statistics 2021).

Figure A.2: Share of Claims in Destination State by Relative Year



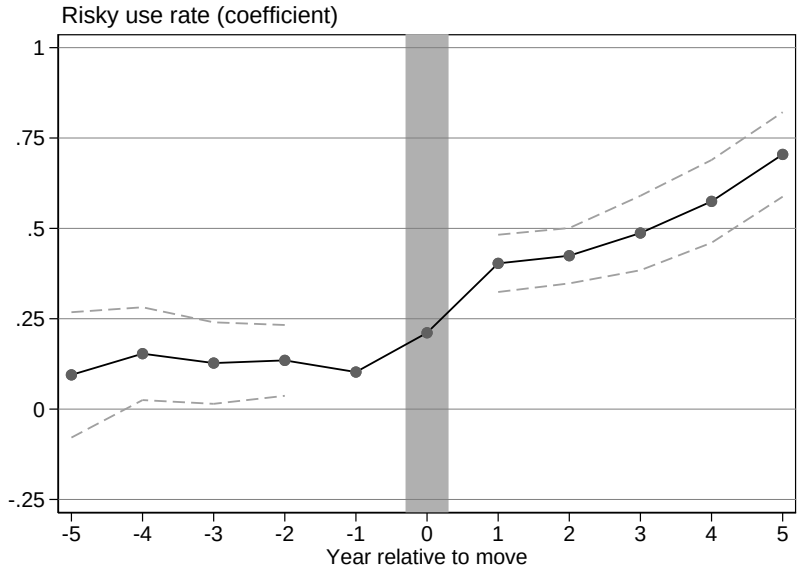
Notes: Figure presents the average fraction of claims in a mover's destination state (as a fraction of claims in either the origin or destination state) by year relative to move. Observations are at the mover-year level. The figure shows a sharp change in the year of the move, with only a small share of claims in the destination pre-move or in the origin post-move. The claim share in the year of the move (relative year 0) is close to 0.5, consistent with moves being roughly uniform throughout the year.

Figure A.3: Event Studies - Moves Up vs. Moves Down



Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. In panel (a), we restrict our sample to movers who moved to a location with a higher risky use rate (N = 229,884 enrollee-years) while in panel (b), we restrict our sample to movers who moved to a location with a lower risky use rate (N = 191,183 enrollee-years). We define "higher" and "lower" risky use locations by comparing the average rates of risky use among non-movers in each mover's origin and destination during the year of their move. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction.

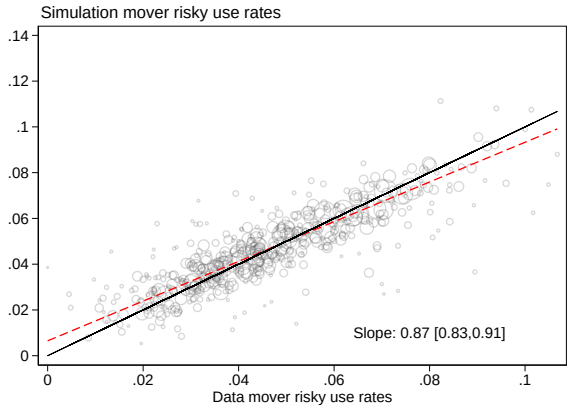
Figure A.4: Event Studies – No Individual Fixed Effects



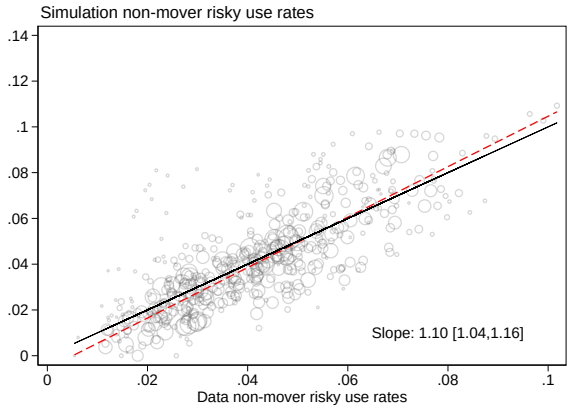
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort and omitting individual fixed effects (i.e. estimating the model in levels rather than in differences). The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that, while our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction for event studies when the model is estimated in differences, there is no such mechanical implication for relative year -1 here. This specification is discussed in more detail in Appendix B, and sample sizes are given in Appendix Table A.8.

Figure A.5: Model Calibrations

(a) Movers



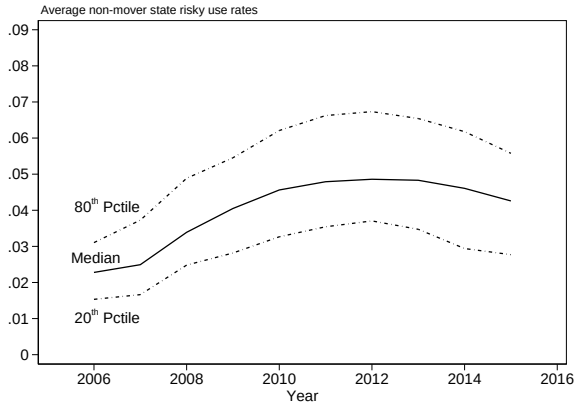
(b) Non-movers



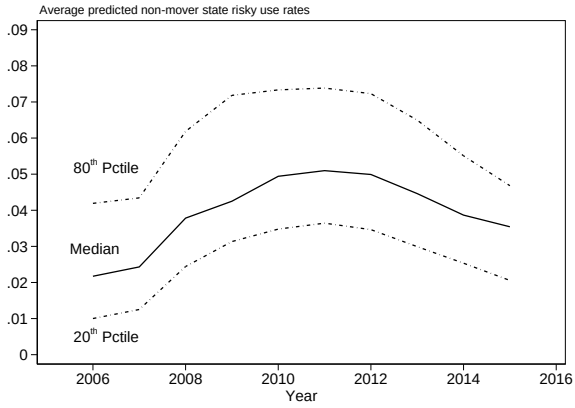
Notes: Figure compares mover (Panel A) and non-mover (Panel B) mean risky opioid use rates predicted from our simulations to those in the Medicare data. Each moment is a state and calendar year combination. In each panel, the y-axis corresponds to the mean of each moment across 50 simulations, and the solid line in each figure shows the 45 degree line. The size of each observation is proportional to the number of enrollee-years used in constructing the moment. The dashed line shows the line of best fit, using weighted least-squares with weights corresponding to the number of enrollee-years used in constructing the moment. The slope is presented in the bottom left-hand corner, with 95% confidence intervals presented in brackets.

Figure A.6: Time Series of Average Risky Opioid Use Rates

(a) Medicare Data

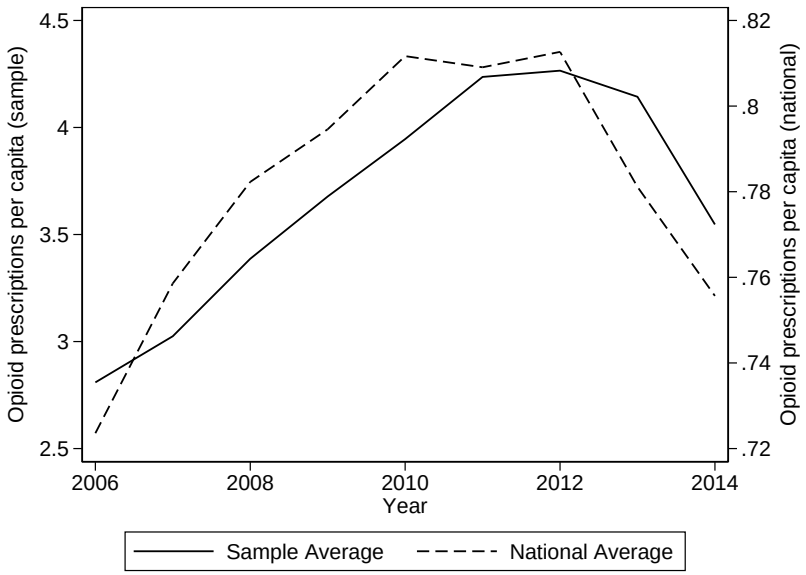


(b) Model Predictions



Notes: Figures show time series of average risky use rates among non-movers in the 20th, 50th, and 80th percentile states of the Medicare data (Panel A) and of the model predictions in each year (Panel B). Percentiles are calculated annually, so each line may correspond to different states in each year. Panel A shows results from the Medicare data, while Panel B shows predicted non-mover risky use rates from parameters estimated in our state-level movers only model.

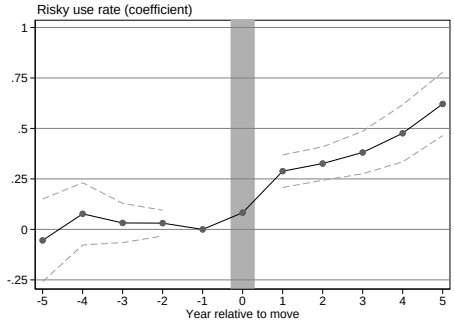
Figure A.7: Time Trends in Opioid Prescribing Rates



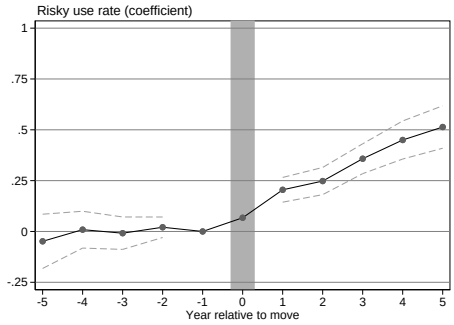
Notes: Figure presents averages over time for 2006-2014 of the number of opioid prescriptions per capita among non-movers and from national QuintilesIMS data. Sample prescriptions per capita are plotted on the left-hand vertical axis, and national prescriptions are plotted on the right. Both prescription rates are calculated on a January-December calendar year.

Figure A.8: Event Studies - Various Geographies

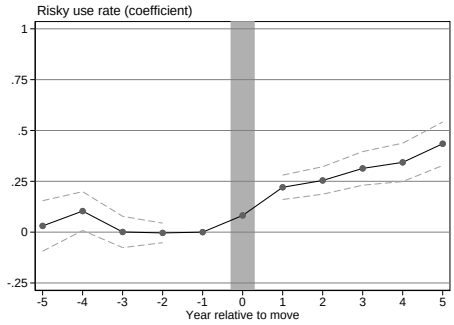
(a) Baseline Geography (State)



(b) Commuting Zone



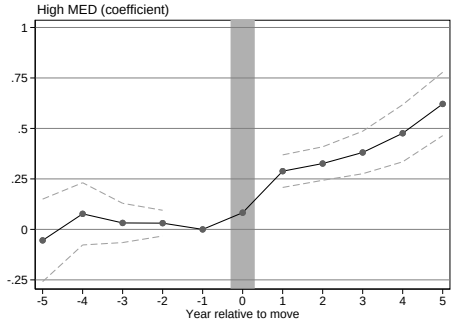
(c) County



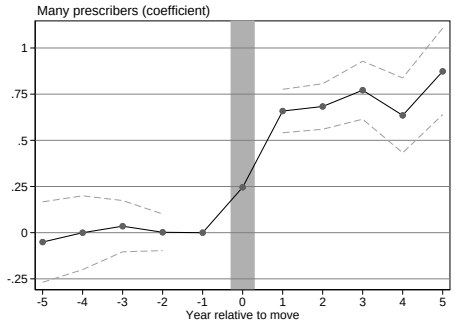
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction. Panel A is our baseline specification, Panel B uses commuting zones as the moving geography, and Panel C uses counties as the moving geography. These specifications are discussed in Appendix B, and sample sizes are given in Appendix Table A.5.

Figure A.9: Event Studies - Other Outcomes

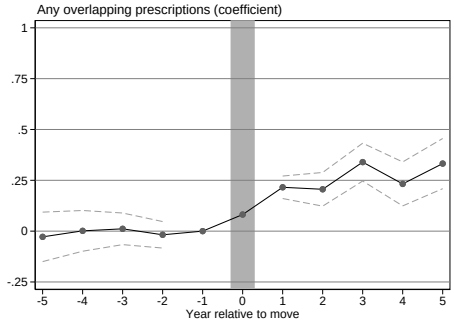
(a) High MED (Baseline)



(b) Many Prescribers



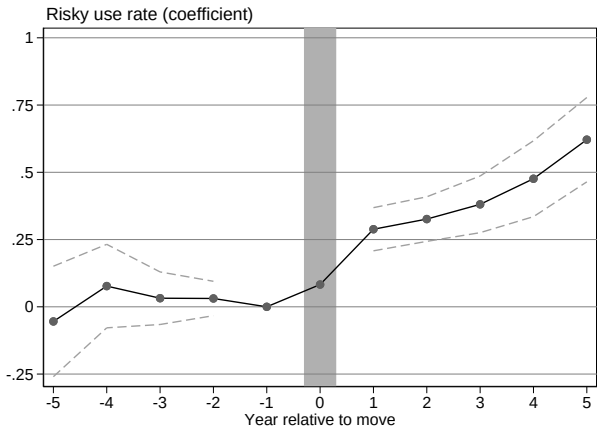
(c) Overlapping Prescriptions



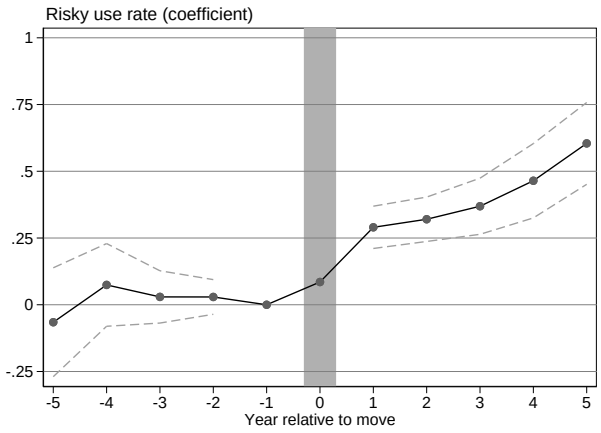
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort, and y_{ir} (when used to construct $\hat{T}_{cr}$) is defined as the indicated opioid use measure for individual i in relative year r. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Panel A reports results using the baseline measure (high morphine equivalent doses or "High MED") while Panels B and C report the results with alternate risky use measures of Many Prescribers and Overlapping Prescriptions. These risky use measures are discussed in Appendix A and sample sizes are given in Table A.7.

Figure A.10: Event Studies – Alternative Specifications

(a) No Relative Year Fixed Effects

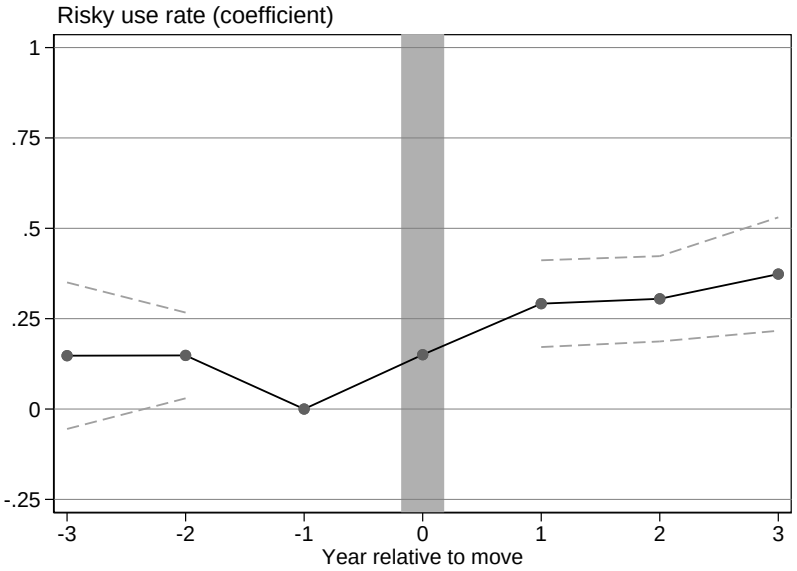


(b) No Covariates (X)



Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Each panel shows a single deviation from our baseline specification. Panel A omits the relative year fixed effects (ρ_r), and Panel B omits the covariates for five-year age bin, race and gender. These specifications are discussed in more detail in Appendix B, and sample sizes are given in Appendix Table A.8.

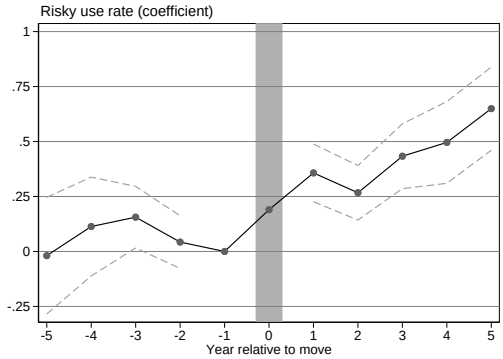
Figure A.11: Event Studies – Balanced Panel



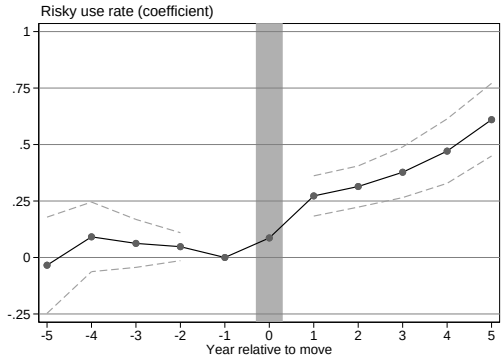
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—for a balanced panel of individuals with observed Part D coverage three years before and three years after the move, where the weights on the regression are given by the number of movers observed in each cohort. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction. The sample consists of movers who are observed for all three years before and after their move (N = 123,135 enrollee-years).

Figure A.12: Event Studies – Sample Restrictions

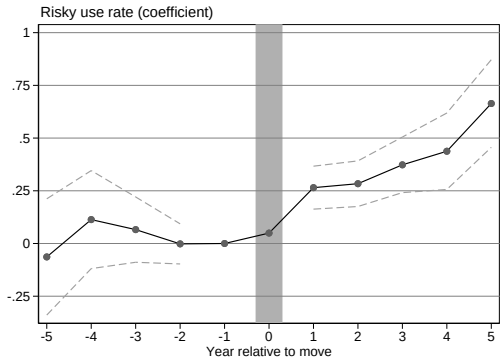
(a) Always 12 Months of Part D Coverage



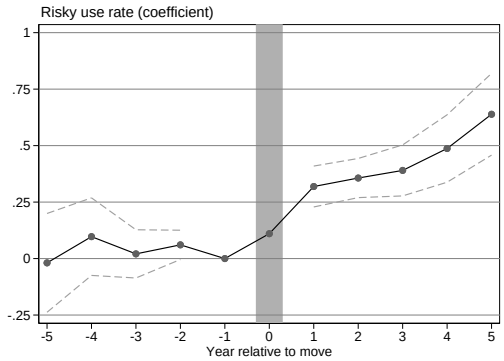
(b) Alive All Years



(c) No Moves to/from FL/TX/CA

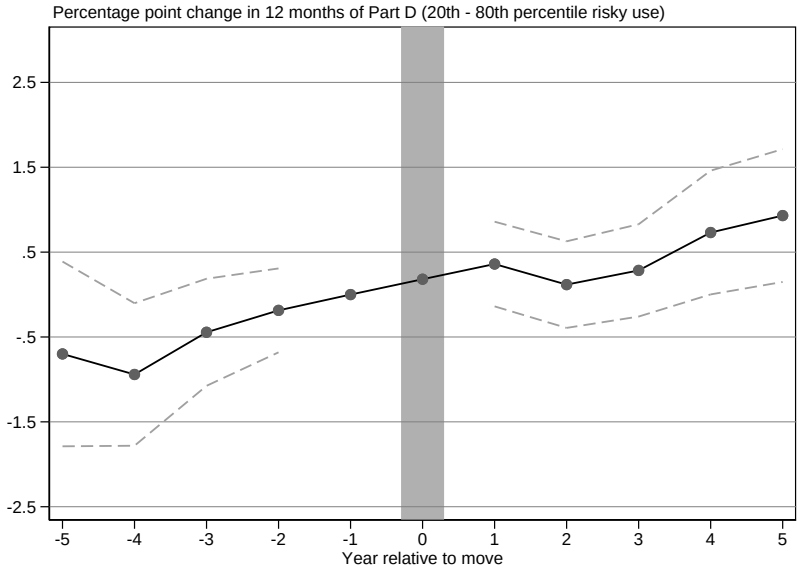


(d) No Cancer/Hospice Patients



Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Panel A restricts to movers with 12 months of Part D coverage for each year they are observed in the sample, and we drop movers who are ever observed with less than a full year of coverage; Panel B restricts to all movers who did not die from 2006 to 2015; Panel C removes all moves to or from Florida, Texas, and California from the sample; and Panel D removes all cancer or hospice patients. These specifications are discussed in Appendix B, and sample sizes are given in Table A.9. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction.

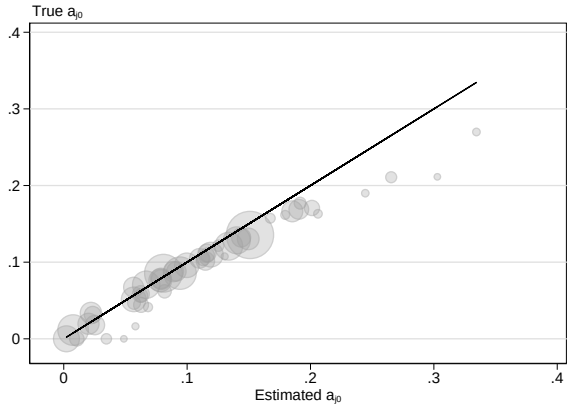
Figure A.13: Event Studies - Selection on Part D Extensive Margin



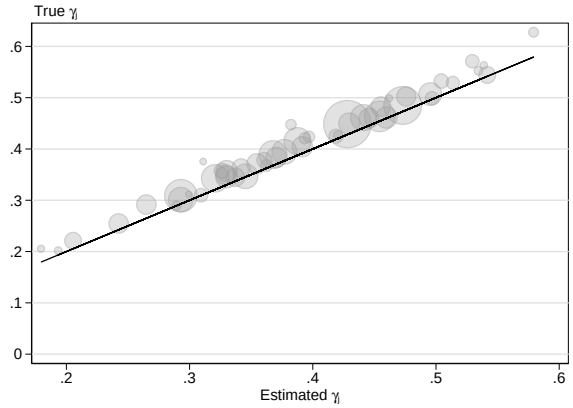
Notes: Figure shows estimates from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort, and y_{ir} (when used to construct $\hat{T}_{cr}$) is defined as having twelve months of Part D coverage during the year. The dashed lines are upper and lower bounds of the 95% confidence interval. Confidence intervals are computed using 50 replications of the Bayesian bootstrap. Note that our definition of $\hat{T}_{cr}$ in Section 4.1 implies that the coefficient in relative year -1 is zero by construction. The y-axis is scaled to represent a 20th percentile to 80th percentile move in the rate of risky opioid use. The sample is all mover-years, including mover-years without 12 complete months of Part D coverage (N = 662,243 enrollee-years).

Figure A.14: Monte Carlo Estimations: Visualizations of Parameter Bias

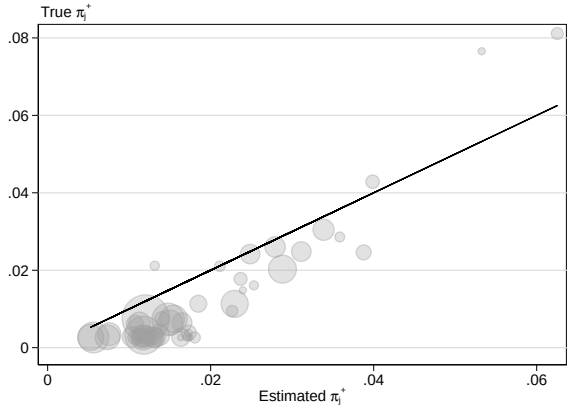
(a) Initial Share Addicted (a_{j0})



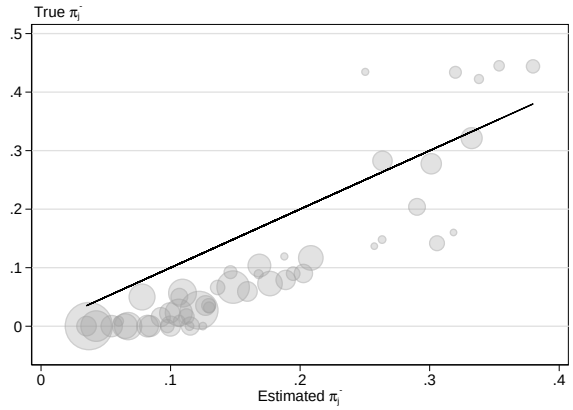
(b) Place-Specific Availability (γ_j)



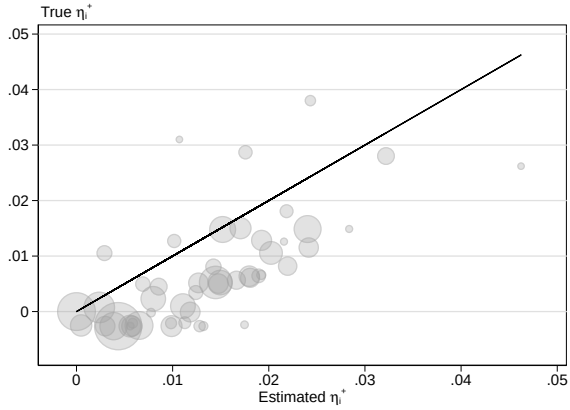
(c) Place-Based Transitions into Addiction (π_j^+)



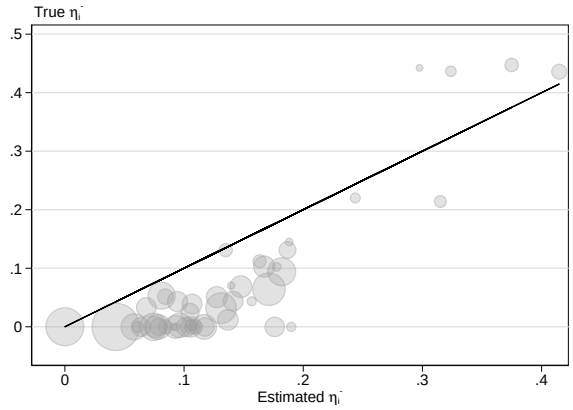
(d) Place-Based Transitions out of Addiction (π_j^-)



(e) Person-Based Transitions into Addiction (η_i^+)



(f) Person-Based Transitions out of Addiction (η_i^-)



Notes: Figures show scatterplots of estimators calibrated by averaging one hundred Monte Carlo simulations versus the true parameters. All estimates vary at the state level. Panel A presents the initial share addicted, Panel B presents the time and place-specific availability, Panels C and D present place-specific factors affecting addiction transitions, and Panels E and F present person-specific factors affecting addiction transitions. The black line in each figure shows the 45 degree line. The size of each observation is proportional to the number of mover-year observations in the observation sample where a mover was observed in the state. We define γ_j by averaging γ_{jt} over states and re-normalizing τ_t to zero, because these two sets of parameters are jointly defined by the normalization of τ_t, as discussed in Section Appendix D.

Table A.1: Opioid Prescribing Measures and Adverse Opioid Outcomes

(a) Opioid Prescribing Measures

	All Enrollees	Enrollees with Poisoning Event in Year $t+1$	Enrollees with Opioid Use Disorder in Year $t+1$
High MED (Baseline)	0.044	0.335	0.282
Many Prescribers	0.061	0.379	0.338
Overlapping Prescribers	0.154	0.676	0.567
Any Opioids	0.420	0.905	0.808
Chronic Opioid Use	0.210	0.733	0.610
Share of Non-Movers	1.000	0.002	0.011
N	4,678,601	7,195	49,399

(b) Predictive Power: Positive Likelihood Ratios

	Poisoning Event in Year $t+1$	Opioid Use Disorder in Year $t+1$
High MED (Baseline)	7.67	7.12
Many Prescribers	5.76	5.58
Overlapping Prescribers	4.37	3.88
Any Opioids	2.12	1.95
Chronic Opioid Use	3.54	3.12

Notes: Table shows the relationship between adverse opioid outcomes (opioid poisonings and diagnoses of opioid use disorders) with our proxies for risky opioid use and opioid prescribing measures. We limit the sample to non-mover enrollee years covered by traditional Medicare whom we also observe in the following year; this allow us to observe their adverse outcomes in year $t+1$. Panel A shows the rate of these measures among all enrollee-years in the first column; the second and third columns restrict to enrollees with adverse opioid outcomes in the year following. Panel B shows the positive likelihood ratios defined for each opioid measure and adverse outcome. The positive likelihood ratio is defined as the ratio of true to false positives when the risky opioid use or prescribing measure is used as a diagnostic for adverse opioid outcomes in the following year. These various risky opioid use measures are discussed in Appendix A.

Table A.2: Measures of Hazardous Opioid Prescriptions

	High MED (Baseline)	Many Prescribers	Overlapping Prescriptions	Any Opioid	Chronic Opioid Use
High MED (Baseline)	1.000				
Many Prescribers	0.714	1.000			
Overlapping Prescriptions	0.729	0.797	1.000		
Any Opioids	0.486	0.690	0.929	1.000	
Chronic Opioid Use	0.533	0.659	0.948	0.976	1.000

Notes: Table presents pairwise rank correlations between the baseline measure (high morphine equivalent doses or "High MED"), Many Prescribers, Overlapping Prescribers, Any Opioids, and Chronic Opioid Use variables for 2006-2015, averaged across years at the state level and assigning equal weight to each state. These various risky opioid use measures are discussed in Appendix A.

Table A.3: State-Year Level Risky Opioid Use Measure Summary Statistics

	25th Percentile	Median	75th Percentile	Interquartile Range
High MED (Baseline)	0.028	0.041	0.054	0.026
Many Prescribers	0.044	0.058	0.073	0.030
Overlapping Prescriptions	0.114	0.146	0.175	0.061

Notes: Table presents the 25th percentile, median, and 75th percentile of the state-year averages of the indicated opioid prescription measure: our baseline measure (high morphine equivalent doses or "High MED"), Many Prescribers, and Overlapping Prescriptions. These various risky opioid use measures are discussed in Appendix A. Each state-year average is determined by averaging the opioid prescription measure outcome within a year across non-movers in the sample. The final column presents the interquartile range of each opioid prescription measure.

Table A.4: 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: Table identifies opioid conversion factors used in construction of our baseline measure of risky use. Adapted from Meara et al. (2016) supplementary material as well as an opioid dose calculator developed by Washington State Agency Medical Directors' Group available at http://www.agencymeddirectors.wa.gov/calculator/dosecalculator.html.

Table A.5: Event Study Coefficients – Various Geographies

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline Geography (State)			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.053)	(0.026)	(0.104)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.058)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel B: Commuting Zone			
Average of 1-5 years pre-move	-0.005	0.005	-0.010
	(0.037)	(0.019)	(0.071)
Average of 1-5 years post-move	0.355	0.093	0.629
	(0.042)	(0.023)	(0.093)
Enrollees	83,375	43,580	38,636
Enrollee-years	555,688	294,557	254,322
Panel C: County			
Average of 1-5 years pre-move	0.026	0.014	-0.003
	(0.036)	(0.015)	(0.065)
Average of 1-5 years post-move	0.313	0.087	0.579
	(0.042)	(0.022)	(0.099)
Enrollees	87,249	45,616	38,275
Enrollee-years	575,941	303,762	246,782

Notes: Table reports the average of coefficients and bootstrapped standard errors (in parentheses) one to five years pre-move and one to five years post-move, as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Estimates are shown for the baseline sample, naive enrollees, and prior users observed in the year before move for whom we are able to observe matched non-movers. Panel A reports estimates for our baseline result, Panel B reports estimates for moves by commuting zone, and Panel C reports estimates for moves by county. 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. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table A.6: Yearly Effects of Counterfactual Policies Targeting Risky Use

	Targeting Place-Based Addiction Channel	Targeting Place-Based Availability Channel	Targeting Both Place-Based Channels
2006	0.0%	35.7%	35.7%
	[0.0%, 0.0%]	[31.7%, 66.4%]	[31.7%, 66.4%]
2007	18.9%	35.0%	47.3%
	[16.4%, 37.2%]	[35.0%, 55.6%]	[47.1%, 70.1%]
2008	32.3%	24.9%	49.2%
	[27.9%, 50.9%]	[24.4%, 35.6%]	[47.5%, 65.8%]
2009	42.3%	21.4%	54.6%
	[35.3%, 58.4%]	[20.8%, 27.2%]	[51.2%, 67.8%]
2010	49.9%	19.7%	59.8%
	[40.8%, 63.2%]	[18.3%, 23.7%]	[53.7%, 70.4%]
2011	55.8%	19.0%	64.2%
	[45.0%, 67.6%]	[17.1%, 22.6%]	[56.2%, 73.5%]
2012	60.5%	19.7%	68.4%
	[48.3%, 70.8%]	[17.0%, 22.8%]	[58.8%, 76.1%]
2013	64.3%	22.5%	72.4%
	[50.8%, 73.2%]	[18.0%, 25.5%]	[61.9%, 78.9%]
2014	67.4%	26.3%	76.1%
	[53.0%, 75.0%]	[20.3%, 29.7%]	[64.5%, 81.3%]
2015	70.0%	30.4%	79.2%
	[53.9%, 76.8%]	[22.6%, 34.1%]	[66.5%, 83.5%]

Notes: Table reports the yearly percentage reduction in risky use rates driven by the effect of counterfactual policies that lower place-based addiction and place-based availability parameters as described in Section 6.3 and presented in Figure 10. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The yearly risky use rate is computed by taking a weighted average of simulated risky use over all states in a given year in our non-mover sample, weighting by the number of non-movers observed in each state. 95% confidence intervals are computed using 50 replications of the Bayesian bootstrap. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary.

Table A.7: Event Study Coefficients – Other Outcomes

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: High MED (Baseline)			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.053)	(0.026)	(0.104)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.058)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel B: Many Prescribers			
Average of 1-5 years pre-move	-0.003	0.031	-0.040
	(0.067)	(0.034)	(0.128)
Average of 1-5 years post-move	0.724	0.293	1.165
	(0.085)	(0.059)	(0.175)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel C: Overlapping Prescriptions			
Average of 1-5 years pre-move	-0.007	0.022	0.001
	(0.037)	(0.023)	(0.067)
Average of 1-5 years post-move	0.265	0.166	0.376
	(0.047)	(0.037)	(0.091)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807

Notes: Table reports the average of coefficients and their bootstrapped standard errors (in parentheses) between one to five years pre-move and one to five years post-move as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort and y_{ir} (when used to construct $\hat{T}_{cr}$) is defined as the indicated opioid use measure for individual i in relative year r. Estimates are shown for the sample of all movers, naive enrollees, and prior users observed in the year before move for whom we are able to observe matched non-movers. Each panel reports results for a different outcome: our baseline measure (high morphine equivalent doses or "High MED"), Many Prescribers, and Overlapping Prescriptions. These risky opioid use measures are discussed in Appendix A. 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. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table A.8: Event Study Coefficients – Alternative Specifications

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline Specification			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.053)	(0.026)	(0.104)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.058)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel B: No Individual Fixed Effects			
Average of 1-5 years pre-move	0.123	-0.001	0.216
	(0.062)	(0.026)	(0.120)
Average of 1-5 years post-move	0.519	0.152	0.921
	(0.050)	(0.033)	(0.123)
Enrollees	90,871	32,482	30,544
Enrollee-years	521,279	217,922	202,807
Panel C: No Relative Year Fixed Effects			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.054)	(0.026)	(0.105)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.059)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel D: No Covariates (X)			
Average of 1-5 years pre-move	0.014	0.001	0.037
	(0.053)	(0.025)	(0.104)
Average of 1-5 years post-move	0.410	0.145	0.686
	(0.058)	(0.032)	(0.112)
Enrollees	63,108	32,520	30,588
Enrollee-years	421,409	218,248	203,161

Notes: Table reports the average of coefficients and their bootstrapped standard errors (in parentheses) between one to five years pre-move and one to five years post-move as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Estimates are shown for the baseline sample, naive enrollees, and prior users observed in the year before move for whom we are able to observe matched non-movers. Each panel shows a single deviation from our baseline specification (baseline shown in Panel A). Panel B omits individual fixed effects (estimating the model in levels rather than in differences); Panel C omits the relative year fixed effects (ρ_r), and Panel D omits the covariates for five-year age bin, race and gender. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table A.9: Event Study Coefficients – Sample Restrictions

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline Sample			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.053)	(0.026)	(0.104)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.058)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel B: Part D All Years			
Average of 1-5 years pre-move	0.059	0.009	0.109
	(0.077)	(0.040)	(0.150)
Average of 1-5 years post-move	0.441	0.145	0.732
	(0.082)	(0.046)	(0.166)
Enrollees	20,834	11,045	9,746
Enrollee-years	167,896	89,191	78,371
Panel C: Alive All Years			
Average of 1-5 years pre-move	0.034	-0.002	0.083
	(0.055)	(0.028)	(0.109)
Average of 1-5 years post-move	0.409	0.146	0.682
	(0.062)	(0.032)	(0.126)
Enrollees	52,176	27,294	24,836
Enrollee-years	364,436	190,876	173,198

Notes: Table reports the average of coefficients and their bootstrapped standard errors (in parentheses) between one to five years pre-move and one to five years post-move as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Estimates are shown for the baseline sample, naive enrollees, and prior users observed in the year before move for whom we are able to observe matched non-movers. Each panel shows a single deviation from our baseline specification (baseline shown in Panel A). Panel B restricts to movers with 12 months of Part D coverage for each year they are observed in the sample, and Panel C restricts to all movers who did not die from 2006 to 2015. 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. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table A.10: Event Study Coefficients – Moves Up and Moves Down

	(1)	(2)	(3)
	All	Naive	Prior User
Panel A: Baseline (All Moves)			
Average of 1-5 years pre-move	0.017	-0.001	0.042
	(0.053)	(0.026)	(0.104)
Average of 1-5 years post-move	0.419	0.152	0.696
	(0.058)	(0.033)	(0.118)
Enrollees	63,065	32,482	30,544
Enrollee-years	421,067	217,922	202,807
Panel B: Moves Up			
Average of 1-5 years pre-move	-0.054	0.016	-0.145
	(0.099)	(0.047)	(0.186)
Average of 1-5 years post-move	0.408	0.128	0.735
	(0.145)	(0.071)	(0.292)
Enrollees	34,422	18,150	16,254
Enrollee-years	229,884	121,832	107,909
Panel C: Moves Down			
Average of 1-5 years pre-move	0.062	-0.014	0.145
	(0.130)	(0.054)	(0.253)
Average of 1-5 years post-move	0.492	0.098	0.899
	(0.174)	(0.056)	(0.305)
Enrollees	28,643	14,332	14,290
Enrollee-years	191,183	96,090	94,898

Notes: Table reports the average of coefficients and their bootstrapped standard errors (in parentheses) between one to five years pre-move and one to five years post-move as estimated from equation (3) of μ_r—the coefficient on year relative to move interacted with $\hat{\delta}_{cr}$—where the weights on the regression are given by the number of movers observed in each cohort. Each panel shows a single deviation from our baseline specification (baseline shown in Panel A). The table reports estimates for moves up (Panel B) and moves down (Panel C) based on the difference in the rates of risky opioid use between the origin and destination state in the year of the move. 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. Estimates are shown for the baseline sample, naive enrollees, and prior users observed in the year before move for whom we are able to observe matched non-movers. Standard errors are computed using 50 replications of the Bayesian bootstrap.

Table A.11: Estimation Sample Summary Statistics

Sample Used to Construct Moments	
Number of Movers	90,301
Number of Cohorts	12,528
Number of Enrollee-Years	451,124
Average Risky Use Rate	4.8%
Moments Targeted in Model Estimation (Years Relative to Move for Each Cohort)	
Number of Moments	81,892
Median Number of Enrollee-Years per Moment	2
Mean Number of Enrollee-Years per Moment	5.5
Minimum Number of Enrollee-Years per Moment	1
Maximum Number of Enrollee-Years per Moment	316

Notes: Table presents summary statistics on the empirical moments that are targeted in the estimation of the model as well as the sample used to construct those moments. The sample used to construct the moments is simply the sample of all movers presented in Table 1 column 1, excluding enrollee-years observed during the year of move. The empirical moments are average risky use rates constructed at the cohort – relative-year level, where cohorts are defined by a mover's origin, destination, and move year.

Table A.12: Monte Carlo Estimations: The Effect of Counterfactuals on Risky Use

Predicted reduction in rate of risky use from a one standard deviation reduction in...		All States *(True Value Below)*	Ten Highest Risky Use States in 2006 *(True Value Below)*
(1)	All Place-based Parameters (γ_j, π_j)	68.4% *(64.6%)*	64.6% *(61.9%)*
(2)	All Person-based Parameters (a_{i0}, η_i)	61.7% *(64.1%)*	60.8% *(60.5%)*
(3)	Availability (γ_j)	27.7% *(23.8%)*	26.4% *(22.1%)*
(4)	Place-based Addiction Transitions (π_j)	55.3% *(53.1%)*	50.8% *(50.6%)*
(5)	Person-based Addiction Transitions (η_i)	47.4% *(44.1%)*	45.7% *(42.6%)*
(6)	All Addiction Transitions (π_j, η_i)	76.7% *(74.4%)*	69.7% *(68.4%)*
(7)	Initial Addiction States (a_{i0})	28.0% *(35.7%)*	29.8% *(32.2%)*
(8)	All Addiction Parameters (π_j, η_i, a_{i0})	87.1% *(87.9%)*	80.5% *(80.4%)*

Notes: Table reports estimators of the effects of counterfactuals described in Section 6.3, constructed from averaging one hundred Monte Carlo simulations. The true effect—which is identical to the estimates from Table 3 because the same underlying parameters were used as the baseline for the Monte Carlo—is presented in parenthesis under each estimate for comparison. Estimates are presented in percent reduction in simulated overall risky use rates from 2006 to 2015 from a one standard deviation reduction in parameters. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation, except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The overall risky use rate is computed by taking the average across all simulated risky use outcomes for our non-mover sample, weighted by the number of non-movers in each state-year. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary.

Table A.13: Monte Carlo Estimations: Yearly Effects of Counterfactual Policies Targeting Risky Use

	Targeting Place-Based Addiction Channel *(True Value Below)*	Targeting Place-Based Availability Channel *(True Value Below)*	Targeting Both Place-Based Channels *(True Value Below)*
2006	0.0% *(0.0%)*	49.2% *(35.7%)*	49.7% *(35.7%)*
2007	25.9% *(18.9%)*	45.1% *(35.0%)*	59.3% *(47.3%)*
2008	40.0% *(32.3%)*	30.63% *(24.9%)*	58.2% *(49.2%)*
2009	48.7% *(42.3%)*	25.4% *(21.4%)*	61.9% *(54.6%)*
2010	54.6% *(49.9%)*	22.8% *(19.7%)*	65.0% *(59.8%)*
2011	58.6% *(55.8%)*	21.7% *(19.0%)*	67.7% *(64.2%)*
2012	61.6% *(60.5%)*	22.2% *(19.7%)*	70.2% *(68.4%)*
2013	63.8% *(64.3%)*	25.4% *(22.5%)*	73.1% *(72.4%)*
2014	65.16% *(67.4%)*	29.6% *(26.3%)*	76.1% *(76.1%)*
2015	67.0% *(70.0%)*	34.2% *(30.4%)*	78.7% *(79.2%)*

Notes: Table reports the estimators of the yearly effect of counterfactuals constructed from averaging one hundred Monte Carlo simulations. The true effect—which is identical to the estimates from Table A.6 because the same underlying parameters were used as the baseline for the Monte Carlo—is presented in parentheses under each estimate for comparison. Estimates are presented as the yearly percentage reduction in risky use rates driven by the effect of counterfactual policies that lower place-based addiction and place-based availability parameters as described in Section 6.3 and presented in Figure 10. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The yearly risky use rate is computed by taking a weighted average of simulated risky use over all states in a given year in our non-mover sample, weighting by the number of non-movers observed in each state. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary.

Table A.14: Robustness of Counterfactuals to Alternative Models

		(a)	(b)	(c)	(d)	(e)	(f)
Predicted reduction in rate of risky use from a one standard deviation reduction in...		Baseline Model	Allowing Addiction Transitions During Relative Year 0	No Selection ($s=0$)	Allowing for Risky Use by Non-Addicted Individuals	Alternative Global Addiction Share ($\bar{a}=0.15$)	Alternative Global Addiction Share ($\bar{a}=0.20$)
(1)	All Place-based Parameters (γ_j, π_j)	64.6%	62.1%	64.7%	71.7%	61.0%	58.5%
(2)	All Person-based Parameters (a_{i0}, η_i)	64.1%	61.2%	52.0%	62.9%	63.2%	64.0%
(3)	Availability (γ_j)	23.8%	22.2%	23.3%	26.7%	22.9%	21.5%
(4)	Place-based Addiction Transitions (π_j)	53.1%	50.6%	52.9%	60.7%	48.8%	46.5%
(5)	Person-based Addiction Transitions (η_i)	44.1%	49.4%	38.8%	46.1%	49.3%	50.3%
(6)	All Addiction Transitions (π_j, η_i)	74.4%	74.8%	65.5%	80.3%	76.3%	74.7%
(7)	Initial Addiction States (a_{i0})	35.7%	23.5%	23.3%	29.2%	27.1%	26.1%
(8)	All Addiction Parameters (π_j, η_i, a_{i0})	87.9%	82.7%	74.9%	89.4%	86.9%	85.0%

Notes: Table presents the effect of counterfactuals for our baseline specification as well as a set of alternative models. Estimates are presented in percent reduction in simulated overall risky use rates from 2006 to 2015 from a one standard deviation reduction in parameters. Each counterfactual policy is to reduce parameters in the relevant channel(s) by a standard deviation, except for the probabilities of transitioning out of addiction, which we increase by a standard deviation. The overall risky use rate is computed by taking a weighted average of simulated risky use over all state-years in our non-mover sample, weighting by the number of non-movers observed in each state-year. Throughout, we maintain our standard parameters bounds of [0,1] by taking the minimum (maximum) of the reduced (increased) parameter and the bound as necessary. Column (a) presents the results from our baseline specification. Column (b) examines the model which allows for addiction transitions to occur for an additional year in the year of the move. These transitions are assumed to occur according to the place-specific addiction transition parameters of the origin location. Column (c) examines the model where we restrict the selection parameter to be zero. Column (d) examines the alternative model where we allow for risky use by non-addicted individuals at a constant rate, which is an additional parameter that we estimate. Finally, columns (e) and (f) examine models which specify different global shares of addiction ($\bar{a}=0.15$ and $\bar{a}=0.20$). We discuss these specifications further in Appendix D.