JOHANNES KEPLER
UNIVERSITY LINZ

DEPARTMENT OF ECONOMICS
JOHANNES KEPLER UNIVERSITY OF LINZ

Supply-side drug policy, polydrug use, and the economic effects of withdrawal symptoms

by

Alexander AHAMMER
Analisa PACKHAM

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Johannes Kepler University of Linz
Department of Economics
Altenberger Strasse 69
A-4040 Linz - Auhof, Austria
www.econ.jku.at

alexander.ahammer@jku.at

*Supply-side drug policy, polydrug use, and the economic effects of withdrawal symptoms**

Alexander Ahammer
Johannes Kepler University Linz and IZA

Analisa Packham
Vanderbilt University, IZA, and NBER

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Abstract

Despite the fact that 30 percent of opioid overdoses also involve a benzodiazepine, there is little policy guidance on how to curb concurrent misuse and even less evidence on how changes to co-prescribing practices can affect patients' economic trajectories. In 2012, Austria restricted access to flunitrazepam, one of the most potent, and most heavily misused, benzodiazepines. We use linked individual-level data to identify opioid users and estimate the reform's impact on their health and labor market outcomes relative to a randomly selected comparison group of non-opioid users. Estimates indicate a 12.7 percent drop in employment, a 13.1 percent increase in unemployment insurance claims, and a 26.5 percent increase in overall healthcare expenditures. We provide suggestive evidence that these effects are due to incapacitating withdrawal symptoms, rather than substitution to other drugs, including heroin or alcohol.

JEL Classification: I38, I12, J18
Keywords: opioids, substance use disorder treatment, benzodiazepines

**Correspondence:* Alexander Ahammer, Johannes Kepler University Linz, Altenberger Straße 69, 4040 Linz, Austria, ph. +43-732-2468-7372, alexander.ahammer@jku.at. For helpful discussions and comments we thank David Bradford, Janet Currie, Matt Harris, Joe Sabia, and participants at the 13th Workshop on the Economics of Risky Behavior and the 2023 International Health Economics Association Meeting.

1. Introduction

The opioid crisis is still ongoing in the U.S.; more than 106,000 people died from drug-related overdose in 2021, a 16 percent increase from 2020 (NIDA, 2023). An often overlooked fact is that overdoses frequently involve combinations of opioids and other substances. In the first half of 2018, nearly 63 percent of opioid overdose deaths co-occurred with at least one non-opioid drug (Gladden, O'Donnell, Mattson, and Seth, 2019). Benzodiazepine misuse is particularly common among opioid users, because the substances have similar euphoric properties and potentiate each other. In 2018, about 33 percent of overdoses involved an opioid and a benzodiazepine. Yet relatively little is known about the effectiveness of policies that target benzodiazepine misuse in opioid users. The ongoing concurrent usage of opioids and benzodiazepines highlights the need for new evidence to encourage safer prescribing.

In this paper we analyze a 2012 reform in Austria that restricted access to flunitrazepam, one of the most potent benzodiazepines available prescribed almost exclusively to opioid users. Due to concerns about overdose risk and black market diversion, the government sought to clamp down on prescriptions by introducing a triplicate prescription system, strict patient monitoring including regular urine testing, and daily dispensing under supervision in pharmacies. Crucially, only flunitrazepam was targeted, and prescription regulations for other benzodiazepines were not affected. After 2012, the share of opioid users prescribed flunitrazepam fell from 21 percent to 5.4 percent.

The welfare effects of restricting access to flunitrazepam are *a priori* unclear. For example, if this drop in concurrent prescribing leads opioid users to substitute to heroin or other potent drugs, we would expect to observe adverse health and labor market effects. If the policy leads opioid users to switch to less potent drugs or reducing concurrent use altogether, both positive and negative effects are possible, depending on how well opioid users can cope with replacing a very potent drug with less potent substitutes.

To estimate the effects of the 2012 reform on health outcomes and employment, we first identify

a set of regular opioid users in our linked administrative registers based on histories of substance abuse treatment and opioid-related health events, such as overdoses. We then use a difference-in-differences approach to compare effects between opioid users and a 10 percent random sample of non-opioid users before and after the reform. While studies on the opioid crisis typically focus on county- or state-level estimates, our data allow us to track individual-level changes in economic trajectories and healthcare utilization patterns of those affected by the reform. In doing so, we gain a better understanding of the comprehensive effects of such policies on opioid users themselves.

Difference-in-differences estimates indicate that the sudden and unexpected access restriction on flunitrazepam leads to a large drop in prescribed use for opioid users, with no effects on prescription usage for non-users. As a result of the reform, we find a 27 percent increase in health expenditures for opioid users. This is driven by increases in physician visits, indicating that restricting flunitrazepam impacts health and well-being. Additionally, we find that restricting access to flunitrazepam has adverse effects on mental health; we show that antidepressant prescription take-up increases as a result of the reform. The reform also affects labor market outcomes. We show that restricting use of flunitrazepam leads to a drop in labor force participation by 12.7 percent.

This inability to work might create reliance on unemployment benefits; indeed, we show that the ban on flunitrazepam leads to a large and persistent increase in unemployment insurance (UI) claims for opioid users relative to non-users. For workers that remain employed, we find a 6.8 percentage point reduction in daily wages. Estimates are similar when comparing effects by gender, age, educational attainment, and citizenship status. Also, we note that our estimates are not sensitive to the addition of covariates, using a comparison group of non-opioid drug users, or not using a control group at all. Overall, our findings indicate significant health and labor market penalties due to the increased restrictions in prescription drugs commonly used by those in opioid treatment, both directly (for users) and indirectly (for taxpayers and employers).

How can these adverse effects be explained? Importantly, although flunitrazepam prescriptions have decreased significantly, we find that the reform led to an immediate uptick in other, less potent benzodiazepines. Benzodiazepines create significant physical dependency, and reducing doses can

pose risks of withdrawal and seizures, implying that switching patients to less potent prescription drugs may cause health-related disruptions in employment. In contrast, there is little evidence that opioid users substitute to other drugs or increase opioid intake. Arrest numbers indicate that the black market for benzodiazepines practically disappeared after the reform, and find no changes in opioid-related hospitalizations, prescription opioid take-up, or alcohol and other drug use.

Our findings contribute to a larger literature on controlled substances and how access prescription drugs can affect career trajectories and economic outcomes. Biasi, Dahl, and Moser (2021) document that the availability of lithium treatment for bipolar disorder reduces the risk of declining into the bottom earnings decile by 13 percent and lowers the risk of zero earnings by 33 percent. Nevertheless, other studies have shown that regulating prescription drugs can be problematic in the presence of substitutes. Alpert, Powell, and Pacula (2018) and Evans, Lieber, and Power (2019) show that when states introduced an abuse-deterrent version of *OxyContin* in 2010, heroin deaths increased by over 200 percent, replacing opioid-related deaths with heroin deaths. We note that in our setting, we find no evidence of increases in heroin deaths or other black market sales for benzodiazepines after the flunitrazepam access restriction.

Findings from other recent work suggest that restricting one prescription drug in a class leads to substitution effects from prescribers. Gupta, Nguyen, Freeman, and Simon (2023) finds that tightening prescribing restrictions on one opioid leads to decreases in its use coupled with increases in prescriptions of close competitors, with no statistically detectable short-run reduction in total opioid prescriptions. We document substitution effects to other benzodiazepines, although these effects are not one-to-one. We note that in an already highly regulated market, such as the substance treatment market in Austria, where users are regularly checked in on, reducing access to prescription drugs reduces provider options for treatment.

We build on this literature in two main ways. First, using linked administrative data, we provide new evidence on the effects of changes in addictive prescription drug access for a targeted group of individuals—those currently or previously enrolled in opioid treatment. Austria provides an ideal setting to study poly drug use in opioid users, because: (i) it has one of the highest per-capita opioid

use rates worldwide, especially compared to other (non-U.S.) OECD countries; (ii) the country has universal healthcare, such that cost to treatment and other healthcare services is not an impediment to take-up, and; (iii) there is excess supply of opioid substitution treatment with practically no waiting lines to visit a provider and substitution treatment take-up is high among opioid users (Ahammer and Halla, 2022), which allows us to observe a substantial share of regular opioid users in our data.

Second, we measure the impacts on changes in treatment in terms of labor force participation and participation in safety net programs. These estimates provide new insight on the opportunity costs of restricting addictive prescription drugs in the presence of substitutes. In other words, our conclusions shed new light on the importance of finding solutions for effective opioid treatment.

Our findings also contribute to a growing literature on policies aimed at curbing the opioid epidemic. For example, recent studies have shown that state-level legal restrictions, including prescription limits, patient ID laws, prescription drug monitoring programs (PDMPs), doctor shopping restrictions, pain clinic regulations, and naloxone laws reduce opioid use (Bao, Pan, Taylor, Radakrishnan, Luo, Pincus, and Schackman, 2016; Doleac and Mukherjee, 2022; Mallatt, 2017; Meara, Horwitz, Powell, McClelland, Zhou, O'Malley, and Morden, 2016). Recent work has documented that PDMPs can decrease the number of Oxycodone shipments, opioid abuse among young adults, and misuse for Medicare Part D patients, but are most effective when doctors are required to consult them (Buchmueller and Carey, 2018; Dave, Grecu, and Saffer, 2017; Mallatt, 2017). Moreover, physician training can reduce opioid prescribing (Schnell and Currie, 2017), suggesting that certain supply-side policies may be an effective way to address opioid misuse. However, evidence on demand-side interventions are less promising. Doleac and Mukherjee (2022) and Packham (2023) show that naloxone access and openings of syringe exchange programs, respectively, can *increase* opioid-related mortality.

Other relevant work focuses on how changing treatment access, either via physical facilities or financial barriers, affects drug-related hospitalizations and mortality. For example, it is well-documented that access to substance abuse treatment facilities play an important role in reducing

opioid misuse (Corredor-Waldron and Currie, 2022; Swensen, 2015). Moreover, recent work leveraging variation from ACA expansions shows that insurance coverage for SUD treatment is negatively related to treatment take-up, including OST prescriptions, with larger effects among publicly insured individuals (Grooms and Ortega, 2019; Hamersma and Maclean, 2018; Maclean and Saloner, 2019).[1] These conclusions are in line with findings that allowing physicians to dispense opioid treatment medication increases MAT take-up (Barrette, Dafny, and Shen, 2023).

However, there is much less empirical evidence, including very few papers in economics, evaluating medication-assisted treatment programs (MATs) and what drugs provide the most effective treatment, especially in an outpatient setting. One such paper finds that increasing access to MATs through Medicaid expansion is not successful at reducing fatal opioid overdoses (Maclean and Saloner, 2019). While this study provides some evidence that access for those already insured is not enough to reduce severe cases of opioid misuse, we note that in U.S. settings, providers also may lack the ability to accept new patients, leading to an inability to observe large effects (Saloner, 2017). Indeed, in a technical brief comparing research on MATs from 1996–2016, the U.S. Department of Health and Human Services notes that more research, *"is needed to clarify optimal MAT models of care and to understand effective strategies"* (Chou, Korthuis, Weimer, Bougatsos, Blazina, Zakher, Grusing, Devine, and McCarty, 2016). On the other hand, Bullinger, Wang, and Feder (2022) show that after MAT opioid treatment programs opened in Indiana in 2018, methadone dispensing increased, and ED visits related to opioid overdoses decreased. These mixed findings imply that there is scope for more work to be done regarding outpatient treatment programs. We build on this literature by offering new insights on the effectiveness of monitored medication-based treatment programs and the consequences of changing the availability of prescription drugs used by those in treatment. In doing so, we address a broader question of how alterations in MAT due to prescription drug regulation can affect individuals when there exists excess supply, rather than demand, for treatment services.

[1]Similarly, Maclean, Tello-Trillo, and Webber (2023) shows that large disenrollments of public health insurance recipients reduces hospitalizations for SUD billed by Medicaid by 15 percent, implying that financial costs pose a meaningful barrier for receiving inpatient care.

We also note that our findings speak to the risks of simultaneous prescribing of benzodiazepines and opioids for long-term use. While there is a great deal of work on how state-level policies and policies aimed at prescribers can do to reduce opioid-related mortality, there exists much less work on how policies can effectively target individuals who choose to enroll in substance use treatment. Moreover, as death tolls increase over time due to the misuse of opiates and benzodiazepines, our findings provide key evidence on how these drugs can be safely prescribed and can serve as a benchmark on how changes in treatment due to drug regulation can affect the health and well-being of those already enrolled in MAT programs.

2. The setting

2.1. Social security in Austria

Austria has a Bismarckian social security system with universal public healthcare financed through social security contributions. Since enrollment to the system is automatic, virtually all Austrian residents are covered by health insurance, regardless of employment status (Ahammer, Wiesinger, and Zocher, 2021). Healthcare provision is two-tiered, with most services being provided publicly and parallel private markets existing for outpatient services and specialized hospitals, such as fertility clinics. Private suppliers usually offer the same types of services as public providers, so they are used almost exclusively to avoid waiting lines. Hospitals are reimbursed based on a diagnosis-related group system, and outpatient providers are reimbursed on a fee-for-service basis.

There is no formal gatekeeping system, but general practitioners (GPs) are typically the first point of access to the healthcare system. Cost-sharing is limited to minor co-payments for drug prescriptions (around €5 per script), which are waived for unemployed and low-income individuals, and overnight hospital stays (€9,97 per day in 2018, for a maximum of 28 days). Top-up private insurance is available but only covers single rooms in hospitals, free physician choice in hospitals, and expenses for private physicians. Apart from healthcare, the Austrian social security system provides universal access to accident, pension, disability, and unemployment benefits.

2.2. The Austrian labor market

The Austrian labor market is characterized by strong industrial relations with centrally bargained wages and working conditions (Böheim, 2017). At the same time, the labor market is highly flexible, with particularly weak job protection and high turnover (OECD, 2020).[2] Employment contracts can generally be terminated without a given reason, but unilateral terminations require a notice period be observed. Unemployment rates have historically remained low, ranging, for example, from 4.72 in 1998 to 5.21 in 2018 (OECD, 2023). Female labor force participation is particularly low, and almost 50 percent of female workers work part-time.

2.3. Opioids and substance use disorder treatment in Austria

Austria has a large population of opioid users, constituting a large illicit drug problem. Opioids rank second in the number of drug seizures between 2016 and 2020, and drug-induced deaths are nearly double that of the European average (Ahammer and Halla, 2022; EMCDDA, 2014). Among all OECD countries, Austria ranks fifth in per capita opioid prescriptions (Figure 1). And while the share of opioid-dependent individuals has slightly decreased over time, the share enrolled in drug-related treatment has increased (Weigl, Anzenberger, Busch, Grabenhofer-Eggerth, Schmutterer, Horvath, and Strizek, 2017). In Austria, a majority of opioid users end up in drug-related treatment at some point, often also managing psychiatric comorbidities (Ahammer and Halla, 2022; Tanios and Busch, 2018).[3]

The main source of drug-related treatment in Austria is the opioid substitution therapy (OST) program. Austria's OST program is a medication-based harm reduction program organized and run by public health insurance. Drug coordinators are responsible for treatment at a regional level. Treatment is primarily delivered in the outpatient sector via primary care clinics. To regulate the

[2]For example, in 2018, the last year of our data, job turnover for female workers and male workers was 9.6 percent and 9.3 percent, respectively. In comparison, the European Union averages were 8.6 for female workers and 8.1 for male workers. The OECD employment protection legislation indicator is 1.7 for Austria, which is the fifth-lowest value among OECD countries. The United States rank last with an indicator of 1.3.

[3]Although it is difficult to obtain precise estimates for the proportion of regular opioid users in treatment, official estimates range between 50–70 percent at any given time, with larger estimates for those enrolling in treatment at some point (Weigl, Anzenberger, Busch, Grabenhofer-Eggerth, Schmutterer, Horvath, and Strizek, 2017).

quantity of opioids, suppliers adhere to a "triplicate" prescription system, meaning that prescriptions must be seen and approved by a physician, public health officer (which are administrators employed by the county), and a pharmacist.

This highly regulated market has low barriers to entry; users experience a large supply of providers, no waiting lines, and therapy is fully funded by statutory health insurance regardless of employment status. Every patient who produces a positive urine test for opioids will, in principle, be admitted. Patients receive medication via supervised daily dispensing at pharmacies. Treatment includes stable doses of slow-acting opioids, in particular methadone, buprenorphine, and extended-release morphine.[4] However, patients are often also be prescribed other drugs, like benzodiazepines, in conjunction with treatment for alcohol withdrawal or mental health symptoms. One key component of OST in Austria is strict monitoring; participants must undergo frequent urine tests and visual checks for injection marks.

Despite the fact that treatment is aimed at reducing opioid dependence and misuse, OST is intended to be a long-term program. OST focuses on maintenance treatment over abstinence due to the chronic nature of opioid dependence, with relapse rates nearing 90 percent (Ahammer and Halla, 2022). Nevertheless, retention in the Austrian system is relatively high, with two-year retention rates averaging approximately 61 percent (Busch, Klein, Uhl, Haltmayer, Cabanis, Westenberg, Vogel, and Krausz, 2021). This long-term nature of the program is reflected in the stated objectives of the program, which aims to *"reintegrate addicted persons into social life or prevent their marginalisation and to enable [them] to gain control over their lives"* (International Society of Substance Use Professionals, 2024). Therefore, at least from the perspective of the provider, effective and continued medication-assisted treatment is one such avenue for individuals to exist as a productive member of society.

[4]The morphine preparations used currently in substitution therapy are also the only ones that can be dissolved and injected to increase their euphoric effects. Methadone is only dispensed as a fluid diluted with sugary syrup, which makes it impossible to inject. Buprenorphine is a partial opioid antagonist that does not elicit euphoria and is therefore more unlikely to be misused.

2.4. The 2012 restriction of flunitrazepam

Benzodiazepines are highly prevalent among opioid users in Austria. In low doses and if taken for a short period of time, these drugs help to treat anxiety, insomnia, and muscle pain. In surveys, opioid users reveal a distinct preference for flunitrazepam over other benzodiazepines, likely due to its euphoric effects and its ability to alleviate opioid withdrawal symptoms (Simmons and Cupp, 1998; Woods and Winger, 1997). As part of the Austrian OST program, doctors often prescribe benzodiazepines alongside prescriptions like methadone or morphine for mental health treatment. As identified in our linked data, prior to 2012, 45 percent of opioid users were prescribed a benzodiazepine, with over 21 percent prescribed the drug flunitrazepam. Flunitrazepam (marketed under the brand names *Rohypnol* and *Somnubene*, in the U.S. sometimes colloquially referred to as "roofies") is the most potent market-available benzodiazepine, with potency approximately ten times that of alprazolam (brand name *Xanax*) (Simmons and Cupp, 1998), and is rarely prescribed to individuals outside of OST.[5] As with any benzodiazepine, there is a high risk of psychological and physiological dependence; similar to opiates, patients report having trouble quitting due to powerful withdrawal symptoms (Soyka, 2017).

In 2012, the Austrian government severely restricted access of flunitrazepam, due to a large black market presence and concerns about widespread misuse in the opioid user population.[6] Although the prescription drug was not banned outright, the government increased the transaction cost of prescribing and receiving flunitrazepam, so as to effectively deter any prescriptions.

Since December 2012, every single flunitrazepam prescription must be authorized and countersigned by a public health officer (PHO) and capped to a month's supply. PHOs may alter or reject prescriptions and mandate that other medication be prescribed, if necessary. Doses of flunitrazepam must be dispensed daily, under supervision, in a pharmacy, and must be documented in a nationwide database accessed by PHOs (similar to PDMP laws in the U.S.).

[5]The FDA has not approved flunitrazepam; it is considered an illegal drug in the United States.

[6]For example, prior to the policy change, flunitrazepam was the most common subject of forged prescription attempts. According to Department of the Interior reports, flunitrazepam comprised 5 percent of all illegally sold or stolen pills seized in Upper Austria in 2011.

In practice, the 2012 initiative aimed at curbing the use of flunitrazepam simultaneously did two things: (1) it effectively prohibited anyone who wanted illegal access to flunitrazepam from finding it on the black market, and (2) it severely restricted access to flunitrazepam for those legally obtaining the prescription drug in OST. In Figure A1 we present the number of flunitrazepam prescriptions over time. Before the 2012 reform, the likelihood of getting a flunitrazepam prescription is between 4–5 percent in any given quarter for identified opioid users, while after the reform this likelihood drops to 1–2 percent.[7] Non-opioid users rarely take up this drug in any period.

3. Data

To estimate the effects of the 2012 prescription drug reform on prescribing, healthcare utilization, and labor market outcomes, we use administrative data from the Upper Austrian Health Insurance Fund (UAHIF) database spanning 1998–2018. Upper Austria is a state in northern Austria, containing approximately 1.5 million, or 17 percent, of the total inhabitants of Austria. The database contains all inpatient and outpatient claims for insured individuals, including hospitalizations, physician visits, drug prescriptions, healthcare expenditures, and sick leaves.

Data from the UAHIF contain detailed individual-level information on inpatient and outpatient visits, including information on total physician visits and fees paid, and occurrence of acute health events. Hospital data do not include information on emergency department visits, but we do observe overnight hospital stays. Prescription data contain information on outpatient prescriptions and include the names and doses of every medication. Prescriptions are classified according to the Anatomical Therapeutic Chemical (ATC) system. There are no prescription refills in Austria, which allows us to capture all possible prescriptions during our sample period. Diagnoses are recorded for inpatient stays and sick leaves using ICD-10 codes.

To track labor market outcomes, we link the UAHIF health records to individual-level social security records from the Austrian Social Security Database (ASSD). The ASSD contain administrative data on labor market outcomes, including information on employment spells, wages,

[7] Approximately 13 percent of opioid users ever receive flunitrazepam.

occupation, as well as employee characteristics, such as age, gender, migrant status, and residence location. These data also contain information on other social security programs, including spells of sick leave or unemployment insurance (UI).

For our main sample, we identify opioid users using proxies for opioid misuse at any point between 1998–2018. We classify an individual as an opioid user if they meet one of the two criteria: (i) entered substance use treatment during our sample period, and/or (ii) have a recorded diagnosis of an opioid-related health event (e.g., an overdose requiring hospitalization). We note two important additional points regarding our sample restrictions. First, we also include individuals who enter substitution treatment or had an opioid-related health event *after* the reform, because they were most likely opioid users for some time before actually entering treatment. The advantage of this approach is that we hold the sample composition constant over the observation period, and we can capture effects on patients not yet in treatment, although we show below that our results hold if we use only pre-reform data to identify opioid users. Second, the reason why we focus on all opioid users and not just those who had been prescribed flunitrazepam before the reform is because there was plenty of evidence that pills had been diverted to the black market in large quantities, so even opioid users not currently prescribed flunitrazepam but acquiring their supply through other channels are potentially treated by the reform.

Our control group is a random 10 percent draw of non-opioid users from the general population. As shown in Figure A1, even before the reform, we see virtually no flunitrazepam prescriptions among non-opioid users, so their healthcare utilization and labor market trajectories are unlikely to be affected by the 2012 reform. Our final sample is an unbalanced panel containing 3,829 opioid users and 999,105 control group individuals observed for a maximum of 10 quarters before the reform to 10 quarters after the reform.

We provide the summary statistics in Table 1, displaying pre-reform quarterly means. We note that opioid users are younger and more likely to be male, with an average age of 32. In terms of education and labor market outcomes, these individuals are negatively selected; in particular, opioid users are much less likely to have a college degree compared to the general population (3

versus 13 percent), have higher healthcare expenditures, and have worse labor market attachment. While nearly one-fifth of opioid users are prescribed benzodiazepines in any given period, take-up for the general population for these drugs is nearly zero.

4. Design

Our empirical strategy is a difference-in-differences approach, comparing opioid users to a sample of non-opioid users before and after the restriction of flunitrazepam in December 2012. We estimate the following models:

$$y_{it} = \sum_{j=-10, j\neq -1}^{10} \beta_j \left[\mathbb{1}\{2012\text{q4} - t = j\} \times \text{opioid user}_i \right] + \tau_t + \alpha_{it} + \theta_i + \varepsilon_{it}, \tag{1}$$

where y_{it} represents the main outcome variables of interest for individual i in calendar quarter t, such as prescription benzodiazepine use, hospitalizations, and labor force participation, opioid user$_i$ is a binary indicator variable for whether an individual was enrolled in substance use disorder treatment or experienced any opioid-related health events between 1998–2018, and $\mathbb{1}\{2012\text{q4} - t = j\}$, $j = -10, , \ldots, -2, 0, \ldots, 10$, is a series of relative time indicators with the last pre-reform quarter $j = -1$ being the omitted reference period. Our main coefficients of interest are the β_js, which measure changes in healthcare utilization and labor market trajectories in quarter j relative to $j = -1$ between opioid users and the comparison group of non-opioid users. We also add a set of relative time fixed effects τ_t and flexible year-of-age fixed effects α_{it}, which account for potential cyclicality and life-cycle effects in prescriptions or labor market conditions. Finally, we control for individual fixed effects, θ_i, to account for unobserved time-invariant differences in health status and labor market attachment across individuals.

The identification assumption underlying this model is that trends in outcomes for opioid users would have tracked non-opioid users similarly after the flunitrazepam restriction implementation, absent the policy change. We note that opioid users generally are negatively selected in terms of socioeconomic status, education outcomes, and health status. However, we show that the trends in

health and labor market outcomes for these individuals, although worse on levels, follow a similar pattern prior to the 2012 drug reform, providing us reassurance that we are measuring causal effects of the reform. Additionally, we show estimates when using an alternative comparison group drawn from non-opioid drug users and when using no control group at all (i.e., simply comparing pre- and post-reform trends for opioid users).

An important design choice is that we sample all opioid users, even if they enter treatment or have an opioid-related health event after the reform, to construct our treatment group. We do this to capture effects on individuals who are likely to use opioids but have not yet entered treatment. A potential concern is that treatment group assignment is thus a function of the treatment itself (in particular if the reform changed the selection into opioid use). We address this in several ways: First, we show in Figure A2 that the unconditional probability of being an opioid user in the entire population aged 15–65 is relatively constant at about 0.1 percent and did not change discontinuously after the reform. Second, we provide evidence that the reform did not differentially affect opioid-related health events and prescription opioid take-up between the treatment and the control group. Third, and most importantly, we also estimate effects using only pre-reform data to identify opioid users.

5. Main results

5.1. The 2012 reform reduced flunitrazepam prescriptions

In this section, we address to what extent the restriction of flunitrazepam affected prescription take-up among opioid users, relative to other individuals. We first show descriptive evidence that the prescribing reform differentially affected flunitrazepam take-up for opioid users, relative to non-users, in Figure A1. In any given quarter prior to 2012, opioid users had a 4 percent likelihood of filling a flunitrazepam prescription. After the reform, this percentage is cut in half.

In Figure 2 we present our event-study estimates from Equation (1) for flunitrazepam prescriptions. Notably, after the 2012 restriction on flunitrazepam, we observe a sharp drop in prescription

use for opioid users, relative to the general population. Estimates indicate a 46 percent drop in take-up for this benzodiazepine, indicating that the policy change dramatically affected prescribing patterns for this group. As shown in Figure A3, effects are similar across age, gender, education, and citizenship.[8]

5.2. Some patients switch to other benzodiazepines

Next, we investigate whether this drop in flunitrazepam prescription take-up is accompanied by an increase in other types of benzodiazepines. To do so, we separately estimate effects on short-acting and long-acting benzodiazepines. Short-acting benzodiazepines are typically more potent; for comparison, flunitrazepam is considered a fast onset and short-acting benzodiazepine. Withdrawal typically begins 1–2 days after the last dose, and continues for 2–4 weeks or longer.[9] Indeed, as shown in Figure 3, we find a 28 percent increase in short-acting prescription benzodiazepines. Estimates indicate small positive changes in take-up for long-acting benzodiazepines. Together, these results suggesting that some, but not all, opioid users switch to new prescriptions, while many stop taking benzodiazepines altogether.

We note that symptoms of benzodiazepine withdrawal include anxiety, insomnia, restlessness, agitation, seizures, poor concentration, and muscle tension (Pétursson, 1994). The safest way to manage withdrawal is to gradually decrease the dosage, as sudden and full withdrawal can lead to more severe symptoms or even death (WHO, 2009). If patients are using benzodiazepines for an anxiety or psychological disorder, it is expected that stopping use of the drug will create a recurrence of these psychological symptoms. Therefore, there is some scope to believe that these substitution patterns could lead to worse health and labor market outcomes for individuals previously prescribed flunitrazepam. In the next two sections, we explore this relationship directly.

[8]In particular, estimates across each group are statistically similar in both pre- and post-periods, with slightly larger magnitudes for individuals below age 45 and men. Because of these similarities, in subsequent analyses, we focus on the full sample.

[9]Other short-acting benzodiazepines in our dataset include triazolam, nitrazepam, oxazepam. Long-acting benzodiazepines include bromazepam, diazepam, and clonazepam. Withdrawal typically begins 2–7 days after the last dose, and continues for 2–8 weeks.

5.3. Health expenditures increase

Next, we ask whether the reform affects opioid user welfare, and as a first welfare proxy we consider healthcare expenditures. We argue that an increase in health expenditures likely reflects a deterioration in health status. This is indeed what we find. In Figure 4, we show effects on total health expenditures. Estimates indicate a 26.5 percent (18 percentage point) increase in relative spending for opioid users after the flunitrazepam regulation went into effect. In Figure A4 we explore what is driving this increase. We show that doctor's visits and prescription drug costs increase, consistent with the idea that opioid users are more likely to experience adverse health events when a potent benzodiazepine is no longer available.

We note that changes in prescription drug access for opioid users may also have direct and indirect effects on mental health. Figure A5 shows effects on antidepressants. We find a slight uptick in antidepressant take-up after the reform, starting one year afterwards. Estimates off a small baseline indicate a 3.8 percent increase in antidepressant prescriptions for opioid users in the ten quarters following the restriction of flunitrazepam. These findings provide suggestive evidence that regulating flunitrazepam leads to not only worse physical health status, but also worse mental health status for opioid users.

5.4. Labor market attachment decreases

As a second proxy for welfare we also analyze labor outcomes. We do so given that opioid users have a lower attachment to the labor market and may face additional hurdles to obtaining employment, especially so if they are experiencing prescription drug withdrawal symptoms. Figure 5 displays estimates for employment and wages. The top panel presents an event study figure for the probability of employment before and after the reform. We find that restricting flunitrazepam prescriptions leads to a 12.7 percent drop in employment for opioid users.

We then ask whether this drop in employment is mirrored by an increase in the take-up of social services. In the middle panel of Figure 5, we show estimates for the probability that an opioid user claims unemployment insurance (UI) benefits before and after the 2012 restriction on flunitrazepam,

relative to the general population. We find that additional restrictions on benzodiazepines lead to a large increase in UI claims for this group. Estimates indicate that the reform increased UI take-up by 4 percentage points, or over 13 percent, corresponding to nearly an additional 300 claims as a result.

Lastly, we investigate whether, conditional on employment, opioid users experience career effects in terms of lost wages. The bottom panel presents estimates for daily log wages. Estimates indicate that opioid users in treatment experience a 1.3 percent drop in wages as a result of the reform, driven by a decrease beginning 2 years after the change in flunitrazepam access. Altogether, these results suggest that the regulation of a prescription drug that was primarily used by opioid users had negative effects on labor force attachment coupled with large increases in government-provided funds for non-employment.

6. Testing the sensitivity of the estimates

In this section, we test whether our estimates are sensitive to using different sets of covariates and note whether our conclusions change when estimating specifications using alternative definitions for both the treatment and the control group. In Table A1 we first show robustness of results when adding individual and age fixed effects separately. Prescription and health estimates are statistically similar at the 1 percent level across columns. Looking at labor market outcomes, estimate are larger in magnitude when including individual and age fixed effects; this likely accounts for the fact that older individuals are more likely to be employed and have higher wages.

In Table A2 we test how our results change if we use a different control group specification. Column (1) includes estimates for our main specification, comparing opioid users to a 10 percent sample of non-users. In column (2), we presents interrupted time series estimates for our treatment group, compared to no control group. Estimates largely mirror our main findings; we observe a large drop in flunitrazepam prescriptions and an increase in short-acting benzodiazepine prescriptions for opioid users. Moreover, labor market outcomes and wages fall for opioid users and UI take-up increases. This suggests that our results are primarily driven by changes in the treatment group,

while trends in outcomes for the comparison group are mostly flat after partialling out age and individual fixed effects.

Column (3) displays estimates comparing our main treatment group to individuals with identified drug-related health problems due to substances other than opioids (mostly these are patients treated for alcohol or cannabis dependence). This alternative control group is perhaps more similar in terms of observed and unobserved characteristics to our treatment group. We again estimate a large drop in flunitrazepam prescriptions, an increase in short-acting benzodiazepine prescription take-up, a drop in labor force participation and wages, and an increase in UI benefit take-up for those in our treatment group, relative to other drug users. However, while we estimate an increase in total health expenditures in our main specification, this estimate is statistically insignificant when comparing opioid users to other drug users, likely because other drug users have upward trending health expenditures over time that we cannot partial out using age fixed effects.

Finally, to narrow in on effects for individuals more definitively being opioid users prior to 2012, we use a different treatment group definition and rerun our estimates. In our baseline specification we consider all individuals with a history of OST or opioid-related health events as treated, even if they enroll in OST or suffer an overdose after the reform. This ensures that we capture opioid users even before they enter treatment, but a potential concern is that our estimates are biased toward zero because we may include individuals that do not currently use opioids. Above, we have shown that the probability of being an opioid user did not change after the reform (i.e., Figure A2). Additionally, in Table A3, we provide estimates for a sample that only uses pre-reform data to identify opioid users, and drop opioid users that enter OST or have an overdose post-reform. Our findings are not sensitive to this choice.

7. Mechanism and alternative explanations

We find that restricting flunitrazepam access causes some opioid users to substitute to less potent benzodiazepines and others to stop taking benzodiazepines altogether. This appears to lead to worse health outcomes and reductions in employment in this population. There a several plausible

explanations for these findings. For example, individuals may switch to other benzodiazepines or stopping benzodiazepines altogether, causing withdrawal symptoms that lead to health problems and make it more difficult to participate in the labor market. Or, instead of obtaining prescriptions, opioid users turn to the black market for flunitrazepam, other benzodiazepines, or opioids. Moreover, when flunitrazepam is unavailable, opioid users may substitute to other, more potent drugs. Below, we discuss these three potential mechanisms in turn.

7.1. Withdrawal symptoms likely play an important role

Benzodiazepine withdrawal is dangerous and associated with a variety of severe symptoms, including sleep disturbance, irritability, anxiety, panic attacks, hand tremor, sweating, difficulty concentrating, nausea, headache, and muscular pain (Pétursson, 1994). Compared to opioid withdrawal, benzodiazepine withdrawal is particularly prolonged and can persist up to 12 months (Higgitt, Lader, and Fonagy, 1985). Even after the most severe symptoms subside, it is possible that general discomfort explains the adverse health and labor market effects we observe as a result of the 2012 reform.

Measuring withdrawal symptoms is difficult because they are often managed by patients at home without seeking medical help. However, we can look for certain indicators in our data that point to withdrawal symptoms, including diagnosis codes for benzodiazepine-related withdrawal and prescriptions of non-benzodiazepine drugs that are believed to be effective in treating withdrawal symptoms.[10] In Figure 6, we plot effects on these indicators over time. Indeed, we see that signs of withdrawal symptoms increase after the reform and accumulate over time, likely reflecting the fact that less and less flunitrazepam is available on the legal and illegal markets as the reform takes effect. We interpret these trends as suggestive evidence that withdrawal symptoms are a key component in explaining our estimates.

[10]These drugs include certain anticonvulsants like pregabalin or topiramate, the so-called z-drugs (zolpidem, zopiclone, and zaleplon), the benzodiazepine antidote flumazenil, or buspirone, an antianxiety drug. ICD-10 diagnosis codes for benzodiazepine-related withdrawal are included in category F13.

7.2. Opioid users are unlikely to turn to the black market

A second plausible mechanism is that opioid users stop seeking prescriptions and turn to the black market for flunitrazepam or other benzodiazepines, consistent with recent evidence showing that more restrictive drug laws for opioid users can increase substitution to black market substitutes (Alpert, Powell, and Pacula, 2018; Mallatt, 2017). While we do not have individual-level crime data, we can look at aggregate arrest numbers as a measure of black market activity. We present trends in criminal charges around the 2012 reform in Figure 7.

We note three key findings. First, the black market for flunitrazepam virtually disappeared after 2012. While this is an equilibrium outcome, this trend is not consistent with a systematic shift of opioid users to the black market for flunitrazepam. Second, even before the reform, the black market for benzodiazepines consisted mostly of flunitrazepam, and charges for other benzodiazepines did not increase notably after 2012. Third, heroin-related criminal charges had already been on a downward trend before the reform and did not increase after 2012 either, indicating that there was also little substitution to other black market drugs.

7.3. There is little evidence for substitution to opioids, alcohol, or other drugs

Figure 7 suggests that black market activity does not change after restricting flunitrazepam—in particular, we do not see changes in heroin-related criminal charges. This is consistent with the patterns we see in our data. Figure A6 displays estimates for opioid-related hospitalizations before and after the reform.[11] We do not see significant changes in hospitalizations due to the reform, although we note that we cannot observe overdoses if they do not require an overnight hospital stay. In Figure A7, we plot effects on prescription opioid take-up, not including OST medications. If anything, opioid users are slightly less likely to be prescribed opioids, but none of the dynamic effect estimates are statistically different from zero. Taken together, these findings imply that opioid users are unlikely to systematically increase opioid consumption after the reform.

[11]This estimate is based on an interrupted time series regression without a control group, because the outcome for the control group is zero by construction.

We also estimate effects for indicators of alcohol or other non-opioid drug use in Figure A8. These may include alcohol poisoning, alcohol-related liver disease, or hospitalizations and rehabilitation treatments related to alcohol and another drugs. We do not find changes in any of these outcomes, suggesting that opioid users are not more likely to use alcohol or other drugs due to the 2012 reform, relative to non-opioid users.

Therefore, we do not find any evidence that restricting flunitrazepam access has led opioid users to substitute to heroin or other potent substances. However, we note that in our context, unlike in the U.S., opioid users are primarily in OST, so they are monitored daily and continue to receive opiates as the primary part of therapy. The major change in our setting is that individuals already in treatment faced inability to continue taking a different prescribed drug, potentially leading to withdrawal symptoms that make it more difficult to succeed and be productive in daily life. Our findings thus provide new insight on regulating controlled substances in an already highly regulated environment.

8. Discussion and conclusion

In this paper we study the effects of changes in prescription drug access for opioid users in treatment. A 2012 reform in Austria that severely restricted access to flunitrazepam, a drug commonly prescribed to opioid users, allows us to study the economic effects of changes of outpatient prescription drug treatment that potentially interferes with OST. Using linked individual-level data, we are able to first identify opioid users and then show that this reform had adverse health and economic consequences for opioid users, as compared to a random control group of non-opioid users. In particular, we find that restricting flunitrazepam leads to a 1.8 percentage point, or 45.5 percent reduction, in the probability of filling a flunitrazepam prescription and that opioid users are 28.1 percent more likely to switch to a short-acting benzodiazepine as a result. We then show that health expenditures also increase, driven by physician visits, potentially due to illness resulting from withdrawal symptoms or other related health conditions. Additionally, we show that antidepressant prescriptions increase by nearly 4 percent, giving support to the idea that

the 2012 reform also had adverse effects on mental health.

Looking at labor market outcomes, we show that restricting flunitrazepam leads to a 12.7 percent reduction in employment and a 13.1 percent increase in the probability of UI receipt. We provide evidence that these adverse health and labor market effects are not due to substitution to black markets for illegal drugs; we show that criminal charges for both heroin and other benzodiazepines fall slightly after the 2012 reform. Instead, there is some evidence that opioid users experience withdrawal symptoms after being forced off flunitrazepam. Therefore, we posit that abruptly taking away an effective treatment option for patients in OST yields large harmful economic effects. Reducing the regulation of prescription drugs in such a highly regulated setting could create large gains in total social welfare by encouraging continued employment and discouraging UI dependency.

Our findings have important implications for policy, with a caveat that implications from our estimates are largely context dependent. We note that Austria has universal healthcare, with widespread access to substance abuse treatment programs. Moreover, patients in OST are highly monitored and therapy focuses on longer-term treatment, rather than short-term arrangements. Thus, in countries with smaller social safety nets or without universal healthcare, like the U.S., regulating some controlled substances may have even larger consequences in the presence of a large black market and/or fewer labor market protections.

Finally, as outpatient prescribing continues to grow as a potential treatment avenue in the U.S. and in other OECD countries, it is important to understand the economic effects of prescribing benzodiazepines for opioid users. This is especially important in an era when one in ten opioid-related deaths is linked to benzodiazepine use (NIDA, 2023). Our findings indicate that prescribing and monitoring an effective combination of opiates and mental health drugs can have major effects on well-being for individuals seeking treatment.

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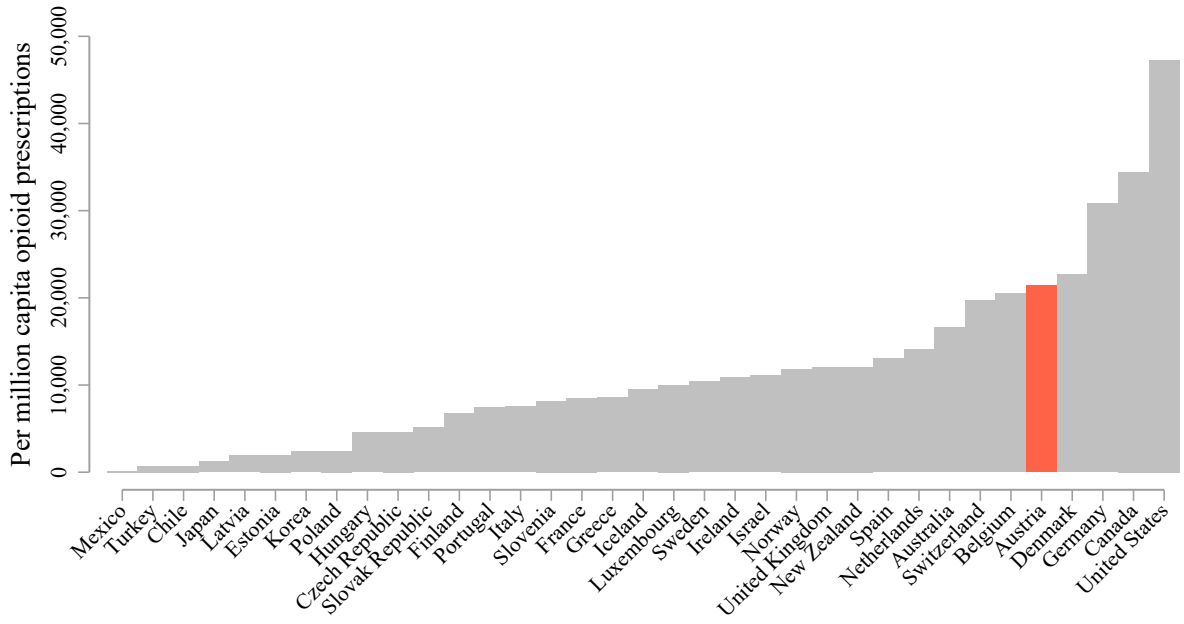
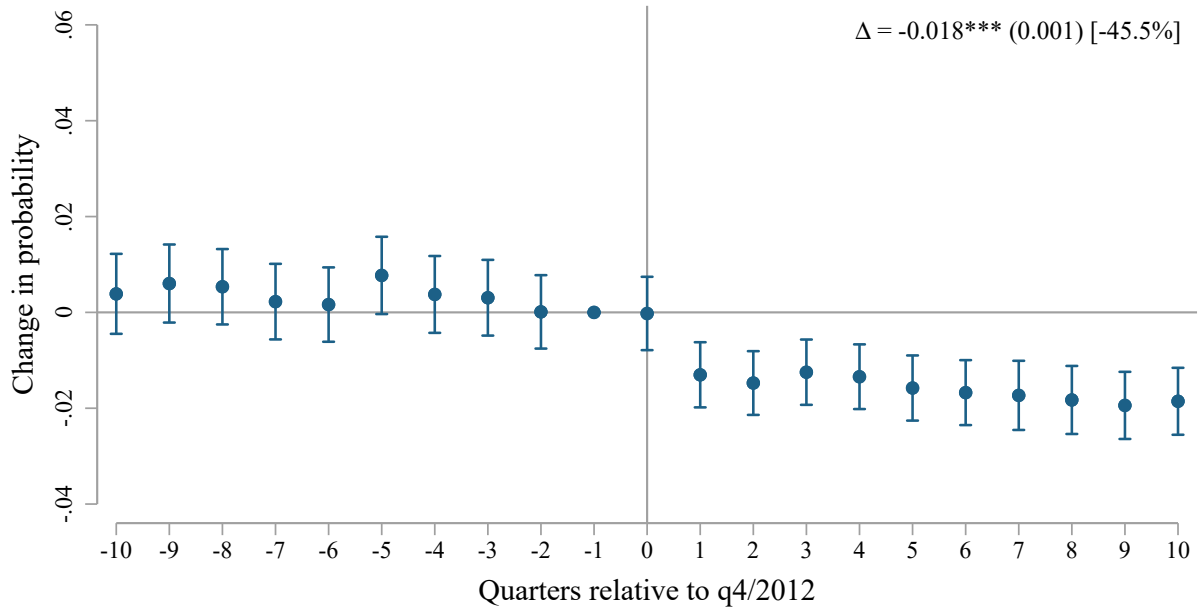
9. Figures and Tables

Figure 1 — Per capita prescribed opiates, by OECD country



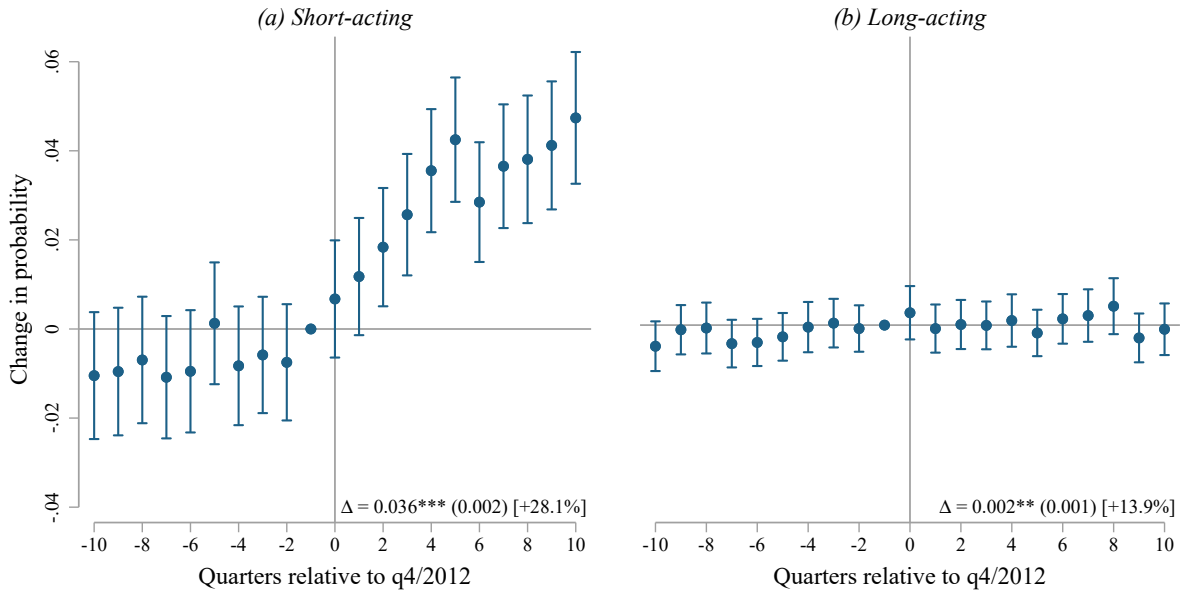
Notes: Austria ranks as the fifth-highest per capita opioid prescriber and is highlighted in orange. Source: OECD.

FIGURE 2 — Effects of the 2012 reform on flunitrazepam prescriptions



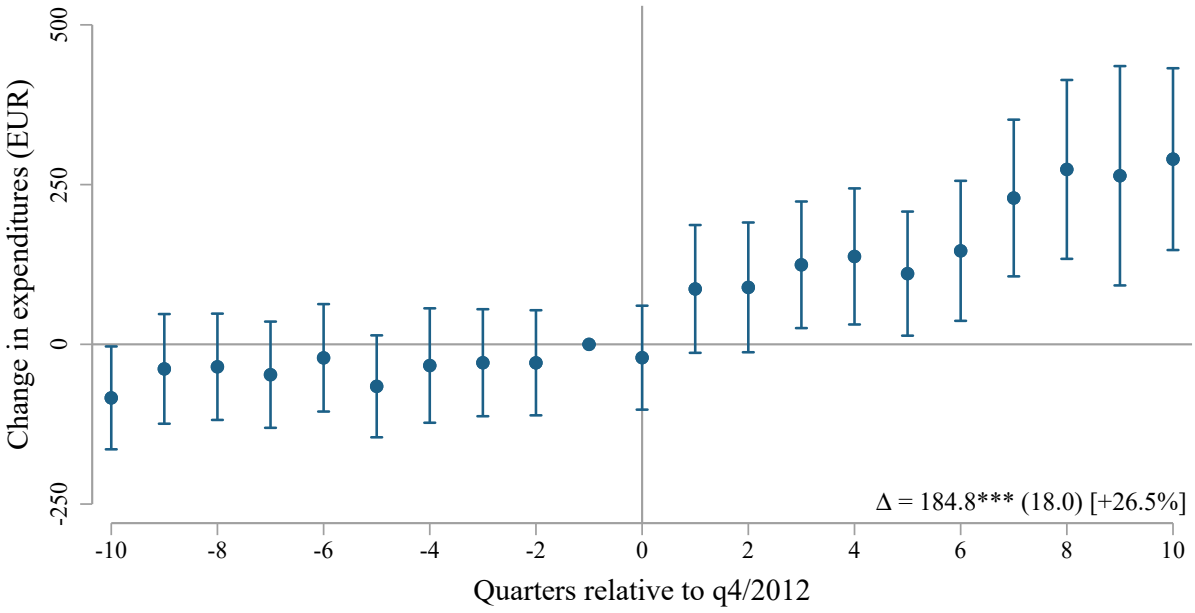
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. We calculate the percent change relative to the pre-reform rate of flunitrazepam prescriptions in the treatment group. Each scatter represents the difference in flunitrazepam prescription take-up between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE 3 — Effects of the 2012 reform on other benzodiazepine prescriptions



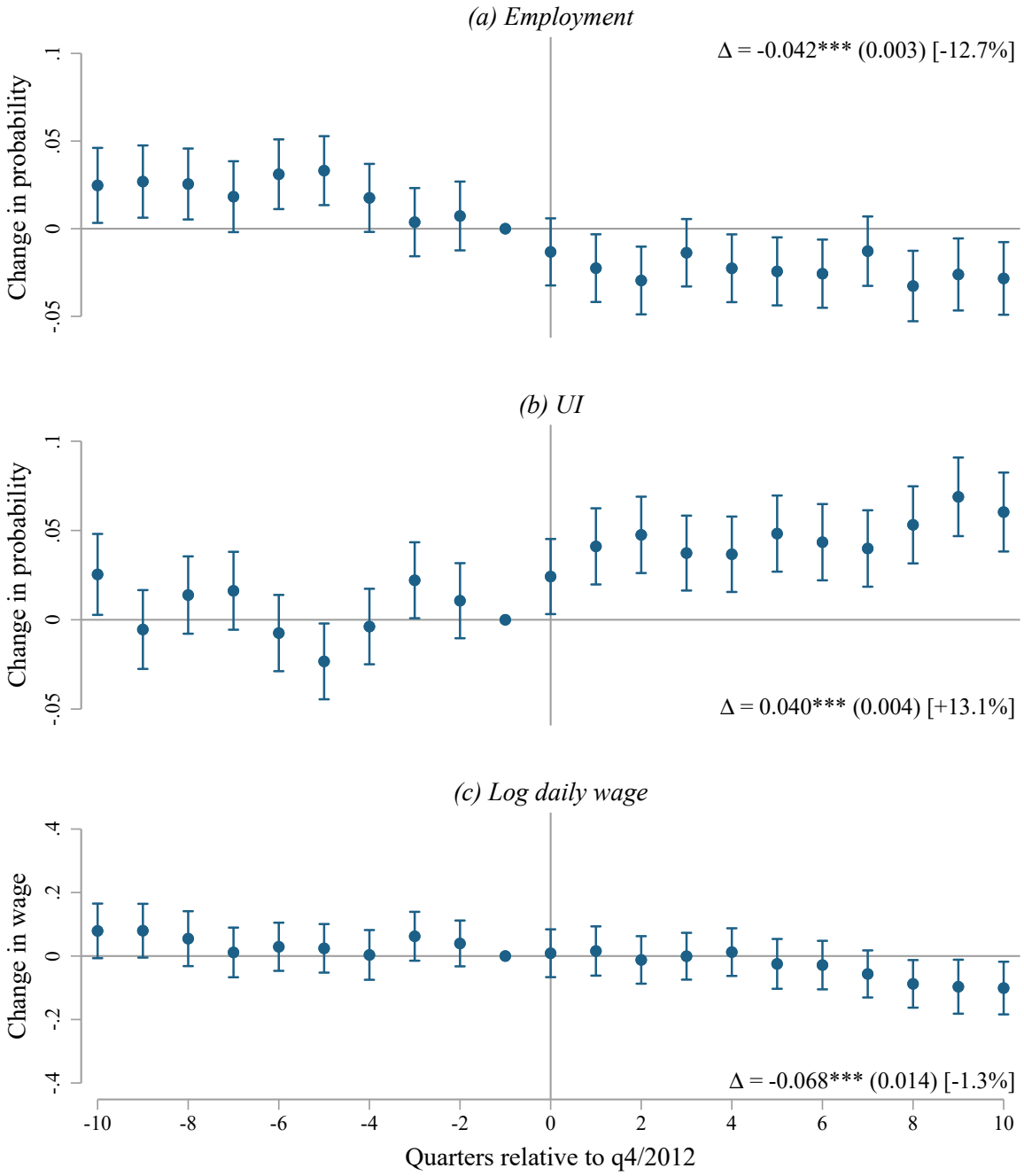
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). Each scatter represents the difference in potent (non-flunitrazepam) benzodiazepine prescription take-up between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. We display the calculated average effect and corresponding percent change estimate in the bottom right corner. N = 2,302,619

FIGURE 4 — Effects of the 2012 reform on total health expenditures



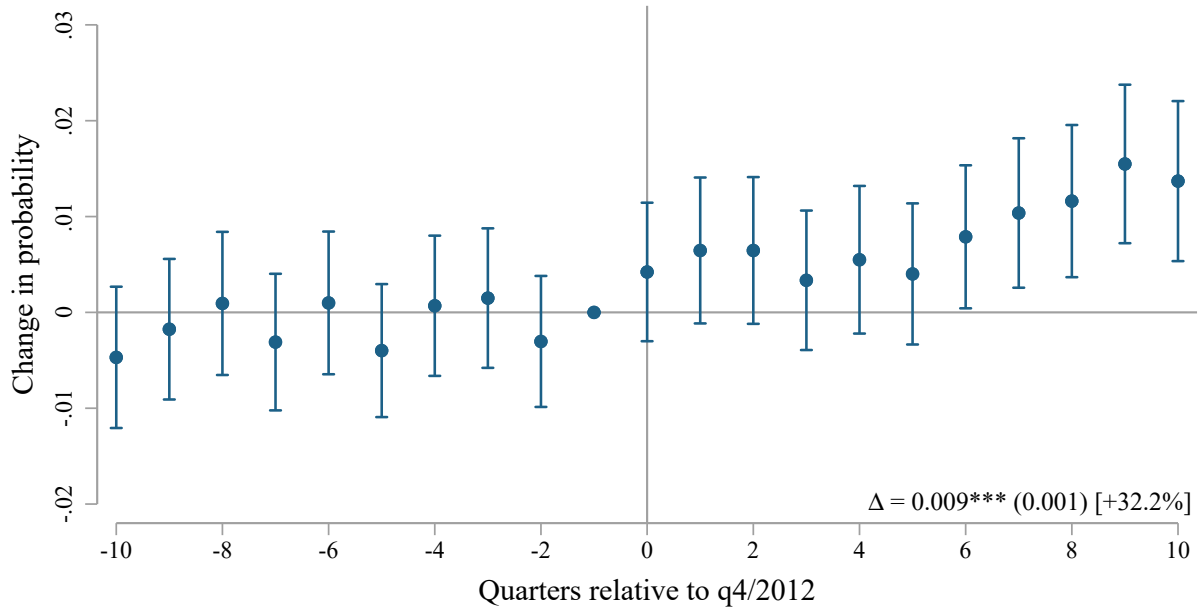
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). Each scatter represents the difference in healthcare expenditures between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. We display the calculated average effect and corresponding percent change estimate in the bottom right corner. N = 2,302,619

FIGURE 5 — Effect of the 2012 reform on labor market outcomes



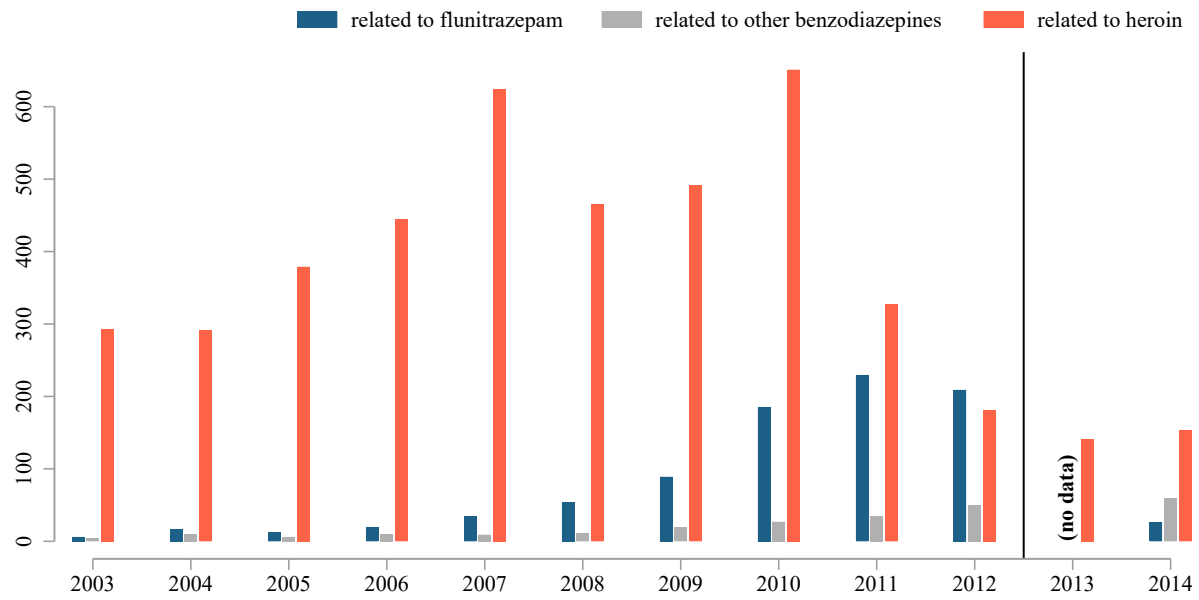
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. Data on labor market outcomes is from the Austrian Social Security Database. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the bottom right corner. Each scatter represents the difference in the probability of employment (top panel) and daily log wages (bottom panel) between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE 6 — Effects of the 2012 reform on indicators for benzodiazepine withdrawal



Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the bottom right corner. We calculate the percent change relative to the pre-reform rate of benzodiazepine withdrawal indicators in the treatment group. Each scatter represents the difference in withdrawal indicators between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE 7 — Drug-Related criminal charges over time, by drug type



Notes: Data on drug-related arrests are from the Federal Ministry Republic of Austria Drug-Related Crime Annual Reports from 2003–2014, available here: https://bundeskriminalamt.at/302/. The last year with complete data on benzodiazepine-related drug charges in 2014.

TABLE 1 — Descriptive Statistics

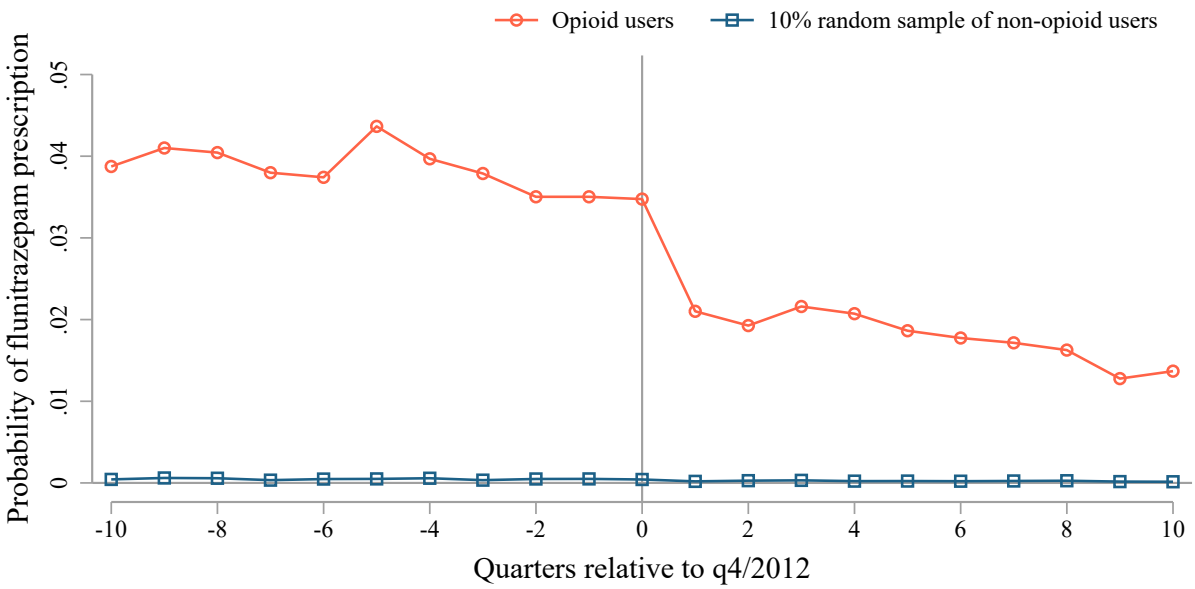
	General pop.	Opioid users
Female	0.50	0.28
Age	40.02	32.51
College degree	0.13	0.03
Migrant	0.35	0.40
Opioid substitution treatment		
In OST	0.00	0.37
Benzodiazepine prescriptions		
Flunitrazepam	0.00	0.04
Other short-acting benzo	0.00	0.13
Other long-acting benzo	0.00	0.02
Health status		
Total healthcare expenditures (EUR)	219.64	697.88
Mental health prescription	0.05	0.15
Labor market status		
Employed	0.54	0.33
Unemployed	0.03	0.16
Retired	0.12	0.05

Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. Data on labor market outcomes is from the Austrian Social Security Database. Columns 1 and 2 present quarterly means for the pre-reform period: 2010/Q2–2012/Q3. Column 1 displays means for a randomly chosen 10 percent of the general population (N = 999,105 individuals). Column 2 displays means for opioid users, as determined by whether an individual experiences an acute opioid-related event (hospitalization) or is enrolled in opioid substitution therapy (N = 3,829 individuals).

Web appendix

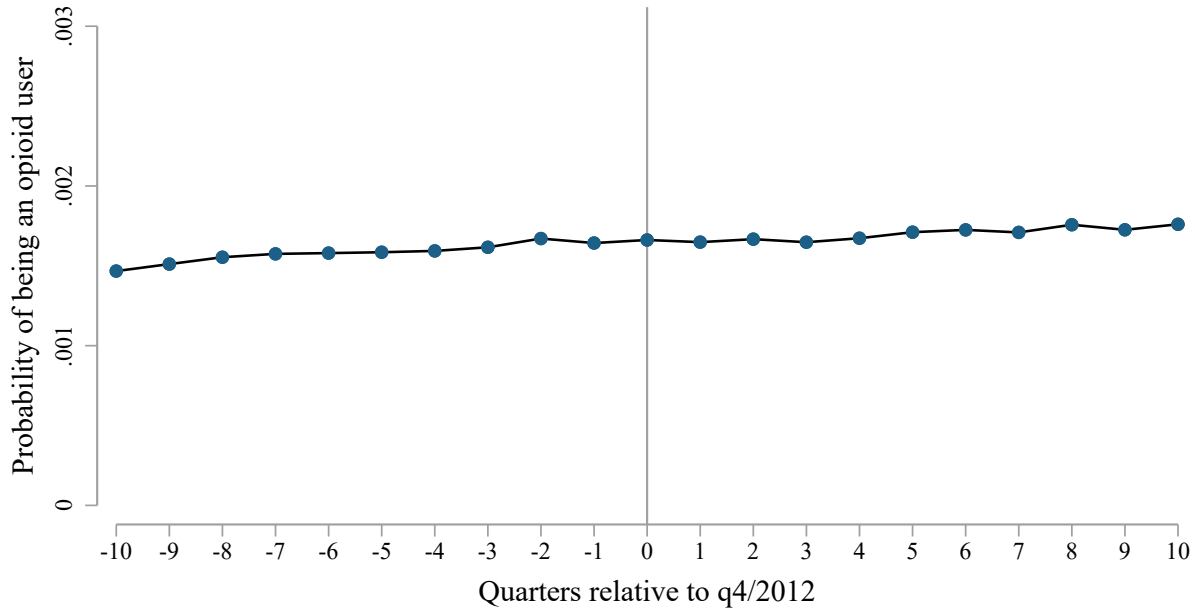
A. Additional Tables and Figures

Figure A1 — Flunitrazepam prescriptions, by treatment status



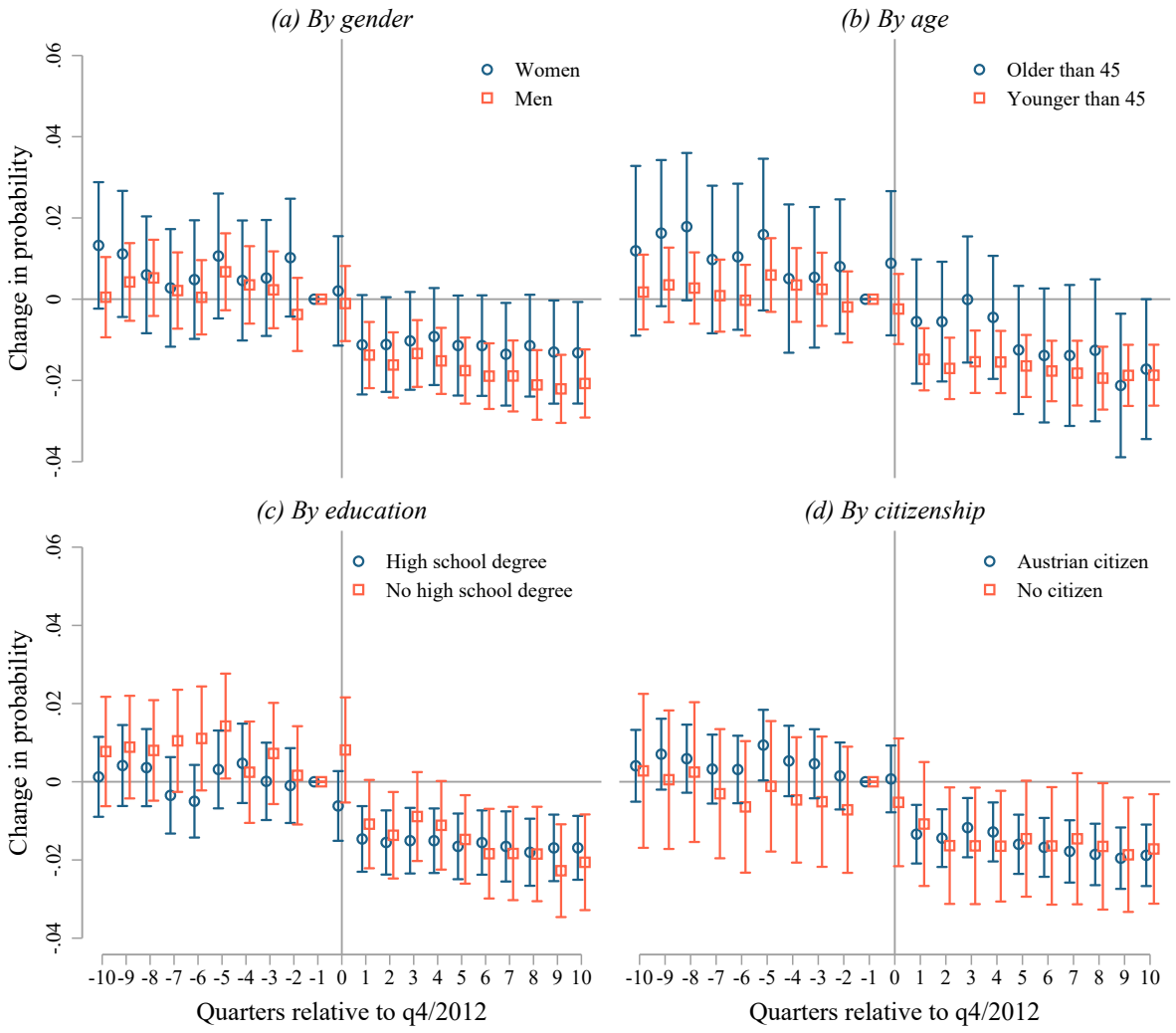
Notes: This graph shows flunitrazepam prescription in the treatment and control group. The orange circles mark the probability of opioid users (i.e., our treatment group) having a flunitrazepam prescription in a given quarter. The blue squares denote the probability of non-opioid users (i.e., our comparison group) having a flunitrazepam prescription in a given quarter. N = 2,302,619

FIGURE A2 — Selection into opioid use: Probability of being an opioid user over time



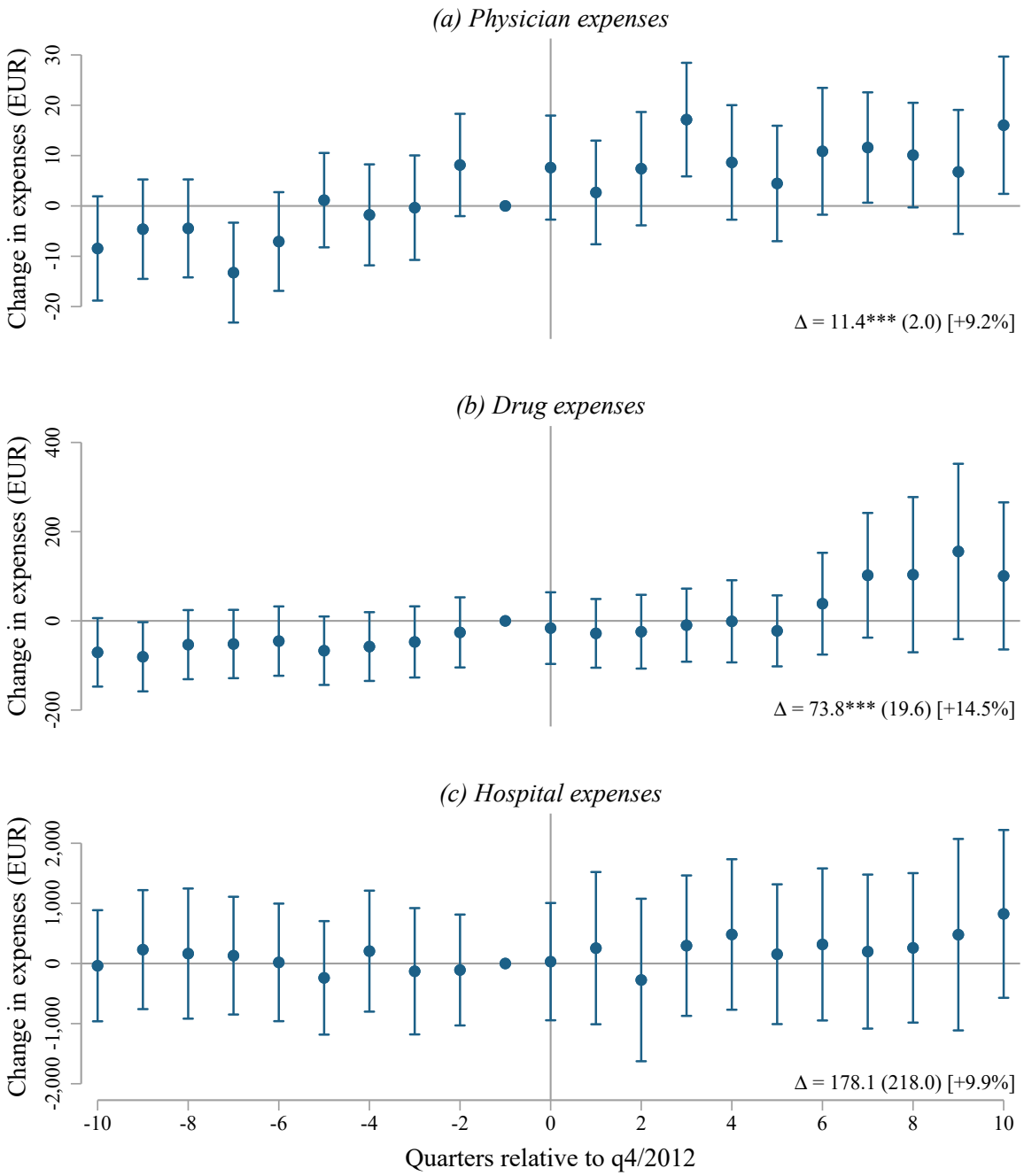
Notes: This figure plots the unconditional probability of being an opioid user 10 quarters before and after the reform that restricted flunitrazepam access in the full Upper Austrian population aged 15–65. N = 18,007,066.

FIGURE A3 — Heterogeneous effects of the 2012 reform on flunitrazepam prescription take-up



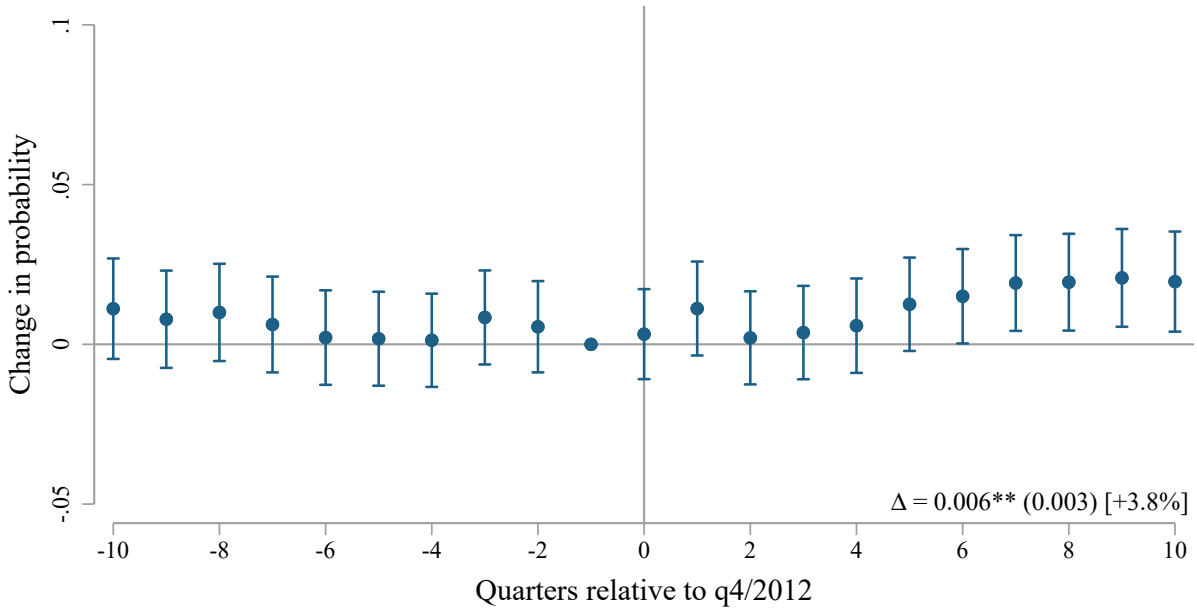
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. Each scatter represents the difference in flunitrazepam prescription take-up between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. Estimates are based on a balanced sample. N = 2,302,619

FIGURE A4 — Effect of the 2012 reform on physician, drug, and hospital expenses



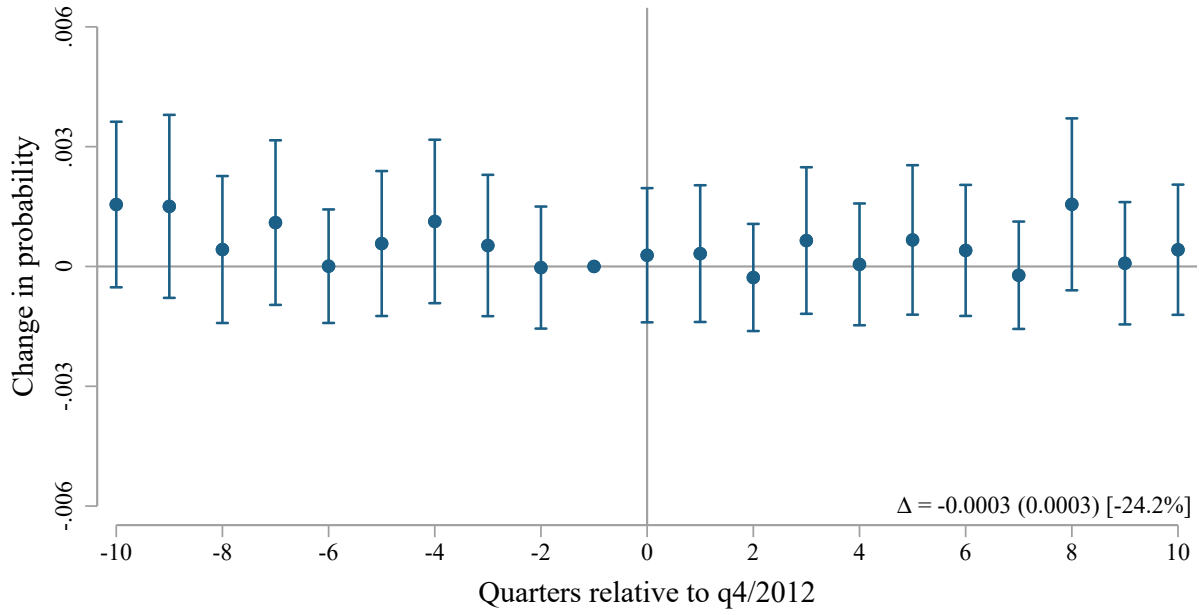
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. Each scatter represents the difference in overdoses for the listed condition between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE A5 — Effects of the 2012 reform on mental health prescriptions



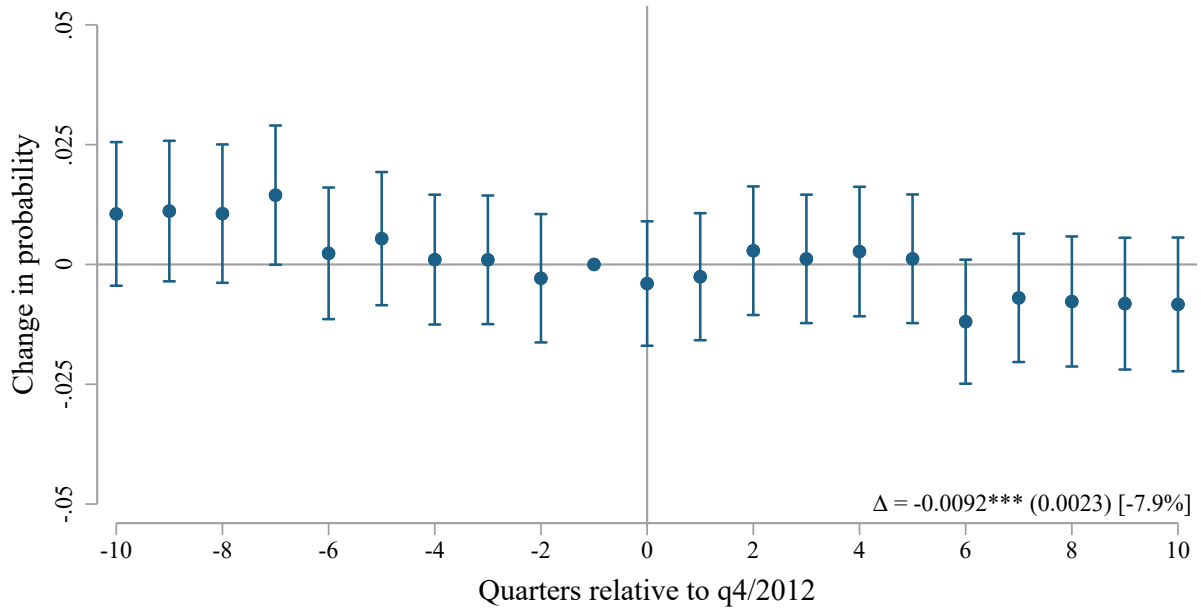
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). Each scatter represents the difference in antidepressant prescription take-up between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. We display the calculated average effect and corresponding percent change estimate in the top right corner. N = 2,302,619

FIGURE A6 — Effects of the 2012 reform on hospitalizations for opioid overdose



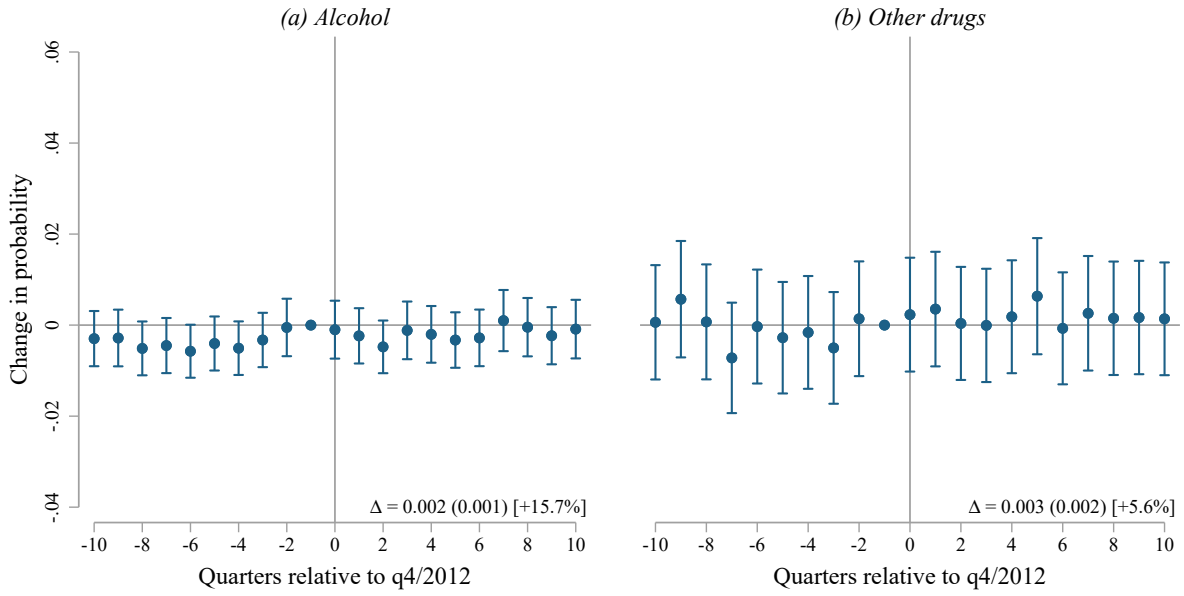
Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. Each scatter represents the difference in hospitalizations for opioid overdoses between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE A7 — Effects of the 2012 reform on prescription opioids



Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. Each scatter represents the difference in prescription opioid take-up between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

FIGURE A8 — Effect of the 2012 reform on indicators for alcohol and other non-opioid drug use



Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. This figure plots the coefficients $\hat{\beta}_j$ and their respective 95% confidence intervals from Equation (1). We display the calculated average effect and corresponding percent change estimate in the top right corner. Each scatter represents the difference in overdoses for the listed condition between opioid users and a random 10 percent draw of the general population for each quarter before and after the 2012 reform to restrict flunitrazepam. N = 2,302,619

TABLE A1 — Robustness to different regression specifications

	OLS (1)	+ Ind. FEs (2)	+ Age FEs (3)
Flunitrazepam prescriptions	−0.019***	−0.018***	−0.018***
	(0.001)	(0.001)	(0.001)
Other benzodiazepine prescriptions			
Short-acting	0.035***	0.037***	0.036***
	(0.003)	(0.002)	(0.002)
Long-acting	0.002*	0.002**	0.002**
	(0.001)	(0.001)	(0.001)
Health outcomes			
Total health expenditures	169.7***	165.0***	184.8***
	(15.1)	(17.8)	(18.0)
Labor market outcomes			
Employment	−0.011***	−0.018***	−0.042***
	(0.004)	(0.003)	(0.003)
UI	0.039***	0.043***	0.040***
	(0.003)	(0.004)	(0.004)
Log daily wage	−0.029**	0.001	−0.068***
	(0.013)	(0.015)	(0.014)
Opioid overdose	−0.0004*	−0.0003	−0.0003
	(0.0002)	(0.0003)	(0.0003)
Individual fixed effects	No	Yes	Yes
Age fixed effects	No	No	Yes

Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. Each difference-in-differences estimate presents separate average effects for the listed outcome, from analogues of Equation (1). Column 1 presents estimates without individual fixed effects or age fixed effects; Column 2 adds individual fixed effects; Column 3, presents estimates from our preferred approach, which includes both individual and age fixed effects. Robust standard errors are clustered at the individual level and are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A2 — Robustness to different control groups

	Baseline (1)	No control group (2)	Other drug users (3)
Flunitrazepam prescriptions	−0.018***	−0.008***	−0.015***
	(0.001)	(0.001)	(0.001)
Other benzodiazepine prescriptions			
Short-acting	0.036***	0.012***	0.021***
	(0.002)	(0.003)	(0.003)
Long-acting	0.002**	0.006***	0.002
	(0.001)	(0.001)	(0.002)
Health outcomes			
Total health expenditures	184.8***	130.5***	−5.7
	(18.0)	(21.4)	(33.8)
Labor market outcomes			
Employment	−0.042***	−0.015***	−0.016***
	(0.003)	(0.004)	(0.005)
UI	0.040***	0.035***	0.026***
	(0.004)	(0.004)	(0.005)
Log daily wage	−0.068***	0.052***	−0.044*
	(0.014)	(0.018)	(0.023)
Opioid overdose	−0.0003	−0.0001	−0.0003
	(0.0003)	(0.0004)	(0.0007)

Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. Data on labor market outcomes is from the Austrian Social Security Database. Each difference-in-differences estimate presents separate average effects for the listed outcome, from analogues of Equation (1). Column 1 presents estimates from our main approach, comparing opioid users to a random sample of non-users. Column 2 presents estimates for a time series approach using no comparison group for our main assigned treatment group (opioid users). Column 3 presents estimates from an approach that compares opioid users to other non-opioid drug users, as defined by pre-reform drug-related hospital expenditures. Robust standard errors are clustered at the individual level and are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A3 — Robustness to using only pre-reform data to identify opioid users

	Baseline	Only pre-reform opioid users
	(1)	(2)
Flunitrazepam prescriptions	–0.018***	–0.025***
	(0.001)	(0.002)
Other benzodiazepine prescriptions		
Short-acting	0.036***	0.038***
	(0.002)	(0.003)
Long-acting	0.002**	0.000
	(0.001)	(0.001)
Health outcomes		
Total health expenditures	184.8***	147.9***
	(18.0)	(23.8)
Labor market outcomes		
Employment	–0.042***	–0.016***
	(0.003)	(0.004)
UI	0.040***	0.011**
	(0.004)	(0.004)
Log daily wage	–0.068***	–0.161***
	(0.014)	(0.017)
Opioid overdose	–0.0003	–0.0007
	(0.0003)	(0.0004)

Notes: Data on health expenditures is from the Upper Austrian Health Insurance Fund spanning 1998–2018. Data on labor market outcomes is from the Austrian Social Security Database. Each difference-in-differences estimate presents separate average effects for the listed outcome, from analogues of Equation (1). Column 1 presents estimates from our main approach, comparing opioid users to a random sample of non-users. Column 2 presents estimates for a time series approach using no comparison group for our main assigned treatment group (opioid users). Column 3 presents estimates from an approach that compares opioid users to other non-opioid drug users, as defined by pre-reform drug-related hospital expenditures. Robust standard errors are clustered at the individual level and are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.