Initially, finding a relevant and well-defined topic for this analysis was more difficult than I expected. I wanted to apply the techniques we learned throughout this course, especially focusing on *difference-in-differences* methods.

At first, I struggled to find a meaningful topic and dataset, and honestly, I was a bit uncertain. Eventually, I changed my approach and explored academic literature, which led me to the topic I’ll present today: **the relationship between marijuana legalization and opioid-related deaths**.

To give you a brief introduction — as you may have seen in the news — the United States continues to struggle with a devastating opioid crisis, with unprecedented numbers of overdose deaths. At the same time, several states have begun legalizing marijuana. This raises an important question: **Can cannabis policy reform help reduce opioid-related harms?**

Before starting my analysis, I conducted a review of the existing literature. For example, some earlier studies — including one from Harvard University — found promising results, reporting up to a 25% decrease in opioid-related deaths after marijuana legalization. However, more recent studies have found no statistically significant differences in overdose trends.

These mixed findings show how complex the relationship is, and they highlight the need for further research. Understanding this link is crucial not only for U.S. drug policy and public health, but also for other countries like those in Europe, where similar issues are beginning to emerge. Learning from the U.S. experience could provide valuable insights for shaping more effective responses here too.

Our main dataset is the **provisional drug overdose death count**, which is publicly available from the **CDC**, the Centers for Disease Control and Prevention. It’s a **longitudinal panel dataset** that includes variables for each U.S. state and provides detailed data across multiple drug categories over time.

Since the dataset is structured longitudinally, we needed to **reshape it into panel format** and carry out several **data cleaning steps**. This included removing inconsistencies, addressing missing values, and ensuring overall data quality.

Now, I’d like to give you a brief overview of the drug categories included:

* **Total opioid deaths**: This includes all deaths involving opioids.
* **Synthetic opioids**: Such as **fentanyl** and its analogs, which are among the most dangerous contributors to the opioid crisis.
* **Cocaine**: A **non-opioid stimulant**, tracked separately.
* **Methadone**: A legal opioid used in **treatment programs** for opioid dependence.
* **Psychostimulants**: Including **methamphetamine** and **amphetamine**, also non-opioid stimulants.
* The dataset also uses **ICD codes** (International Classification of Diseases) to help identify **causes of death**, including **cases involving multiple substances**.

This level of detail allows for a more precise analysis of how different drug categories — especially opioids — respond to policy changes such as marijuana legalization.

One of the first key decisions we had to make was whether to focus on **medical** or **recreational marijuana legalization**. This choice was actually quite straightforward. Recreational use has a much broader population-level impact, while medical use is limited to a restricted set of patients with specific conditions, and usually requires prescriptions and formal approval.

Moreover, by the time our data starts — in **2016** — most U.S. states had already legalized medical marijuana. This makes it difficult to conduct any meaningful before-and-after comparison for medical use. On the other hand, **recreational use** was legalized more recently in a smaller number of states, making it a better candidate for a **difference-in-differences** analysis.

In fact, as of that time, slightly less than 50% of states had legalized recreational marijuana, while almost all had legalized medical use. So overall, focusing on recreational legalization was both more **empirically pratical** and **statistically meaningful** for our purposes.

Moving forward, we needed to choose a **treatment state** and a **control state**. Ideally, we would have included multiple states, but due to the difficulty of collecting and cleaning data — and the fact that I worked on this project alone — I limited the comparison to **one treated and one control state**.

I selected **Massachusetts** as the treatment group and **New Hampshire** as the control group. These two states are both located in the **Northeastern U.S.**, making them geographically comparable. Socioeconomically, they are relatively similar, though Massachusetts is more populous. Their healthcare systems are also comparable — particularly in terms of **public access to overdose-reversal drugs** like **Naloxone**.

For context, **Naloxone** is a life-saving medication used to reverse opioid overdoses. In some states, it requires a prescription; in others, it can be distributed freely. The availability of Naloxone is critical, because in an overdose situation, the person affected may not be in a condition to request or access it. So consistent access to Naloxone across both states helps ensure that differences in overdose rates are not driven by emergency treatment availability.

Massachusetts legalized recreational marijuana via a **referendum in November 2016**, though the first retail stores didn’t open until **late 2018**. Therefore, **our analysis period starts in 2018**, which allows us to capture the effect of the actual rollout.

New Hampshire, by contrast, has **not** legalized recreational use — it only permits medical marijuana — making it a reasonable control group.

Lastly, I want to note that in difference-in-differences analysis, having an appropriate **pre-treatment and post-treatment window** is crucial. For example, the Harvard study I mentioned earlier used **Colorado** as the treatment state, which legalized recreational marijuana back in **2014**. Their dataset included at least two years of pre- and post-treatment data. We aimed to follow the same logic by selecting a clear policy change point and collecting data on both sides of it.

Let’s now take a look at the data. I plotted the **monthly drug overdose death counts** for each state as a time series. In the plot, the **dashed lines** indicate two key events in Massachusetts: the date of the **recreational legalization referendum** and the date when the **first dispensaries opened**.

As you can see, the **parallel trends assumption** — which is crucial for difference-in-differences — doesn’t hold perfectly here. Also, notice the **scale difference**: the y-axis reveals that Massachusetts has about **five times more overdose deaths** than New Hampshire, highlighting a major discrepancy in magnitude.

A major challenge was that **New Hampshire had missing data**, especially in some drug categories. I initially planned to impute these gaps, but it turned out to be more complex than expected:

* **Methadone** data needed only minimal filling.
* **Psychostimulants** were reasonably completed using **backward fill**.
* But **heroin** data had substantial missing values, and basic imputation methods didn’t perform well.

I tested some **simple imputation techniques** like:

* **Polynomial interpolation (degree 2)** — but this yielded poor results.
* **Spline interpolation** — slightly better, but it doesn't support backward fill natively and, worse, it estimated **negative overdose values**, especially for heroin. Even after applying a clip at zero, the resulting plots showed inconsistency.

Ultimately, I used a **Kalman Filter** — a more robust technique for dynamic time series modeling, which also considers **seasonality**. The imputed results were acceptable overall, although we’ll later see some **side effects** this method introduced.

After imputing the drug variables, I moved on to **feature engineering**. I enriched the dataset by merging it with external sources to include key **socioeconomic covariates**, such as:

* Minimum wage
* Population
* GDP
* Unemployment rate

These variables were collected from official state websites. Their update frequency varied — **yearly, quarterly, or monthly**. Since our overdose data is monthly, I **aligned all variables** to a monthly format: for example, yearly data was repeated for each month of the year, and quarterly values were copied across the corresponding three-month blocks.

Additionally, while crawling the web, I found a **pro-cannabis website** that provided **monthly marijuana-related arrest data** for each state. I also manually scraped **political party websites** to gather data on **election outcomes and referenda**.

To match the electoral data to the time structure of our dataset, I **rounded referendum dates** to the nearest first day of the month — either the current or the next one. The election variables were considered valid **until the next election**.

Finally, I created a **COVID dummy variable**, set to 1 from **March 1st, 2020 to December 1st, 2021**, based on the timeline of the U.S. COVID-19 epidemic as reported by Google. The aim was to capture the pandemic's potential impact on overdose trends, although — as we’ll see later — this control alone was not sufficient.

At the end of the cleaning process, I applied standard **data normalization**:

* Overdose and covariate values were converted **per 100,000 people**, to ensure comparability between states of different population sizes.
* Political data (such as election outcomes) were expressed in **percentages**.

To check for multicollinearity, I started by computing a **correlation matrix**. I removed variables with a correlation coefficient above **0.9** to avoid redundancy and instability in the model estimates.

Next, I constructed the key variables for the **difference-in-differences (DiD)** analysis:

* treated: equals 1 for **Massachusetts** (the treated state), 0 for **New Hampshire** (the control).
* post: equals 1 for dates **after September 2018**, when the first recreational marijuana shops opened in Massachusetts, and 0 otherwise.
* treated × post: the **interaction term** that captures the **treatment effect** in the DiD framework.

The model I used was a **fixed effects DiD model**, with the following components:

* γᵢ (**state fixed effects**) control for time-invariant differences between states, such as demographics, long-term healthcare infrastructure, and cultural attitudes.
* λₜ (**time fixed effects**) account for nationwide shocks and trends affecting both states, such as the **COVID-19 pandemic** or the **fentanyl crisis**.
* Covariates include the **time-varying socioeconomic variables** discussed earlier (e.g., unemployment, minimum wage, arrests, etc.).

The coefficient of interest is **σ**, which estimates the **average change in overdose death rates in Massachusetts after legalization**, relative to New Hampshire, controlling for:

* Structural state differences (γᵢ)
* Common time shocks (λₜ)
* Time-varying covariates

A summary table outlines the theoretical interpretation of the DiD components under the **parallel trends assumption**, but let’s focus on the actual **empirical results**.

Among all drug categories, **cocaine** was the only substance with a **statistically significant result**. The DiD estimate for cocaine overdose deaths was **−3.3**, indicating a decline post-legalization.

On the other hand, most **opioid-related categories** — including heroin, synthetic opioids, and methadone — were **not statistically significant**, with coefficients close to zero or even slightly positive. Some of these results were likely influenced by **imputed values**, especially for heroin, which had many 0 entries. In fact, for heroin, the standard error dropped to nearly zero, suggesting **low reliability** and possible **lack of variance**.

One limitation of the standard DiD model is that it assumes a **constant treatment effect over time**. To address this, I implemented an **event study** approach, replacing the binary post variable with a set of **monthly leads and lags** — that is, dummy variables indicating how many months before or after legalization each observation occurred.

However, this led to **perfect multicollinearity**, so I had to remove one category (usually the baseline month) to estimate the model. The results showed a **downward trend in opioid deaths even before treatment**, suggesting a **violation of the parallel trends assumption**. However, there was also a **sharp drop in overdose deaths** shortly after legalization — though this effect faded within a year. This drop **coincided with the COVID-19 pandemic** and a **major fentanyl wave**, which were **not fully captured** by the fixed effects or covariates, indicating possible **omitted variable bias**.

Interestingly, the **cocaine** results from the event study were much more stable. The downward trend **only began after legalization**, reinforcing the idea of a genuine treatment effect.

As a final robustness check, I conducted a **placebo test**, simulating a fake legalization event in **late 2017**. This helped verify that the observed effects were not driven by **pre-existing trends** or **unobserved shocks**.

* For all opioid-related drugs, the **placebo effects were insignificant** and mixed in direction, confirming that the real estimates are not purely artifacts.
* For cocaine, the **true treatment effect remained negative and significant**, while the placebo was **neutral and non-significant**, highlighting the **robustness** of the cocaine finding.

Overall, the analysis suggests that **recreational marijuana legalization in Massachusetts had no clear impact on opioid-related mortality**, but was associated with a **significant reduction in cocaine-related overdose deaths**.

Although this final part wasn’t strictly required, I wanted to treat it as a **playground** to apply some of the more advanced methods we covered in the course. In this case, I switched from modeling the **level of opioid deaths** to modeling the **direction** of change.

So, I created a **binary outcome variable** called opioids\_up, which equals **1 if opioid-related deaths increased**, and **0 if they decreased** in a given month. This allowed me to explore **binary response models**, such as **Logit** and **Probit**, rather than linear regression.

I first applied a **Logit model**, with opioids\_up as the outcome. The model achieved a **pseudo-R² of 0.14**, which is reasonable for this type of classification task.

**Why not use standard R²?**  
 Traditional R² is based on minimizing squared residuals, which doesn’t apply in non-linear models like Logit. Instead, we use **pseudo-R²** (e.g., McFadden’s), which compares model likelihoods.

The **likelihood ratio test (LLR)** gave a **p-value < 0.001**, suggesting that the model overall is statistically significant.

However, most **marginal effects** of the covariates were **not statistically significant**, meaning that while the model classifies reasonably well, **individual variables** don’t have strong predictive power.

I also ran a **Probit model**, but the results were very similar. Since Logit had slightly better fit and interpretability in this context, I chose to present just the Logit results.

I then tested whether the effect of treated\_post on opioids\_up might be **biased due to endogeneity** — specifically, the possibility that a state legalizes marijuana **in response to rising opioid issues**, rather than independently of them.

To address this, I implemented a **Two-Stage IV model with Logit (2SRI)**:

I constructed an instrument equal to **1 only for Massachusetts after January 2018** — the start of the full recreational rollout. This instrument should:

* **Affect the likelihood** of being in the treated\_post group (**relevance**).
* **Not affect opioid deaths directly**, only through marijuana legalization (**exclusion restriction**, assumed).

I regressed treated\_post on the instrument and covariates. From this, I saved the fitted values treated\_post\_hat.

* This captures the **predicted probability** of treatment given only exogenous variation.
* I also calculated the **F-statistic** to check instrument strength.

I then ran a **Logit model** using the instrumented value treated\_post\_hat instead of the original treated\_post.

* This accounts for **endogeneity** by purging treatment assignment of any correlation with unobserved confounders.

The **F-statistic = 79.4**, indicating the instrument is strong.

After correcting for endogeneity:

* + treated\_post still has a **negative coefficient**, but it's **not statistically significant** (p = 0.264).
  + However, **some covariates** — like marijuana-related **arrests** and **Democratic vote share** — remain significant, suggesting they might independently explain some of the variation in overdose increases.

The **direction** of the effect supports our original finding (a reduction in risk), but we can’t claim a statistically significant effect once we control for endogeneity.

IV-2SLS (Linear Model for Levels)

* This model keeps opioid deaths in **levels**, rather than binary.
* R² = 0.16; F-stat = 130.9 — again, a very strong instrument.
* After instrumenting:
  + treated\_post remains negative, but again **not significant** (p = 0.241).
  + **Arrests** and **Democratic percentage** again show significant relationships.

**Conclusion**:

* These results suggest that the **original DiD estimates** are **not likely biased by reverse causality**, since instrumented and non-instrumented models give **similar results**.
* However, since the exclusion restriction is **assumed but not tested**, we still interpret these cautiously.

After the DiD and IV analysis, I also ran an **event study** — but this time with a **binary outcome**: whether opioid deaths rose or fell. This helps us explore how the effect **evolves over time**, rather than assuming it’s constant.

Initial Problem: Too Many Parameters

* I first used **monthly dummies** (e.g., one for each month before and after legalization). This meant estimating nearly **50 coefficients** — from **24 months before** to **24 months after** treatment.
* This caused **overfitting and numerical instability**, especially with a binary outcome in Logit.

Solution: Binning

To solve this, I **grouped time into broader intervals**, or **bins**.

* This reduced the number of dummy variables, while still capturing the overall **shape** of the treatment effect.
* The plot in the center shows:
  + Opioid deaths **spiked** right after treatment.
  + Then they **fell** significantly, before returning to baseline.
  + This pattern **coincides with external shocks** — such as the **COVID pandemic** and the **fentanyl wave** — which likely confound the treatment effect.
* While the **parallel trend assumption is violated**, the **post-treatment trend** shows a **sharp drop** that eventually fades.
* The **timing of these effects** aligns with known external events, showing that **binary event studies** can help disentangle **dynamic treatment effects**.
* This reinforces the importance of **controlling for time shocks**, and it supports our **robustness checks** from the IV models.

While the event study with Logit gave us some insights into how opioid death patterns evolved after legalization, the method **suffers from serious limitations** when applied to this small dataset.

1. **Strong Imbalance in Bin Classes**:
   1. Some time bins — especially around the legalization window — have very few or no observations in one class (e.g., all "opioid up" or all "opioid down").
   2. This leads to **sparse cells** and **unstable estimates**.
2. **Infinite Estimates**:
   1. In Logit models, when one category **perfectly separates the outcome**, the model tries to push coefficients toward infinity.
   2. This makes the model **numerically unstable**, and many coefficients lose any meaningful interpretation.
3. **Huge Standard Errors**:
   1. Some coefficients become very large or **nonsensical**, and **standard errors explode**.
   2. This results from **overfitting** and **high dimensionality** relative to the small sample size.

Why This Happened:

* **Sample size = 46 observations**
* **17 predictors**
* Extremely **unbalanced bins** (e.g., one month has only a handful of events)

The pseudo-R² of **0.46** and the confusion matrix look decent **at face value**, but they mask deeper issues. The predicted trend in the center plot shows **volatile swings** — some bins appear to drastically reduce opioid risk, while others increase it — but none of it is statistically robust due to the above issues.

Finally, we acknowledge that **omitted variables** could still bias our results. These are factors we **couldn’t control for** but that likely influence opioid mortality:

* **Access to treatment**: availability of clinics, methadone programs, Naloxone, etc.
* **Mental health data**: suicidality, depression rates, hospitalizations — hard to measure monthly.
* **Cultural factors**: religiosity, stigma, institutional trust — which may influence both drug use and policy support.
* **Legal opioid prescriptions**: we did not have full data on prescribed opioids during the period.
* **Local shocks**: such as news events, law changes, or economic disruptions that may affect behavior.

These variables represent **reducible error** — meaning future studies could incorporate them to **improve causal inference**.

While my core analysis focused on the U.S., the **opioid crisis is not an isolated American issue**. Although Europe currently has lower overdose rates, early signs suggest a **growing threat** — one that could intersect with **cannabis policy reforms**.

For example:

* In **Germany**, over **1,600 overdose deaths** were recorded.
* **Ireland** reported **314 deaths** in 2020.
* The **Netherlands**, a long-standing reference for liberal marijuana policy, still had **nearly 300 deaths**.

On average, the **EU overdose mortality rate** stands at **2.25 per 100,000 people**, and **74% of those deaths involve opioids**.

Moreover, in 2023 alone, **52 new psychoactive substances** were reported in Europe — a signal that the market is changing rapidly, possibly becoming **less predictable and more dangerous**.

These figures suggest that Europe should start **anticipating the kind of policy evaluation** the U.S. is already dealing with. Questions like:

* Can cannabis reform alleviate part of the opioid problem?
* Or could it introduce **unintended consequences** in markets already dealing with synthetic drugs?

The methods applied in my U.S. analysis — including **difference-in-differences**, **instrumental variables**, and **event studies** — could be adapted to **European contexts** as cannabis legalization expands.

Of course, future European studies would need to account for **different health systems**, **cultural norms**, and **policy baselines** — but the **causal framework** remains broadly applicable.

**"Why This Matters for Europe: Learning Before the Crisis Hits"**

To conclude, while my analysis focused on the U.S., I believe the **European context offers an exciting opportunity** for further investigation.

In Europe, we can:

1. **Perform comparative studies** — for example, contrasting countries like the Netherlands, which has long tolerated cannabis use, with others adopting stricter or newer approaches.
2. Use **dynamic econometric models**, such as event studies or panel-based fixed effects, to assess policy shifts over time with more nuance.
3. **Integrate more and better data** — including cultural indicators, access to treatment, or mental health trends — to improve our understanding of policy impacts.

Summary points

* The **legalization of marijuana** is not a silver bullet. But it can be a **component** in a broader public health strategy to reduce opioid-related harms.
* Our findings are **uncertain but directionally informative** — especially the result on **cocaine**, which showed a robust decline post-legalization.
* This suggests a **need for more targeted investigations**, particularly into **heterogeneous effects** across drug categories, populations, and time periods.

For those interested in digging deeper, here are a few **notable studies** that have examined the intersection of cannabis and opioid use, including:

* **Bachhuber et al. (2014)**, who were among the first to link marijuana laws to reduced opioid deaths.
* **Shover et al. (2019)**, who cautioned against assuming causality too quickly.
* **Bleyer et al. (2022)** and others, who have contributed newer clinical and policy evidence.

These works emphasize that **the evidence is still evolving** — and highlight the importance of **rigorous, transparent econometric analysis** like the one we attempted here.

In short, legalization may offer **promise**, but it must be **evaluated carefully** — with context, caution, and continuous data collection.

**Thank you for listening — I’m happy to take your questions.**